ENGLERA 20

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Report on botanical nomenclature – Saint Louis 1999

XVI International Botanical Congress, Saint Louis: Nomenclature Section, 26 to 30 July 1999

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PREFACE

This is the official report on the deliberations and decisions of the nine sessions held by the Nomenclature Section of the XVI International Botanical Congress on five consecutive days prior to the Congress proper. The Section meetings were hosted by the Missouri Botanical Garden in Saint Louis during what may well have been that summer’s hottest week, with noon temperatures regularly approaching and sometimes exceeding 40°C. The air-conditioned lecture hall of the Ridgeway Center was an agreeable setting under these circumstances. Technical facilities included full tape recording of all comments that were spoken into the microphone (Burdet’s firm yet kindly chairmanship did much to discipline the audience in this respect, the heat of the debates notwithstanding), and screen projection from the Recorder’s laptop on which the texts of all proposals had been stored and where suggested amendments could be keyed in as the need arose. The Section had the pleasure and honour of being welcomed by the Congress President, Peter Raven, who, although not enrolled as a Section member, took part in the opening session. The Secretary General of the Congress, Peter Hoch, took a personal interest in the participants’ welfare and was omnipresent behind the scenes, ever helpful in spite of his countless other duties. Generous staff support and efficient voluntary aid made the complex business of getting smoothly through the agenda a pleasurable task.

A preliminary report of the Section meetings was published soon after the Congress (see Taxon 48: 771-784, 1999). It includes a tabulation of the preliminary mail vote on the published proposals, specifying how the Section acted on each and detailing amendments and new proposals approved upon motions from the floor. It also includes the full report of the Nominating Committee as well as the Congress resolution ratifying the Section’s decisions, neither reproduced here. The main result of the Section’s deliberations is the new Saint Louis Code, which was published as Regnum vegetabile, vol. 138, on 7 June 2000 – the quickest Code to be published since Alphonse de Candolle’s 1867 Lois.

The present full Report of the Saint Louis Section debates conveys, we believe, a true and lively picture of that memorable (as always) event. It is essentially based on the tape records, which in case of doubt were supplemented by two sets of documents: the texts submitted by many
speakers on the apposite, numbered sheets that were made available to
them, and two parallel series of notes, by the Rapporteur and the Record-
er, on the sequence of speakers and on actions taken. With these sources
combined, with all motions and voting results double-checked through
the sound track and the notes taken, we are confident that the record
published hereunder is accurate and complete. There is one (fortunately
minor) exception to that rule: three segments of the tape records of the
second session, on Monday afternoon, were accidentally erased and re-
placed by duplicates of other portions of the record. For these segments
(10 speakers in all, commenting on the proposed New Principle, and on
Art. 8 Prop. A and Prop. B) the only base for reconstructing what has been
said were the speakers’ own notes on the corresponding numbered sheets.

Before it was cast into its present, final form, this Report went
through a succession of phases. During the six weeks following the
Congress, Mrs Rosemarie Ziegler, secretary at the former IAPT Secre-
tariat, typed out the tape records into a first, rough transcript. On 17
September her text went by electronic mail to the three who had volun-
teered to undertake its first refinement: McNeill was in charge of Ses-
sions 1-2 (Monday), Hawksworth of Sessions 3-6 (Tuesday and Wednes-
day), and Barrie of Sessions 7-9 (Thursday and Friday). Each had the
Ziegler text as well as the tapes and numbered sheets for his sessions
available. The result of their efforts, an intelligible and coherent if still
raw text, was returned to Berlin between 9 November and 31 December,
and redistributed batchwise to the Editorial Committee members be-
tween 25 November and 1 January. Together with the first draft of the
future Code, this was the basic working document on which the Editorial
Committee based itself when meeting toward the end of January to pre-
pare the Saint Louis Code. Work on the Report had to rest until the Code
was ready for printing, and was then resumed by Greuter in the second
half of April. The present version, reflecting the effort to achieve a
coherent style, was based on a comparison of the previous drafts with
the tapes and written notes. It led to a rearrangement of some portions,
in such a way that the Proceedings now reflect the sequence of the
relevant provisions in the Code even when the order of the debates may
at times have differed. Deviations from the chronology of events are
indicated in the text by italicised, bracketed notes. All four authors took
an equal share in proof-reading the final version of the text.

As in the case of previous nomenclature reports, which the present
one faithfully follows in style and general layout, the spoken comments
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Preface

had of course to be condensed and partly reworded, sometimes rather drastically. For this reason, indirect speech has been used consistently, with a very few exceptions in which portions of direct speech have been retained, between quotation marks. The single major insert in direct quotation (almost but not totally unedited) is Raven's address of welcome. Additions by others (mostly due to the authors of this report) are placed between square brackets: they include explanatory or rectifying notes, records of reactions of the audience (to illustrate the sessions' emotional background), and reports on procedural actions, unless they form a paragraph of their own. As in previous Reports, the index to speakers has been integrated with the list of registered Section members.

The Section in Saint Louis was the largest ever, which was particularly noticeable as the previous one, in Yokohama, had been one of the smallest. In Saint Louis the Section had 297 members (+213%), also representing 231 institutions (+56%) holding 494 institutional votes (+37%), so that the total voting power present was 791 (+73.5%). As the nine card votes aptly document, participation was remarkably high, ranging between 74% and 90%. Not unexpectedly, the registration issue was one of two with a 90% score— but participation in the alternative vote on Gandhi’s motion on Art. 53 was subliminally higher, when the score was so narrow that a change of three votes would have tilted the balance in the opposite direction.

In geographical composition, the Section was far from being evenly balanced (it hardly ever is). Predictably, the Saint Louis Section was dominated by members from the Americas (62%) and from predominantly anglophone countries (two-thirds came from the United States plus Canada, Britain, Australia or New Zealand). Almost one half of the membership (147) were U.S. residents, and 10% (29) came from the U.K. No other single country was represented by more than 10 delegates, but 40 (13.5%) came from mainland Europe. The high Latin American presence (32, or 11%) was a salient, novel feature, but on the negative side, the low African representation (5, or less than 2%) must be noted. Such distortions are to some degree unavoidable and are usually compensated, at least in part, by a better balanced representation of institutional votes. An unprecedented and rather troublesome phenomenon was an active lobbying for pre-established positions, on and behind the scene, which, unlike the usual voting instructions carried by delegates, often appeared to suppress proper discussion of relevant issues, preventing their being acted upon in full cognisance of the pertinent arguments. To
many, this added an element of unpleasantness to the normal tension surrounding important, controversial issues and, conversely, deprived them of their customary intellectual thrill.

The three of us who were responsible for preparing the Nomenclature Report for the Tokyo Congress wrote what they believed was a “safe prediction” into its Preface: “The Tokyo Congress will not be remembered for its failure to adopt the proposals for stabilising 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 ... It has provided a challenge for those who 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.” It is tempting, and perhaps realistic, to turn all of their foregoing statements into reverse while replacing “Tokyo” with “Saint Louis”. But then, they also wrote: “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.” So we will not press the point.

Let us rather conclude by thanking the Botanic Garden and Botanical Museum Berlin-Dahlem for having again accepted to include the Nomenclature Report in their serial Englera, and Mrs Ziegler for the skilful and speedy way in which she took care of the initial transcription of the tape records. Our thanks also go to the International Association for Plant Taxonomy and its new Secretary, Tod Stuessy in Vienna, for contributing to the printing cost and taking care of the free distribution of copies of the Report to all registered Section Members.

Werner Greuter, John McNeill, David L. Hawksworth & Fred R. Barrie

Note: The figures given in parenthesis for each proposal, in the Report, correspond to the result of the preliminary mail ballot (Yes : No : Editorial Committee : Special Committee).
SIXTEENTH INTERNATIONAL BOTANICAL CONGRESS
SAINT LOUIS 1999

NOMENCLATURE SECTION

Bureau of Nomenclature

President: H. M. Burdet
Vice-Presidents: R. K. Brummitt, T. V. Egorova, K. Iwatsuki,
R. P. Korf, J. McNeill
Rapporteur-général: W. Greuter
Vice-Rapporteur: D. L. Hawksworth
Recorder: F. R. Barrie

FIRST SESSION

Monday, 26 July 1999, 09:25-12:50

Burdet opened the session by inviting the President of the XVI International Botanical Congress to address the Section.

Raven welcomed the Section and its members as follows: “I first want to express on behalf of my colleagues and myself, of the people of Saint Louis and the United States of America, Canada, and Mexico, what a great pleasure it is to have you here for the Congress and in particular for the nomenclature sessions. My first Nomenclature Section was forty years ago in 1959 in Montreal when I was a graduate student. A couple of highlights I remember from that meeting. I was walking along with my major professor, Harlan Lewis, and Dave Keck said to him: ‘Why, Harlan, I see you are following along with Peter. Trying to meet some important people?’ – and I was only about 22 years old at that time. I also remember how Lewis Wheeler addressed the audience on some matter, and Red Camp said in a stage whisper that one could hear all over the room: ‘I have known Lewis Wheeler for 30 years and that’s the only intelligent thing I have ever heard him say’. We are of course much more refined now and would not engage in that kind of remark,
but as the week wears on we may well get up to that level of discourse which is so characteristic of the *scientia amabilis*.

“256 people are pre-registered for the Nomenclature Section and about 300 are expected, compared to 95 in Yokohama, 157 in Berlin and 152 in Sydney. This shows that there is a great, flourishing interest in nomenclature. 218 proposals have been made, 47 of which concern changes to Art. 60 dealing with orthography. It shows how desperately interested in orthography you all are (Is that the science of handwriting? I just can’t remember.) This compares to 321 proposals in Yokohama, 336 in Berlin, and 213 in Sydney. So the last two Congresses have seen many more nomenclature proposals than this one, and hopefully the numbers might be settling down.

“I would like to thank at the beginning of these sessions the Bureau of Nomenclature, the authors of proposals, and all those who worked hard to prepare the materials for this week’s discussions. It is not easy to get these matters in the right form at the right time, and doing so indicates a high level of faith, and interest, and willingness to participate in the whole system. I would also like to thank the members of the Special Committees that were established at the Tokyo Congress to investigate and report on specific problems, as well as the members of the Permanent Committees who reviewed the proposals affecting their particular areas of jurisdiction. Special thanks are due to Dan Nicolson and John McNeill who helped us with invaluable advice on the organisation and management of nomenclature sessions, over the past few years.

“Let me quote from Alphonse de Candolle’s introduction to his 1867 Laws on nomenclature, which started the kind of process in which we are involved here: ‘There will come a time when all the plant forms in existence will have been described, when herbaria will contain indubitable material of them, when botanists will have made, unmade, often remade, raised or lowered and above all modified several hundred thousand taxa ranging from classes to simple varieties, and when synonyms will have become much more numerous than accepted taxa. Then science will have need of some great renovation of its formulae. This nomenclature which we now strive to improve will then appear like an old scaffolding laboriously patched together and surrounded by the debris of rejected parts. The edifice of science will have been built but the rubbish incidental to its construction not cleared away. Then perhaps there will rise something wholly different from Linnaean nomenclature, something so designed to give certain and definite names to certain and
definite taxa. That is the secret of the future, a future still very far off. In the meantime let us perfect the binomial system introduced by Linnaeus, let us try to adapt it better to the continual necessary changes in science, drive out small abuses, the little negligences, and if possible come to agreement on controversial points. Thus we shall prepare the way for the better progress of taxonomy.' I am glad to see that what Candolle said in 1867 is highly relevant today. It has provided occupation for thousands of people in the intervening years, and we may still take his remarks pretty well at face value.

"George Lawrence – a participant in the nomenclature sessions forty years ago, the first that I attended – wrote in his *Taxonomy of vascular plants*: 'The progress of the development of rules of nomenclature is represented, with one exception [the American Code], by a direct lineal growth based on trial, error and correction; it was the development from the Paris [Lois] to the Stockholm Code'. It is obvious that the same process of growth, experimentation, and correction has continued right down to the present day. This is something of which we can all be very proud.

"The Tokyo Code, in its introduction, states: 'Ultimately, however, plant nomenclature is not governed by a bureaucracy of committees but, in an open and democratic manner, by the community of its users represented by the enrolled members of International Botanical Congresses. The user-driven process by which plant nomenclature is regulated is of utmost importance for a Code which having no teeth in the way of penalties for infringements, entirely depends on user consensus for its universal application and implementation.' This is a key point of nomenclature. When we set up codes of nomenclature and at successive meetings keep improving and revising them, we are basically putting out a system of naming plants that has no teeth, no penalties, no down side; a system that simply depends on the willingness of all dealing with plants and their names, to accept the decisions made by a majority in order to the perfect system so that it may go on into the future. The proposed changes to the Code are now before the botanical community for discussion and debate. Those who wished to do so could vote on them in the preliminary mail ballot, whose results will be made known here. You who have come here will have the ability to engage in what I am sure will be very fruitful discussion, then exercise your right to vote proposals up or down. As always, one of the major benefits of these meeting is to gain an understanding of one another, of the diverse points of view that botanists hold in different parts of the world as they deal
with the rules and principles of naming plants. Many points of view are unique. Many specialists in nomenclature and in taxonomy (not excluding myself) have felt at some point that they had the single inspiration which every reasonable being would understand and accept without delay. But the real importance of these sessions is that all listen to one another, that they recognise and respect the others’ points of view and thereby gain a broader appreciation of plant systematics and of the whole range of different attitudes held by colleagues around the world. As is said so wisely in the Preface to the *Tokyo Code*, it is only by the willing acceptance that results from understanding, that the system of laws we are now going to discuss and vote will then govern our activities.

“Let me then finish where I began, by saying how very welcome you are, welcome in North America that is hosting the International Botanical Congress for the first time in thirty years, after the 1969 Congress in Seattle. I hope that you will fully enjoy this week in Saint Louis.”

**Burdet** thanked Raven for his warm welcome and inspiring address. Even though there were only 215 proposals to act upon, experience showed that even single proposals could give rise to lengthy arguments. The results of the preliminary mail ballot on the proposals, prepared by Fred Barrie and prefaced by Barrie’s comments, had been made available to all present.

**Barrie** added that the mail vote report had a few typographic errors that needed to be corrected by hand, and which he detailed. [See *Taxon* 48: 777-782. 1999, for a corrected version of the tabulation.]

**Burdet** went on to present the Bureau of the Section, including the five Vice-Presidents co-opted by the previously appointed members of the Bureau: Richard Brummitt, Tatjana Egorova, Richard Korf, Kunio Iwatsuki, and John McNeill. The Nomenclature Section would later on elect the Nomenclature Committees that work between Congresses, and a Nominating Committee had been appointed to prepare slates for approval by the Section. The Nominating Committee would be chaired by Dan H. Nicolson and have as members William G. Chaloner, Patricia Dávila, Gerrit Davidse, Knut Faegri, Walter Gams, Ching-I Peng, Gideon Smith, and Judy West. The chairpersons and secretaries of the present Permanent Committees were asked to provide the Nominating Committee, by Wednesday afternoon, with nominations of Committee members to serve from 1999 until 2005. The same deadline of Wednesday afternoon applied for the submission of those Committee reports that had not yet been received.
By 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 as defeated by the chair, unless someone moved its reconsideration from the floor. He moved, from the chair, that the Section confirm the traditional cut-off point of 75% “no” votes. [The motion was carried.]

In the past, parliamentary procedure had been followed, permitting any one person to move from the floor that a heavily defeated proposal or an entirely new proposal be considered, which if supported by a second person must then be discussed. As by a second motion from the chair, whereas it would still be possible for a single Section member to introduce such a motion, this would then require not a single seconder but support by five additional Section members in order to be discussed. [That motion, too, was carried.]

Barrie outlined the session schedule. Morning sessions would begin at 9 a.m. and run until 12, followed by a break for lunch of 2 hours. Afternoon sessions were to start at 2 p.m. and run until 6. There would be coffee breaks of 20-25 minutes at 10 a.m. and at 4 p.m.

Burdet introduced the three tellers appointed by the Bureau: Gordon McPherson, Eckhard von Raab-Straube, and Lucia Lohmann.

Greuter commented on the Section’s procedures. As there had never been a nomenclature session with so many in attendance, presumably many people present had never before been at such a meeting. He still remembered that the first time he was at a Congress, many years before, much of what was going on was rather strange to him. Fortunately, there were also many old hands in the room. The oldest in experience if not in years, Knut Faegri, was to be congratulated on his 90th birthday a few weeks before. [Applause.] These old-timers could provide experience and advice, when needed. Discussions would take more time with such a large number of people in attendance, as all would sooner or later feel the urge of having their say. Would they please try to be concise. In spite of the relatively low number of proposals, which did not mean low weight, the available time would be needed. The Chair or Rapporteur invariably had to resort to the threat of a night session, even though none had so far materialised. Would those who asked to speak please wait for the microphone before speaking (despite slight problems of accessibility to the central ranks for those handing around the microphones), then first announce themselves by name and city of residence.
The discussions were being taped, and it was essential to know who had said what. Speakers would receive a numbered sheet of paper and were asked to write down what they believed they had been saying – sometimes an amusing contrast to their spoken word, and quite helpful for editing the proceedings. (Of course, if one said the exact contrary of what one meant and wrote down, and subsequent discussion followed on what had been heard, the former would appear in the proceedings, so watch your words.) Please would those intending to speak much sit close to the front. Writing space for taking notes was the same in the front and back: on one’s knees – perhaps a local inconvenience. As a compensation, the room had air conditioning and other more modern facilities, e.g., screen projection of important items, reducing the need to consult papers.

The Section usually followed a kind of parliamentary procedure. When someone made a motion, the chair would ask whether it was seconded, and only if it was would discussion be opened. There was a definite sequence in which competing motions were handled, and there were expert people in attendance who would advise in case of doubt. Such well-structured procedures might seem a bit strange to those not yet familiar with them.

Decisions were taken by vote, normally by a show of hands. If the picture was sufficiently clear, the chair would rule that the proposal was accepted or rejected, as the case might be. When the chair, or anyone in the audience, was not satisfied – especially as this kind of poll did not show the institutional voting powers – there might at first be a call for a show of cards, when all were asked to hold up their card or cards, and the tellers would try and count the votes as accurately as possible. If the result was very tight or inconclusive, or anyway if someone asked for it, there would be a formal card vote. Boxes would be carried around, one marked “yes” and the other “no”. The appropriate numbered tag of the voting card, to be announced for each vote, was then put into the appropriate box. Tags number 6 and 9, being too easily confused, would not be used.

A most important point concerned the majority. There was nothing in the Code requiring any other majority than 50% of the votes cast. It was, however, a tradition of very long standing to require a 60% positive majority of votes cast for any proposal to modify the Code to pass, so as to ensure that changes to the Code be generally accepted by its users. [As nobody wanted to discuss the point, a motion to that effect (requiring that any decision to modify the Code would need a 60%
majority of the votes cast to pass) was invited, made, seconded, and carried.

Greuter went on to clarify that the 60% majority rule did not apply to administrative or procedural decisions that did not affect the provisions of the Code. There had also been a tradition, that when there were two competing, alternative proposals the Chair could rule that the change be first voted on in principle, by a 60% majority, and when the change per se had been so adopted the choice between the alternatives would be by a simple majority. This option was not precluded by the motion just carried.

An important routine remained, that the Section should ratify the Tokyo Code, as published, as the basis for its debates. The Tokyo Code was based on the decisions taken in Yokohama, but as a printed document the Code itself had not yet received official approval. This was always done by the next subsequent Congress. [Ratification was presented as a formal motion, which was seconded and carried.]

Greuter then introduced another, equally traditional motion, “that for the revised Code to arise out of this Congress, the Editorial Committee [yet to be appointed] 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 place, 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”. This motion, also by past tradition, used to elicit a comment from a person now sadly absent for the second time, Art Cronquist: “that the section also asked the Editorial Committee to interpret this empowerment in such a way that the changes be minimal and do not reflect the view of individual members of the Committee” – a comment that the Editorial Committee had always borne in mind. [The motion was seconded and carried.]

Barrie explained that a computerised projection system was being used, so that proposals could be put up on the screen for all to see. Most proposals would fit on the screen but the longer ones would have to be scrolled through. This was on a computer and thus [he said, tongue in cheek] would work flawlessly and hopefully not give too much trouble. Proposed amendments to the original texts could be keyed in on the spot: the original wording of the proposal and the modified version, with the change appearing in a different colour, could then be shown in parallel.
Nomenclature in Saint Louis

Burdet acknowledged the fact that a second person present had had his 90th birthday: Rogers McVaugh. [Applause.] He then read out the text of a motion submitted by Voss, bearing on voting procedures: “that all card votes be by secret ballot, i.e., no separate ‘yes’ and ‘no’ collection baskets but the individual cards to be marked by the card holder with ‘yes’ or ‘no’ (or equivalent, even + or −) and placed in a common collection basket.” [The motion was seconded.]

Chaloner feared that the proposed secret ballot on card votes would greatly slow down voting.

Burdet agreed that it would complicate the work of the tellers.

Barrie expected that it would slow down the process considerably, because if the “yes” and “no” votes were in separate boxes they could be counted immediately; if they were mixed together, they would have to be sorted first, which could add 50-100 % to the time of counting the ballots.

Voss, the motion having been seconded, explained the idea of a secret ballot. It eliminated voting by example. Otherwise some people might feel compelled to follow others without really thinking about the issue. A secret ballot would ensure that all could vote as their conscience dictated and would not feel compelled to follow someone else’s example.

Dorr, who had counted votes in Yokohama, concurred with Barrie. Often the count had to be done in haste, and votes in separate boxes were much easier to count. Not always was a card vote required, which was time-consuming and should be reserved for especially contentious or especially close issues. He had no worry that anyone’s vote would be observed by others. Normally the boxes were passed around too quickly for people to follow the herd, as it were.

Gilbert felt that it would be faster to pass around single boxes than pairs of boxes.

Barrie observed that as everyone was crowded together it would be difficult to observe into which box others were putting their ballot, except for the very next person.

Voss’s motion was defeated.

Nicolson, for the benefit of those members who had not been at a Section meeting before, introduced the persons seated at the front table: David Hawksworth, Hervé Burdet, Werner Greuter, and Fred Barrie. He then announced the Thursday afternoon programme: at 3 p.m. the IAPT General Assembly would be held, followed by Committee
meetings. During the time of the Assembly there would be Garden tours laid on for non-members of the IAPT, for them to enjoy the weather. [Laughter; outside it was around 40°C.]

He then told a story that he had got from Bob Ross in a letter describing his first Congress, the Stockholm Congress in 1950. Ross wrote: “I enjoyed the Nomenclature meetings far more than my own speciality during the Congress proper, because the passions that were on view were so exciting and interesting.” Ross had particularly mentioned Hochreutiner, who was from Switzerland. When some points came up that Hochreutiner was particularly concerned about, he would stand up and deliver an impassioned speech in English, and he would follow it by an equally impassioned and exactly identical speech in German, and then in French. Ross had been very impressed that Hochreutiner could get his thoughts together and express them so beautifully.

Greuter had been asked to make two more announcements. One related to the question of institutional vote delegations. It was no secret who was carrying which institutional vote. Whereas it was not possible technically to display these, as the list was changing continually, those interested were welcome to enquire at the registration desk about institutional delegates. A second request came from some of the Latin American members, happily so numerous at this meeting, and their point was very well taken. Would everyone please speak slowly and clearly, because many persons in the room did not have the mastery of the English language that others did. In particular, colloquialisms and rapid speech should be avoided; a concentrated presentation of facts would easily make up for time loss due to slow speaking. [Applause.]

Burdet ruled that the proposals would, as a general rule, be examined in the order in which they were printed in the Synopsis.

General Proposals

Prop. A (192 : 17 : 0 : 12).

Greuter explained that this was an enabling proposal, authorising the General Committee to renew the mandate of the Special Committee on Electronic Publishing. The overwhelmingly positive mail vote reflected the fact that the time was not yet mature for solutions to be found and decisions to be taken concerning the use of new publication media. The present Committee had recognised this and had been very cautious in its recommendations. There was a need to follow up on this, and it was a
good suggestion that a new Committee be appointed, to watch future developments and formulate any proposals that might seem useful by the time of the next Congress.

**Zander**, the Secretary of the Special Committee concerned, referred to the report that Karen Wilson and he had published in *Taxon* on behalf of the Committee. There was an obvious need for electronic publication of new names in the future, and provisions to that effect would have to be considered seriously by the next Congress. Hopefully, by 2005 the required infrastructure would be in place. Botanists were conservative right now, but others were not. He understood that the next edition of the zoological *Code* would allow publication of new names on CD-ROM disks. He appealed to God to help them, as there were major problems ahead with such media as to long-term accessibility.

**Prop. A** was accepted.

**Korf** asked whether the proposal had been for a new Committee or for maintenance of the existing one.

**Greuter** explained that the mandate of the individual members of the Committee had now come to an end, but the mandate of the Committee as such was renewed. Those presently on the Committee might be asked to serve again, but other people might also be asked to serve, at the discretion of the General Committee and after consultation with the present Secretary.

**Prop. B-L** (general discussion).

**Greuter** explained that Prop. B headed a whole series of proposals, all by the Vice-Rapporteur and all on a single main subject: nomenclatural terminology. He suggested that members might first speak on the whole proposal series (B to L) in general, and only later on the individual proposals which might be felt to have, and in his opinion did have, quite different merits – as also reflected by the different scores in the mail vote. These proposals were placed in a two-fold context. First, there was the Committee on Harmonisation, a Special Committee authorised by the Nomenclature Section in Yokohama. Second was the *BioCode* Committee, resulting from an initiative triggered by bodies outside the Nomenclature Section, the International Union of Biological Sciences (IUBS) and the International Congresses for Systematic and Evolutionary Biology (ICSEB), in an attempt to harmonise the rules between botany and zoology. Drafts of a new *BioCode* had been published both
in zoological and botanical taxonomic journals, including *Taxon*, and had raised controversial reactions – which was as should be, because a draft was there to raise controversy. There was no proposal before the Section to approve the *BioCode* as such, which would have been inappropriate for several reasons – one being that zoologists, who were busy with drafting their own new *Code*, soon to be published, had not seriously started discussing the drafts, and there was no point for botanists to set off on their own. Another obvious reason was, that there was still major, justified concern about many aspects of the present draft. Even the question of whether such a future unified biological *Code* was useful and worthwhile at all was still open and might be answered differently by different Section members. It was therefore not useful to enter a *BioCode* discussion at this time, although if time permitted at the end of the sessions there might be a wish to have a discussion on it. What was now before the Section were a few selected aspects that had turned up in the *BioCode* draft. Some of these items, such as the present ones, were genuine endeavours to unify some aspects of biological nomenclature; others were aspects that had just chanced to crop up during the editing of the *BioCode*. Several people present had been involved in the process of producing this first draft, and none of them had meant to impose anything on anyone, although others had misunderstood this. The author of the present proposals, who would certainly want to speak on them in more detail, was Hawksworth, the Chairman of the Committee on Bionomenclature and also the Secretary of the Committee on Harmonisation.

Hawksworth explained that the International Committee on Bionomenclature was set up under the auspices of IUBS and the International Union of Microbiological Societies (IUMS). When it had first met, its members soon became aware that they often failed to understand each other. So, very early on in the Committee’s debates, they came to agree on a series of nomenclatural terms that were unambiguous under the different *Codes*. After thorough discussion, a slate of such suggested terms was published to be debated more widely, and had found general approval. These terms had already been adopted in the cultivated plant *Code* and in the Italian translation of the botanical *Code*. The bacteriologists seemed happy to go along and would be debating this issue at their Congress the following month in Sydney. All who were involved teaching nomenclature across groups knew of the great difficulty of doing so at present without putting off aspiring biological students. Regardless of the *BioCode* issue, adoption of a set of unified terms
would help communication across biology, at a time when biology was increasingly taught as a single discipline. The 1998 IUBS General Assembly in Taipei had felt very strongly that this particular issue of terminology should be put to the relevant nomenclatural bodies as soon as possible.

W. Anderson spoke to the general issue of the BioCode. Orchard, he and some others had already published their opinions on the matter. The summary was that they did not think that harmonisation was a good thing. It was a bad thing, a way for botanists to lose control of their nomenclature. Harmonisation should be extirpated, pulled up by the roots, destroyed. These proposals were a subtle way to push botanists in a direction they did not want to go. The proper analogy was cancer: if one had a few cancer cells in one’s body, did one want to live with them or get rid of them? The answer was, one wanted to wipe out cancer before it got established. The Section should vote against all proposals that were steps towards the BioCode, and also, for the same reason, against continuing the Committee on Harmonisation. [Applause.]

Demoulin disagreed. The Section should not come to an abrupt general conclusion as to the merits of the whole set of proposals, as W. Anderson had suggested, but look at them individually. The Rapporteur was right when saying that some proposals had merit. No term was bad just because it was used by zoologists or bacteriologists. There might be notions more clearly defined in other Codes than in ours. If the botanical Code could be made clearer, as, for example, by restricting “priority” to “precedence by date”, then this should be approved. In other cases, perhaps, there needed to be some kind of political bargaining with the other groups, and so the proposals might better be rejected now and left for a later stage in the process. But if a proposal was a definite improvement in making our Code clearer, it should be accepted. [Some applause.]

McNeill echoed what Demoulin had said. It was incredible that in this age of integration of biology, over environmental and other issues, botanists could take the archaic position expressed by W. Anderson. Instead, they should be looking to a future in which they could simplify nomenclature and follow procedures that could be communicated readily to everybody. In the Taxacom electronic discussion forum, confusion had resulted from the divergent terms used by zoologists and botanists, for example, validity versus correctness of names, demonstrating the seriousness of the problem. It was not, however, timely to adopt all these proposals, because, as Demoulin had implied, some were matters
for agreement between zoologists and botanists. Still there were proposals, such as the one on priority and precedence, or homotypic and heterotypic versus nomenclatural and taxonomic synonyms, that might usefully be adopted now.

Bhattacharyya said that taxonomists in India, generally, did not support the BioCode. The botanical type method was not identical with zoological or bacteriological methods. The established botanical procedures and phraseology had been tested in time and were well known to botanists in general. Many botanists had no opportunity to attend the present Congress, but still had a strong interest in the Code. However, some of the proposed changes in wording were helpful for a better understanding of the present Code, and these he would accept.

Zijlstra spoke on harmonisation, not on the BioCode. Harmonisation threatened to become a goal in itself – but for how many would it be really useful? There were great differences among plant taxonomists, some did know much of the Code but many others did not. For specialists in nomenclature, harmonisation was unnecessary. For her work for the Index nominum genericorum she had to use the zoological Code and had found that it was not so difficult to understand as was being said. For the majority of plant taxonomists harmonisation was not so important but only confusing. They already had problems in understanding the meaning of valid, legitimate etc. Why should a second set of terms be created for them to understand and use? Only when terms were already in use, e.g., homotypic and heterotypic, was it a good thing to have them in the Code. [Applause.]

Nicolson sensed a general agreement that the Section did not want to approve all proposals concerning harmonisation, but wanted to support Demoulin, McNeill and others who felt that there was a baby in the bathwater that we should try to filter out before emptying the tub. [Applause.]

Keil suggested that the proposals be considered in terms of their being improvements to the botanical Code. Those that were appropriate, even in the absence of the proposed BioCode, one should accept. Those that would simply make our Code more compatible with the BioCode, one should reject. [Applause.]

Burdet noted that, while applause was in itself sympathetic, it was unusual at nomenclature sessions, where it was just a waste of time.

Trehane introduced himself as the Rapporteur for the International Code of nomenclature for cultivated plants, which the Section members
Nomenclature in Saint Louis
doubtless all knew and loved. That *Code*, in its last edition in 1995, had
accepted much of the new terminology that was being proposed, and he
had had very little bad press on this. The new terms had facilitated
communication between people who were working on cultivated plant
taxonomy with the users of their names, removing some of the mystic,
arcane aspects of botanical terminology. When discussed individually,
each proposal should be judged on its own merits.

Funk called attention to the results of the mail ballot. The Section,
when voting on these proposals, needed to be especially sensitive to the
opinion of the rank and file members of IAPT.

D. Ward asked whether it was appropriate to use the phrase “call the
question”, this being a request to the chair to terminate discussion and
begin the voting procedure.

Greuter, now that the Section had had a general discussion on the
whole set of proposals (B-L), took this to be a good breaking point. It
was time for taking up the proposals one by one.


Greuter noted the heavy (70 %) negative mail vote. The proposal,
which he did not personally find very attractive, would replace the term
“effectively published” by “published”. It resulted from a trade-off with
zoologists, who used “published” where botanists had “effectively pub-
lished”. The latter in a way was more precise, and botanists might even
lose information by accepting this change.

Prop. B was rejected.


Hawksworth noted that this had already been referred to as adding
precision to what was currently in the *Code*. The present use of “prior-
ity” for, e.g., date was rather confusing. The proposal would confer a
very precise meaning to priority.

Greuter added that the issue had come up when botanists and zoolo-
gists compared their respective *Codes*. The use of “priority” in the bo-
tanical *Code* was imprecise and confusing because it referred to different
situations. The botanists present had then gone through their *Code*
and found that it was very easy to improve it by making a distinction
between what priority really means, i.e., precedence by date, and the
other meanings that “priority” had in the *Code*, which were, just the
date, or just precedence. When two names that were synonyms and had the same date were combined, the Code stated that the one that was first adopted in preference to the other took “priority”; in reality, it did not take priority, as the date remained unaltered, but did take precedence. Also, the Code had “priority of a name” when it meant the date of the name, and again confusion was introduced. The proposal would empower the Editorial Committee to go through the Code, leaving the term “priority” where it indeed meant priority but replacing it by either date or precedence in the other places.

W. Anderson had been trying to use the Code for many years, and this was one area in which he had never found it to be confusing. At the risk of introducing American slang, he quoted the saying: “If it ain’t broke, don’t fix it.”

McVaugh agreed with W. Anderson. He had been using the Code since before W. Anderson was born, and he had never found anybody who had any problem in understanding what priority meant. It meant of course precedence by date, but it also meant precedence by some other method of restriction.

McNeill pointed out that whereas McVaugh was perfectly correct in saying that the use of “priority” in the Code was generally clear, the Code was in fact using the term in a way that differed from its dictionary definition, for precedence of all sorts, whether it had to do with date or not. For example, botanists referred to a conserved name as having priority over an earlier name against which it was conserved, when in fact what they really meant was that it had precedence despite the rejected name’s priority of publication. Empowering the Editorial Committee to make this clarification was appropriate.

Prop. C was rejected.


Hawksworth supposed that the Section would not want to support Prop. D as it was the counterpart of Prop. B, just rejected, but to him it would make a very valid clarification. Words like “effectively” and “validly”, used in connection with “published”, were difficult concepts to get across to students. Obviously, if one was experienced in the field one would become familiar with these terms, but if one was trying to get new people on board, by using these concepts sadly one was putting barriers all along the way.
Greuter, during his many years of experience as an editor, had again and again been facing, and fighting, and weeding out, abuse of the term “valid” in a sense that was contrary to the botanical Code. He had been fighting a losing battle, as habit was too strong. Just as did zoologists, botanical authors kept referring to a “valid species”, or a “valid name”, when they meant an accepted species or a correct name. For this reason another, unambiguous term was needed to for what botanical nomenclaturalists, but not even all botanists, knew as “validly published”.

Keil raised the same objection to “established” or “establishment”, that Greuter had against “validly published” and “valid publication”. Someone talking about the “established name” of a plant might mean simply the name currently used by most people.

W. Anderson agreed with Greuter that this was a problem, and not an easy one to get around. It was not just a matter of words, it was a matter of understanding the difference between taxonomy and nomenclature. The terms “establishment” and “established” were not going to solve it. They would just add a third set of terms to confuse people: a step backward, not an improvement.

Prop. D was rejected.

Prop. E (46: 171: 6: 2) was ruled as rejected.

Prop. F (50: 166: 7: 2).

Greuter did not have the same misgivings concerning the term “correct” as he had for valid. At this stage, this proposal was not necessary.

Prop. F was rejected.

Prop. G (48: 168: 7: 2) was ruled as rejected.

Prop. H (45: 171: 7: 2) was ruled as rejected.


Greuter noted that both proposals had received a positive mail vote of c. 60 %, quite out of the pattern of the related proposals. They would be discussed jointly.

Hawksworth added that the proposed terms [homotypic and heterotypic] did show straight away what was meant, the concepts being implicit in the words. Somebody coming from the outside would not necessarily appreciate this; a more experienced taxonomist would.
Voss moved an amendment: that, instead of the proposed substitution, the Editorial Committee be authorised to add “homotypic” [and “heterotypic”] in parentheses after “nomenclatural” [and “taxonomic”], to show what the latter terms meant. Upon request, he clarified that his motion was not to apply to all places where “nomenclatural” [or “taxonomic”] now appeared, but only where the terms were defined. The main term would remain “nomenclatural” [or “taxonomic”]. His motion being seconded, he then phrased his amendment as follows: “Where ‘nomenclatural’ [or ‘taxonomic’] is defined, authorise the Editorial Committee to add ‘homotypic’ [or ‘heterotypic’] in parentheses”. He could not tell where the main definition sat in the Code (neither could Greuter, who had asked the question).

Greuter liked the idea of mentioning the equivalence in the Code, favoured using the more evocative terms “homotypic” and “heterotypic” in other places, if there were any, apart from the definition proper, and he so moved. Firstly these terms told the reader immediately what was meant, and secondly, they were already widespread in botanical literature and familiar to all. Their use would ease access to nomenclatural debates to newcomers. Would Voss accept this alternative motion?

Voss maintained his preference for “nomenclatural” and “taxonomic”.

Greuter, to gain time, suggested the following procedure: assuming that original proposals were withdrawn [as they were], the Section could first vote on whether or not to introduce “heterotypic” and “homotypic”, requiring a 60 % majority; and then, by a simple majority, decide whether to give preference to “heterotypic / homotypic” or to “nomenclatural / taxonomic”.

Phillipson was unconvinced. Speaking of nomenclatural and taxonomic synonyms helped to maintain the distinction between nomenclature and taxonomy, a point that had been made earlier. Enforcing the use of homotypic and heterotypic would obscure that distinction.

Trehane knew of only one article in the Code in which these terms occurred: Art. 14.4, where they were defined.

Greuter added that they also appeared in the matter heading the Appendices.

W. Anderson liked the terms “homotypic” and “heterotypic”, which were more precise than the present terms and were immediately un-
understood by those who worked extensively in nomenclature. He guessed that for non-nomenclaturalists the traditional terms, “nomenclatural” and “taxonomic”, might be better suited. Voss’s proposed option of using both terms in every place where either occurred would be clear to all.

Greuter tried to sum up. The first question was whether both equivalent sets of terms be mentioned in the Code in the place where they were defined. A second vote, if the first one passed, would establish whether, in the other places, the nomenclatural / taxonomic terminology or the homotypic / heterotypic terminology would be used. W. Anderson’s now was a third option, to use both terms in every place where they occurred. [Thereupon, the first two motions were withdrawn in favour of the third, W. Anderson’s amendment, which thereby technically became an amendment to the original Prop. I and J. W. Anderson’s motion, to “use both the old and the new terminology in every place in the Code where they appear”, was seconded and carried.]

Palacios-Rios commented on the earlier [negative] decision regarding the use of “established” instead of “validly published”. In Spanish, “established” was meaningless when relating to publication, “validly published” was fine. If clarity of the Code was the goal, care should be taken of the meaning of words in other languages. Using alternative terms in the English text would facilitate choosing the most appropriate term in translations.

Greuter was aware of the difficulties linked with the translation of the English Code into other languages. There had by tradition been approved or accredited translations into German and French. St Louis might provide an ideal opportunity to have a new, perhaps approved or accredited, translation of the Code into Spanish. This would have to be sorted out between the many Spanish-speaking persons present. The main reason why there had never been a Spanish translation since the one by Jesus Izco of, he thought, the Leningrad Code [actually the Seattle Code] was that in past Congresses the Spanish-speaking attendance at nomenclature sessions had been close to zero. If there was to be a Spanish translation, or a translation into any other new language, the translators should carefully consider which technical terms were best suited to convey the intended meaning in their language. If, in Spanish, “válido” was not a good match for “valid”, then another Spanish term should be chosen. Translators were free to establish a Spanish tradition of nomenclatural jargon, and the same was true for
any language that had not yet an established tradition in nomenclatural terminology – and there might well be other persons in the room who did consider translating the *St Louis Code* into their language. They were encouraged to do so, as this was the best way to transmit the message of nomenclature throughout the world.

**Prop. I & J,** as amended, were *accepted.*

*The following discussion took place during the debate on Prop. L, but is included here for clarity.*

**Gereau** was of the opinion that a vote on the amended proposals I and J had been missed. There was to have been a second vote, as to which of the alternative sets of terms was to appear first, outside the parentheses, and which was to be second.

**Greuter** set the record straight. There had been no alternative left, because the proposers of both earlier alternative amendment motions, Voss and he, had withdrawn them in favour of W. Anderson’s motion. That motion, being the only one still on the floor, had been accepted, and **Prop. I & J,** so amended, had in turn been accepted. Upon a question by **Chaloner,** Greuter explained that it had not been decided which of the terms was to come first and which went into parentheses, it had only been decided that both were to be used side by side throughout the *Code.* The Editorial Committee would presumably decide to place the new terms in parentheses. This was an editorial matter.

**W. Anderson** confirmed that it had been his intention that the new terms, homotypic and heterotypic be put in parentheses. [The Editorial Committee would, Greuter promised, be aware of this.]

**Prop. K (102 : 109 : 7 : 4).**

**Hawksworth** introduced the proposal, which had achieved an almost evenly split vote in the mail ballot. The Committee on Harmonisation had felt the proposed change to be helpful, because “replacement name” was transparent in its meaning, whereas “avowed substitute” was perhaps confusing.

**C. Anderson** saw this as another instance where a new term was proposed to replace an existing one. The real problem was not semantics, it was understanding the *Code.* As an editor, she foresaw the same problems of having “replacement name” understood and used correctly as “avowed substitute”. There was no problem of understanding what
“avowed substitute” meant if one understood the *Code*. The proposed change would make no real difference for educating people to use the *Code*. The idea of simplifying the *Code* for the benefit of students was slightly patronising, as it implied that those now coming through the ranks were not as bright in understanding the *Code* as their predecessors. The *Code* was difficult reading. It was not possible to save people the trouble to understand it, just by semantics.

**Keil** suggested to deal with “replacement name” in the same way as for the previous issue, by adding the phrase in parentheses so that it would be there for those who wanted to use it. He was opposed to replacing a phrase that botanists already knew. This was an amendment **motion**.

**Brummitt** saw a difference between this proposal and the two previous ones, where both new terms were currently in use. “Replacement name” was currently unused in botanical nomenclature, and to introduce it as an alternative for a phrase that was in use would just add to confusion. He wanted a single option unless, as in the previous case, two terms were already in use.

**Greuter**, while **seconding** Keil’s **motion**, noted that it had not been clearly spelled out what had actually been moved. In his understanding, the motion was to add, in parentheses, “replacement name” at the point in the *Code* where “avowed substitute” was introduced and defined. His reason for favouring the amendment was that the word “avowed”, which might be perfectly familiar to all native English speakers in the room, was not readily understood by non-anglophone people unless they used a dictionary. The effect of the amendment would be to introduce “replacement name” as an explanatory phrase after the definition. Elsewhere in the *Code* “avowed substitute” would continue to be used. Keil agreed with this interpretation of his motion.

**W. Anderson** felt that this was not a case of adding clarity to the *Code*. It was a *BioCode* term sneaking into the botanical *Code*, which he opposed.

**Dorr** failed to see the benefit of adding “replacement name” in the *Code*. Art. 7.3 already had a parenthetical definition of “avowed substitute”, which was “nomen novum”. Where was “replacement name” to be put? Would there be parentheses after parentheses after parentheses?

**Ongaro** thought that, if a change served the purpose of communication, it was inappropriate to object to it just because it was used in the
BioCode. The proposal, as amended, was educational and would improve effective communication.

McVaugh concurred with Greuter in so far as those reading the Code often did not know the meaning of the word “avowed”, which was difficult to define, implying that a name had been vouched for, or had been supported and officially acknowledged by its author. However, the phrase “replacement name” was a wishy-washy phrase that could mean anything: a name put in by accident, perhaps added by somebody who did not know much about nomenclature. It did not explain “avowed”. If one did not like avowed, one should explain it or define it properly.

Demoulin had always, since the Leningrad Congress, found “avowed substitute” a phrase difficult to understand, perhaps because he was not a native English speaker – but even for those who were it posed problems. The previous Art. 72 Note 1, now Art. 58.3, referred to Talinum polyandrum as an example of a nomen novum, thus treating as an avowed substitute a substitute name that was not in any way “avowed”. He strongly supported the proposal, irrespective of its relation to the BioCode, because it would clarify the botanical Code. It had been said that “if it ain’t broke don’t fix it”, but while he could do well with his old Renault 4, which was not broken, he would do even better if someone offered him a new and better car!

McCusker added that “avowed” was a very archaic word in the English language. She doubted that only the non-English speakers had difficulty with it. She wondered whether the Section would “avow” any changes, but she suspected that the word would not appear in the record of the meeting. The concern that the Section was grappling with was not answered by this proposal, it required replacing the word “avowed” in the current expression “avowed substitute”. Retaining the word “substitute”, e.g., by writing “accepted substitute” or “agreed substitute”, might solve the problem.

Kolterman wondered if too much time was being spent on this proposal. The Index to the Code, under “avowed substitute” had: “see Nomen novum”. “Nomen novum”, rather than “avowed substitute”, was now used almost throughout the Code.

Davidse, while opposed to most other proposals of the series, supported the present one, simply because it increased clarity. In response to Brummitt, he pointed out that some journals, Novon among them, had already started using “replacement name” for that very reason.
Greuter expanded on Kolterman’s comment. The problem was that one could not simply translate “nomen novum” into English. A “new name”, in the Code, meant something completely different from an “avowed substitute”, or “replacement name”, or “nomen novum”. There was no straightforward way of rendering “nomen novum” in English. In the absence of any other concrete proposals, Keil’s motion to amend the original, perhaps too far-reaching proposal offered a good solution to those who wanted to get away from archaic English.

Zander claimed that the word “avowed” in the Code was superfluous anyway. If a new name was published as an “avowed substitute” for an older name, one was “avowing” it simply by publishing it. “Replacement name” was a marvellous substitute for the archaic phrase “avowed substitute”.

Orchard, at the risk of introducing confusion, suggested that a good bibliographic technique for dealing with this and similar problems, was to add to the Code a section with all definitions. Might this suggestion be referred to the Editorial Committee?

Greuter advised that this general suggestion be brought up at the final session, but for now to keep the debate to the point. The amendment to Prop. K currently under consideration read: “in Article 7.3, where avowed substitute is defined, in parentheses and to precede the words ‘nomen novum’, the words ‘replacement name’ be added.” Keil agreed to that wording.

Barrie, in response to a question about the exact wording of the present article, admitted that it was not possible to project portions of the Code onto the screen as the full text was not on the computer being used.

Keil’s motion was carried on a show of cards.

Prop. K, as amended, was accepted.


Hawksworth reported that this proposal, defeated in the mail ballot, had the support of the Committee for Harmonisation.

Greuter said that until, he thought, the Tokyo Code [actually the Sydney Code] there had been only one category of rejected names in the Appendices: those rejected against a conserved name. The names now called (in Taxon, by the Committees, and in the heading to the App. IV) “nomina utique rejicienda”, or names rejected per se, had a significantly
different nomenclatural status. Names rejected “utiique”, i.e. throughout, were, e.g., unavailable as basionyms for other combinations, whereas names rejected against other names remained available for use, depending on taxonomic context, and some were indeed in current use. It was therefore useful to designate these different categories of names by different terms. The term “suppressed” was not novel, it was already in use in zoology for exactly the same situation. In botany, the concept itself was novel and no other term had yet come into use. The proposal would have the effect that names listed in App. IV would be “suppressed names”, in contrast to names rejected against conserved names in App. II and III.

[At this point the discussion had reverted to Prop. I & J; see above.]

Borhidi noted that the proposal sought to replace “explicitly rejected” by “suppressed”. He needed clarification. How did this correlate with Greuter’s explanation of two types of rejection that had not been distinguished? Would there be a need for yet another term?

Greuter admitted that the proposal was somewhat deficient in so far as “explicitly rejected” was not part of the present terminology [in fact it did appear in the Code, but only by way of reference, in Art. 15.6]. What was meant by “explicitly rejected” was obvious, but the phrase would better not have been in quotes in the proposal. Definitely, “explicit rejection” meant rejection under Art. 56, as contrasted with simple “rejection” under Art. 14.

Borhidi felt that this was not the same as Prop. L.

Keil requested that, if possible, the Web version of the Tokyo Code be used for screen projection. Not all members of the Section had copies of the Code in front of them.

Barrie regretted that the computer was not hooked up to the Web, but would see if it could be arranged. [It could not.]

Greuter, to cover Borhidi’s concern, suggested a friendly amendment of the proposal’s wording: that rejection (or rejected) under Art. 56 be changed to suppression (or suppressed). This would apply to rejected names listed in App. IV.

Zijlstra disliked the proposal, mainly because rejection under Art. 14 was not such a homogenous condition. Some of the names rejected under Art. 14 were indeed in use, but others were not, because they were homonyms or nomenclatural (homotypic) synonyms. It was
perfectly fine to use “rejected” under Art. 14, with different meanings, and “explicitly rejected” under Art. 56, without adding a new term.

Nic Lughadha pointed out that the term “suppressed” was already used in the Code with respect to suppressed works. Names in suppressed works differed in status from explicitly rejected names that it was now proposed be called suppressed. Names in suppressed works were not accepted as validly published.

Compère drew attention to heavily negative mail vote (74 %).

Prop. L was rejected.
SECOND SESSION
Monday, 26 July 1999, 14:00-17:50

Preamble 8

Greuter noted the heavy positive mail vote. The proposal was in fact merely to update the current wording based on the new edition of the International Code of nomenclature for cultivated plants. Trehane confirmed this.

Prop. A was accepted.

Greuter explained that this was the keystone proposal of a large coherent package relating to hybrid nomenclature.

[At the request of the proposer, Trehane, discussion of this and all related proposals was postponed. Unaware of this, Zijlstra made the following comment during the subsequent discussion of a New Principle.]

Zijlstra explained her objections to the proposal. One of its consequences would be that they required nothogenic names to have a type, which at present was not the case. Thus, many existing such names would become invalid. The proposals referred to names and designations, the latter being invalid nothogenic names. This would not work.

[The following debate actually took place during the Seventh Session on Thursday morning.]

Trehane introduced the deferred hybrid proposals. His series of proposals published in Taxon dealt with cultivated plants and hybrids, the two being interlinked. The Section had already [by Thursday] addressed the proposals related to the cultivated plant Code and was left with the batch concerning hybrids. The Special Committee on Hybrids set up in Yokohama had considered the proposals and about 80 % of its members had approved them. The mail vote, however, showed a lack of enthusiasm, to put it mildly. The Rapporteurs’ comments on the proposals had had an underlying bite to them. This was a privilege granted to vampires.
There was an apparent lack of enthusiasm for the botanical Code to deal with graft chimeras and hybrids that arose artificially under cultivation. He was in a slightly difficult position as he was also Rapporteur for the cultivated plant Code. Preparation of the next edition of that Code would overlap in time with the editorial process for the St Louis Code. He personally, and the various commissions he represented, wanted to harmonise as much as possible the thrust and message of these two Codes, both dealing with the nomenclature of plants. There was one area of great uncertainty: when was a plant cultivated and when was it not? Arguments had been published in Taxon and elsewhere on the merits of taxonomy versus “cultonomy”, the details of which he would forego. Before introducing the proposals one by one, he needed the advice of the botanical community on a guiding principle: should artificial hybrids be in the realm of the botanical Code? He himself, the Section, the Editorial Committee, and the International Commission for Cultivated Plants, all needed a clear indication of the wishes of the botanical community through the Section’s majority opinion. The suggested directive was: “The botanical Code governs the nomenclature of nothotaxa. The cultivated plant Code should govern the nomenclature of artificial hybrids.” He did not argue for or against that directive.

Farjon found it difficult to judge on the matter, because the distinction between nothotaxa and artificial hybrids was unclear. Was, for example, the nothogenus xCupressocyparis an artificial hybrid? It was known to have originated under cultivation but was strongly suspected to have arisen spontaneously in a botanical garden or collection somewhere in the south of England. If nothotaxa and artificial hybrids could be defined in a way that all could understand, he would be happy with the suggested directive, but so far the distinction was unclear to him.

Trehane explained that if the meeting wished to split governance of hybrid names between the two Codes, the next edition of the cultivated plant Code would include a section dealing with the nomenclature of hybrid genera that would run in close parallel to that of the botanical Code.

Kolterman, working on an agricultural sciences campus, had to deal with horticulturists and other people working with cultivated plants. He thought it more useful to maintain the traditional distinction, that botanical names of all plants, whether native or cultivated, be regulated by the botanical Code and cultivar names by the cultivated plant Code.
Greuter tried to bring the point raised by Trehane into focus. The question was not whether cultivated plants could be named under either or both Codes. As long as they followed the rules, botanists were free to choose which Code they used. As had been said, botanical names were the domain of the botanical Code, cultivar names fell under the cultivated plant Code. But there was one category of names now covered by the botanical Code that was a very special exception to almost every one of its provisions and that referred mostly, but not exclusively, to cultigen taxa: the nothogeneric names, to which the cultivated plant Code people would like to have added the names of intergeneric graft chimeras, not now covered by the botanical Code. Both kinds of names were, in fact, very similar and parallel, there was little logic in having formula names for hybrids in one Code and those for graft chimeras in the other. Hybrid formula names were very curious beings: They had no type, could be formed simply by a statement of parentage, would change if merely the name of a parent genus changed. There were those awkward names of tri- or multigeneric hybrids ending in -ara, which looked like normal generic names but which, for unfathomable reasons, also must change if the name of a parent genus changed. These traditions were familiar mostly to growers, and naturally occurring intergeneric hybrids were few when compared to the huge number of the artificial ones that orchid or cactus growers had produced and would produce. Trehane was now asking the Section if it wanted to get rid of such names. The faculty to name hybrid “botanical” genera – by normally constituted names, based on types and validated in the usual way – would then be limited to those cases in which binomially named species were placed under the hybrid genus. Now this was not necessarily the case. Whereas for every new name of a “normal” genus a binomial must nowadays exist, as otherwise there would be no type under Art. 37, names of hybrid genera needed no type, indeed they had no type, so they were exempt from the requirements of Art. 37. One could have intergeneric hybrid formula names and only cultivar epithets associated with them. The option now existed to rid hybrid formulae, and all nothogeneric names that were not associated with binomials, from botanical nomenclature. This would require some tricks not to lose names that were currently needed, but solutions of that problem could be found.

Trehane agreed that this statement reflected the general thrust of the published proposals, but the question he was asking was slightly different: were nothogenera that were entirely artificial to be governed by the
botanical *Code*, or should they be removed and dealt with by the cultivated plant *Code*? This could be easily done but meant some extra work.

**Zijlstra** did not believe that such a solution would work for names of the past. Many nothogeneric names existed for which it would be impossible to make the distinction. In bamboos, cereals, bromeliads, and many other groups, some names were originally applied to natural hybrids; others to artificial hybrids derived from plants that were grown.

**Trehane** noted that Art. 28 required that plants brought from the wild into cultivation retain the name of the naturally growing plant. If a natural hybrid had been named under the provisions of the botanical *Code*, its name would continue to apply when it was brought into cultivation.

**G. Yatskievych** had a similar concern in relation with hybrids made for other than horticultural purposes, e.g., for genomic research. What would happen with some of the cereal grains in the *Triticeae*, where interspecific and intergeneric hybrids had been constructed that were later found to occur naturally? In the ferns, particularly in *Asplenium*, many artificial hybrids had been created for research purposes, and some had even been binomially named. It would seem odd if the names of hybrids synthesised for evolutionary study were to be governed by a *Code* for cultivated plants.

**McNeill** understood the reason for Trehane’s question, but wished to phrase it differently: should the botanical *Code* cover all generic names and all names that resemble generic names, such as nothogeneric formulae and generic formulae for graft chimaeras, or should the latter be hived off to the cultivated plant *Code*? Some typographical distinction might then perhaps be devised between, e.g., ×*Hordelymus* and *Hordelymus*, and the cultivated plant *Code* would need to provide against homonymy.

**Demoulin** feared that acceptance of such a principle would have a similar effect to adoption of Art. 28 Prop. B, “Delete the last sentence of Note 1”, which, as the Rapporteurs had stated, might lead to *Triticum aestivum* no longer being covered by the botanical *Code*. For some of the major crops, like wheat, one might never know if they were artificial or natural hybrids. If one was to go this route, there would presumably have to be a starting date.

**Greuter** reiterated that, in the same way as for ambireginal organisms, plant nothogenera that might or might not be cultivated could be shared
among the two Codes by using a simple criterion: not whether or not they were cultigens, but whether the subordinate taxa were named under the botanical Code or bore designations under the cultivated plant Code. Drawing the limit in this way was easy. Growers could not be prevented from naming their pets as “botanical” taxa if they chose to follow the rules and fulfil the requirements of the botanical Code. Authors could choose the Code they wanted to apply, and users could decide which names they wanted to adopt. A more delicate question was, what would happen to names that already existed. There were answers to that question, solutions of detail, but they were not worth considering before the general feeling was known.

Brummitt had problems with the question. How did one separate nothotaxa from artificial hybrids? Artificial hybrids were surely a subset of nothotaxa?

Trehane replied that the cultivated plant people disliked the term nothotaxon, and the cultivated plant Code did not employ it, whereas the botanical Code had a recent tradition of using the prefix “notho-”.

Brummitt was still baffled by the question. Was the intent to remove responsibility for all artificial hybrids, generic and specific, from the botanical Code?

Greuter had understood that Trehane was requesting a straw vote that would guide him as to which proposals he would present to the Section, and in what way.

Brummitt wanted to separate Trehane’s initial question, to be decided first, from arguments about the suitability of the present nomenclature of intergeneric hybrids and the concrete rules governing it. It would seem extraordinary to have two different ways of dealing with intergeneric hybrids, one for natural hybrids and one for artificial hybrids. He was against any segregation. The two Codes should, as much as possible, have the same rules and recommendations. Removing artificial hybrids from the botanical Code seemed a retrograde step.

Korf felt that the Rapporteur had put his finger on the main question: was it really desirable to deal with non-typified, non-typifiable names under the botanical Code? Clearly those working with the cultivated plant Code were able to handle such names. If there were names of cultivars that needed to be addressed as typifiable names, then they could be typified and treated under the botanical Code. One should
extirpate formula names from the botanical Code, leaving them to those who dealt with cultivated plants.

Trehane added that there was no provision for type method names under the cultivated plant Code. There, taxa were defined entirely by circumscription.

Borhidi was worried about the situation of artificial hybrids which escaped from the garden and naturalised. He supported Brummitt.

McNeill, picking up from Korf’s comments, said that they were clear and logical but side-stepped the question being asked, which referred to the present Code and did not assume that any of the hybrid proposals had been approved. The question was whether or not to transfer all nontypified, “apparent” generic names to the cultivated plant Code, retaining only typified names under the botanical Code. The Rapporteur had foreshadowed possible changes in the Code so as to typify nothogeneric formula names that, after such a transfer, might still be needed in botanical nomenclature, but at present they were not typified.

Zijlstra offered an example. A hybrid bromeliad had been brought back to Utrecht from an expedition to Guyana, along with other bromeliads growing nearby. The plant was described as a cross between two genera and given a nothogeneric name under the botanical Code. Then a grower crossed several species of the parent genera, creating new artificial hybrids. There thus was a generic name published under the botanical Code and several associated species described under the cultivated plant Code. Giving the generic name a type would not solve the problem.

Voss liked the definition of a cultivar once given by Jim Pringle of the Botanical Gardens in Hamilton, Ontario: a cultivar was a named horticultural variant, not just any cultivated plant. As had previously been noted, a cultivar name had no type and no standing under the botanical Code. Conversely, nothotaxa were named taxa of hybrid origin, their names had types and were governed by the botanical Code. So one was back to the Rapporteur’s good advice, that one choose which Code to apply. He agreed with McNeill that the real question was not the one on the board. It was: where should the names of hybrid genera go?

Greuter sensed that all were at a loss, unwilling to express a preference for one option or the other until the consequences of both were known. The Section was not now ready for action. He therefore moved that the Section authorise the establishment of a Special Committee, which,
supposing the governors of the cultivated plant *Code* agreed, could be a joint committee with some members nominated by the General Committee and others by the International Commission for the Nomenclature of Cultivated Plants. The new “Special Intercode Committee ICBN /ICNCP” would have the task to co-ordinate and harmonise the provisions on the nomenclature of hybrids and to produce one or possibly two sets of proposals to be considered for implementation. It was premature to take decisions now, because one did not know which way to proceed in principle until one understood the consequences in detail, and one could not work out the detailed consequences until one knew the preferred option in principle. Trehane [with others] seconded the motion.

W. Anderson supported the motion. He urged that one of the members of the Committee be a plant taxonomist who worked with natural hybrids, e.g., a pteridologist, who really cared about that part of the *Code*.

Greuter’s motion was carried.

Trehane withdrew all proposals except one or two, which the Section was asked to consider. [Details are given under the individual proposals.]

Prop. B was withdrawn (and referred to the Special Intercode Committee ICBN/ICNCP).

[Here the record reverts to the normal sequence of events.]

**Principle (new)**


[First gap in the tape record.]

Greuter introduced the proposed new Principle [see his comments accompanying the published proposal].

Voss felt that “guidance” would be a better word than “rules”. Also, the principle should not override all other principles, e.g., the independence of botanical from zoological nomenclature.

W. Anderson opposed the new principle because it might come back to haunt botanists in the future. If it were adopted, and made permanent, it would later raise the expectation of, e.g., granting protection to listed names in current use, an idea that did follow from it.

Silva explained that, whereas the stated intention of the proposal was to distil the essence of Preamble 1 for insertion as a new Principle, neither clarity nor efficiency of the rules were addressed in Preamble 1. The
proposer claimed support from Alphonse de Candolle's general considerations, but Candolle had not spoken of clarity of rules either. Rather he had stated that the rules should be based on clear and strong motives, to ensure understanding and acceptance. Much more objectionable was the switch from “stability of rules” to “stability of names”. Preamble 1 stated that the aim of the Code was to provide a stable method of naming taxonomic groups, which was the point of Preamble 1 that really deserved to be incorporated as a Principle. Unless plant taxonomy were put in a deep-freeze, there could never be stability of names. Names had to be allowed to change in response to changing taxonomic opinion.

Brummitt reminded the Section of the resolutions on stability adopted at the final nomenclature session of the Tokyo Congress. They had been extremely difficult to argue against because they sounded like common sense. So they had been voted in, but then shortly afterwards he heard that they were being quoted for purposes completely different from what anyone had imagined in Yokohama. The present proposal did not seem essential. While not necessarily against the essence of the proposed new Principle, he was very much against accepting it, because somebody somewhere would likely quote it in a way that neither he nor anyone else in the room could now imagine.

Demoulin failed to understand why the New Principle, which seemed so evident, could raise such opposition. It expressed clearly and straightforwardly what botanists were working towards. If this principle had existed earlier it might have saved the General Committee the trouble of overruling one Permanent Committee decision, so as to retain the nomenclaturalists' credibility before the rest of the botanical and agronomic world by conserving the scientific name of the tomato.

Pipoly, on first reading the proposal, had been struck by its introducing the concept of weighting principles against each other. To that he objected.

McNeill asked Pipoly and W. Anderson, and indeed anyone who opposed the proposal, which principle they thought was more important than stability of names. Plant taxonomists did themselves and the whole of taxonomy a grave disservice by the capricious changes of names, in which others thought they indulged. This was a false reputation: taxonomists did try to keep names stable, and surely that was their overriding principle. By voting down this proposal, the Section would
predictably send a very clear message, that taxonomists were changing names for their own pleasure.

Zander thought that this was a great proposal. The arguments against it were specious. How could one possibly imagine that the Section was forced into voting for something that it in fact disliked? Was the history of this Congress to be that it voted everything down? The Section should not be afraid to follow its own ideals. Specific changes in the Rules should then be dealt with on a vote-by-vote basis.

Kirkbride invoked Arthur Cronquist, who had contended that in every edition of the Code the Editorial Committee altered something because they felt that it affected some Principle of the Code. By installing this as an overriding Principle, the Section would give the Editorial Committee the right to change anything they felt did not support stability.

Faegri asked for comments on Jeffrey’s proposed rewording. [In a sheet of written comments that had been made available that day to the Section, Jeffrey had suggested that the New Principle read: “The aim of botanical nomenclature is to provide clear, unequivocal and effective rules for the establishment and maintenance of stable, unambiguous and universal names for botanical taxa (systematic hypotheses) of a given taxonomic circumscription, position and rank”.

Hawksworth noted that, before it could be discussed, it would have to be proposed formally from the floor, and seconded.

Greuter had not yet read Jeffrey’s alternative wording of the New Principle, which was a potential amendment to his own proposal. Submission in writing by someone who was not present had no standing unless it was taken up and moved formally. With reference to some of the previous comments, he expressed his disappointment at the mistrust they showed in how nomenclature was governed and enacted. Such distrust was not justified by actual facts. He would not let pass unchallenged Brummitt’s statement on what had been passed by the Section in Yokohama, which was a gross misrepresentation. By reading the published proceedings of the Yokohama sessions, one would find that the two relevant motions had been amply and controversially debated. No doubt had been left as to what would happen with the approved texts: One of them was directed at the nomenclatural world, being advice to the Permanent Nomenclature Committees, channelled through the General Committee; the other was to become a Congress resolution aimed at the world at large, to be communicated through the International
Union of Biological Sciences, the scientific umbrella for International Botanical Congresses. He took issue with the contention that there had been subsequent conversion of meaning of the Yokohama decisions.

Some felt that the last sentence ("all of the following principles are subordinate to this overriding goal") was inappropriate and lessened the value of the other principles. Well, Principles were statements of policy, not actual rules (see the Rapporteurs’ comments on Principle II, Prop. A, in the "Synopsis"). His own feeling, shared by many others present, was that this Principle was indeed the most central one for nomenclature to follow. As to the wording proposed by Jeffrey, he would be glad to accept it as a friendly amendment to his own proposal, on two conditions: first, that the rather theoretical and hardly relevant parenthesis "(systematic hypothesis)" be omitted; and second, that the words "first and foremost" be added before "aim".

Faegri moved the Jeffrey amendment. The motion was seconded. He and the seconds agreed to incorporate Greuter’s suggested modifications.

Greuter accepted this as a friendly amendment. The proposal now read: “The first and foremost aim of botanical nomenclature is to provide clear, unequivocal and effective rules for the establishment and maintenance of stable, unambiguous and universal names for botanical taxa of a given taxonomic circumscription, position and rank”.

W. Anderson felt that the vote on this amended proposal was among the most important votes the Section would take that day. This must not be done casually. All of this was a Trojan horse; he hoped that people who voted for this horse knew what was going to come out of its belly, somewhere down the line.

Gereau thought the discussion to be unnecessary at best, and at worst specious. Preamble 1 already included all of the elements: “a precise and simple system of nomenclature”, “a stable method of naming taxonomic groups”, avoiding “error or ambiguity”. As had been pointed out, the shift in emphasis was from rules to names. The subordination of everything else to this overriding goal was already in the Preamble: “other considerations, ... notwithstanding their undeniable importance, are relatively accessory”. The beginning of the Preamble was the appropriate place for this kind of subordination, not found in any of the present Principles. It should remain where it was now. [Applause.]

Hawksworth was disappointed at Brummitt’s impugnment of the integrity of the Section in Yokohama, by implying that it did not know
what it was doing. The fact that W. Anderson and others did not think the Code should aim at stability in names of taxa with an agreed circumscription was disturbing. If the proposal were to fail, this would be a real indictment of the Section.

Pedley felt that some sitting at the front table were overly sensitive. He had listened to Brummitt and had no idea what he was talking about.

Brummitt could not remember the precise wording of the motions passed in Yokohama, but recalled that one was for a Committee that was to consider problems of harmonisation of Codes, and two years later taxonomists were presented with the fait accompli of a BioCode. The second he did not remember, but the fact was that, within three months after the Tokyo Congress, papers had been published claiming that the Tokyo Congress had agreed that the principle of priority was no longer relevant in botanical nomenclature. Such extremist interpretations of apparently innocuous texts made him very hesitant to accept the proposal now before the Section, especially its [former] second sentence.

P. Stevens, as a point of light historical relief, mentioned that Alphonse de Candolle, maybe as a matter of filial devotion, had ascribed to his father the principle of priority. As to the proposal, it might indeed seem strange even to think about voting against anything that made stability paramount. However, looking at the way successive Congresses worked, one realised that a motion that sought to make something absolutely paramount would come fraught with consequences. Such a proposal might actually be desirable, but before adopting it one had to think through the likely consequences which, he dared say, would affect the whole of the Code, and might in turn be desirable – but he was certainly not going to vote for something of which he could not think through the consequences. For instance, in voting on conservation proposals it could become a matter of endless debate whether or not stability was being served by such and such a course of action. Adopting this proposal was driving into a tunnel with no headlights: if someone wanted to do it, fine, but he would not get into that person’s car. With apologies to non-English-speaking people, this would be rather like changing horses in mid-stream: the consequences were predictable.

Chaloner urged that discussions turn back to the two [he thought] motions before the Section: Greuter’s and the Jeffrey proposal. He urged that they be taken in succession, first taking a vote on the Greuter principle, and if that failed, considering the Jeffrey motion.
Greuter explained that a single proposal was being debated. The original text had been amended in the sense of Jeffrey’s proposal moved by Faegri. He had read out the resulting wording, which he as the proposer had accepted, and would gladly read it out again [which he did].

Briggs moved deletion of the last phrase, “of a given taxonomic circumscription, position and rank”, which was superfluous. [Her motion was seconded.]

Greuter did not accept this as another friendly amendment. Jeffrey’s wording took care of a legitimate concern. Without these words, the aim might be misunderstood: the principle was not absolute stability of names, irrespective of changes of taxonomic opinion, but stability within an accepted framework of circumscriptions, positions and ranks of taxa. Jeffrey’s added phrase was appropriate in the interest of precision.

Briggs’s motion was defeated.

Demoulin agreed that what was proposed as a new Principle was already in the Preamble rather than being new matter – but then, why was there all this discussion? As the Rapporteurs had written, the proposal wanted to give more emphasis to something now hidden in the Preamble. The Section was not discussing a really new Principle; indeed the proposal was, at the limit, merely editorial, a decision as to whether the matter be left in the Preamble or made more visible in the Principles. There was no harm in it, yet he thought it essential for clearer visibility that this became a Principle.

Davidse felt that it was very dangerous to make one principle primary over the six others, which was the case even of the amended proposal. If the words “first and foremost” were removed, he would happily agree with it. Similarly, he had no problem with the first sentence of the original proposal and he suspected nobody else in the room had. If the second sentence had been removed from the original proposal, he would have agreed with it, but not when it made the six current Principles subservient, so to speak, to the single new one. Asked whether he was moving an amendment, he replied that he was not, because the whole proposal should be voted down [by which statement, Greuter felt, he was slightly contradicting himself]. [Laughter and applause.]

Keil then moved to delete the words “first and foremost”, so that the Section could then vote on the basic issue and not mess it up with
whether it was of overriding importance over the other Principles. [The motion was seconded.]

Eckenwalder pointed out that the statement was actually stronger without the words “first and foremost”, because it then stated the (sole) aim of botanical nomenclature. The insistence on “first and foremost” was really unnecessary.

Keil’s motion was defeated.

Barrie, upon request, read out the amended text once more and confirmed that there was no second sentence left. The new text replaced completely both sentences of the original proposal.

Prop. A was rejected.

Principle I


Hawksworth noted that the proposal was essentially editorial, depending on the fate of Art. 54 Prop. A. In answer to a question, he confirmed that he intended to raise the latter proposal again, even though it had a 77 % negative mail vote.

Action on Prop. A was deferred; it was later rejected [as explicitly confirmed, at W. Anderson’s request, during the Eighth Session on Thursday afternoon].

Principle II

Prop. A (19 : 189 : 5 : 0) was ruled as rejected.

Article 1


Burdet noted that both proposals, and a number others, pertained to fossil plant nomenclature. Most of them concerned Art. 1, 3, 11, and 59bis, and belonged to two partly conflicting sets. Palaeobotanists present had asked that they be all discussed later in a single context.

Chaloner requested that action on all be postponed until Thursday, to allow an ad hoc committee, composed of members of the Committee for Fossil Plants that were in attendance and any other persons interested in palaeobotany, to meet and reconcile the conflicting views.
Greuter suggested that these two and any other relevant proposals be referred for study to an ad hoc Committee on Fossil Plant Proposals, to be co-ordinated by J. Skog, whom he invited to stand up so that everyone could see her. Any interested Section member could contact J. Skog and [as she confirmed] would be welcome to join the discussion.

Prop. A and Prop. B were referred to the ad hoc Committee on Fossil Plant Proposals. They were subsequently withdrawn in favour of a new proposal (see under Art. 3).

Article 3

Prop. A (34: 84: 19: 57) was also referred to the ad hoc Committee on Fossil Plant Proposals.

[The following debate actually took place during the Seventh Session on Thursday morning.]

J. Skog, reporting for the ad hoc Committee on Fossil Plant Proposals, asked that the Section, having just spent an hour on the letter i [see discussion under Rec. 60C Prop. C], devote five minutes to the really complex situation that existed in fossil plant nomenclature. Some explanations were required, as even members of the Committee for Fossil Plants had not fully appreciated the problems pertaining to fossil plant nomenclature that existed in the current Code.

Palaeobotanists had no great problems with Art. 3.3 as long as form-genera were concerned. Supposing one had a fossil leaf genus, say "Traversifolia", but no idea what family it belonged to, no conflict arose when it became linked to some other kind of fossil, because Art. 3.3 ruled that it was a form-genus (not assignable to a family) and Art. 11.1 permitted form-genera to exist without regards to priority. The problem came in when one defined, e.g., a pollen-grain genus with a wide stratigraphic and geographical range and with all the characteristics of the Tiliaceae, naming it, say, "Faegripollenites". Being assignable to the Tiliaceae it was not, by definition, a form-genus. Someone might then describe a genus "Chalonerflora" for a fossil flower and put it in the Tiliaceae as well, so again it was not a form-genus. If, later still, one found a more complete sample of "Chalonerflora", with stamens that contained pollen assignable to "Faegripollenites", the Code said one had to merge "Chalonerflora" in "Faegripollenites". Neither was a form-genus, so priority came into play. Palaeobotanists needed the information provided by "Chalonerflora" as well as the stratigraphic
information linked with "Faegripollenites", so they would side-step the issue and simply come up with a new generic name for the pollen-bearing flower. As long as palaeobotanists were dealing with fossils down in the Carboniferous, Silurian, or Devonian, such problems rarely arose; but when angiosperm fossils came into play, matters were different. Angiosperm palaeobotanists had to deal with reviewers and journal editors who seemed to think that one should follow the Code. Palaeobotanists thought so too, but had never been able to do so. The Committee for Fossil Plants and other palaeobotanists had wrestled with this problem, producing a number of proposals over the past 25 years. The original wording of Art. 3.3, which came out of the Committee for Fossil Plants and was approved by the Section [at Leningrad], used the verb "may be", but the Editorial Committee had changed this to "are". "Now just think of how many things you have just sent to the Editorial Committee (but no, I shouldn't say that)." [Laughter.]

The ad hoc Committee had considered the alternative sets of proposals and came up with the following solution: to retain Art. 3 Prop. A; to replace Art. 1 Prop. A and B by a new proposal under Art. 3; to combine Art. 11 Prop. A and G plus Art. 59bis Prop. A into a new proposal; and to accept an amended version of Art. 11 Prop. B. As the proposers had agreed, Art. 1 Prop. A and B, Art. 11 Prop. A and G, and Art. 59bis Prop. A would then be withdrawn. [A two-page hand-out with the texts of the new or revised proposals had been distributed.]

Chaloner offered further background information. The ad hoc Committee had considered two sets of conflicting proposals that had divided palaeobotanists: one carried his name, though others were involved, the other was by Skog & Fensome. Both had the same ultimate goal but used different phraseology. The ad hoc group that had drawn up the current proposals comprised four current and one former members of the Committee for Fossil Plants, including representatives of both factions. He was happy to support its conclusions. Essentially, if the package were adopted, this would make the Code conform to one hundred years of palaeobotanical practice. No retrospective nomenclatural change would ensue. Rather than affecting what palaeobotanists used to do nomenclaturally, the proposed rules would tweak the Code, making it conform to what was being done.

Greuter stated that the first proposal to be considered, Art. 3 Prop. A, was to delete Art. 3.3 and, from Art. 3.1, the phrase "except for some fossil plants (see Art. 3.3)." What would then happen to the dangling
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Note 1 would be left to the Editorial Committee. It would either be placed after Art. 3.4 or judged unnecessary.

J. Skog added that upon deletion of Art. 3.3 the present Art. 3.4 would be renumbered 3.3, and the Note would still be appropriate.

Greuter, replying to a request by Eckenwalder, regretted that the two projection screens could not hold the whole, large package of proposals, which anyway would be divided into four discrete items: (1) Art. 3 Prop. A, concerning Art. 3.3; (2) a new proposal to modify Art. 3.4; (3) another new proposal (with additional examples offered to the Editorial Committee), to add a paragraph to Art. 11, dealing with priority; and (4) the modified version of Art. 11 Prop. B, a change to Art. 11.7, which concerned also the algae. The first item, Prop. A, had been part of both sets of proposal, so all were in agreement over it from the start.

J. Skog noted that the Committee for Fossil Plants had been unanimously in favour of Prop. A.

Prop. A was accepted.

New proposal [Amend Art. 3.4 and add a Note: “3.4. As in the case of certain pleomorphic fungi, the provisions of this Code do not prevent the publication and use of names of morphotaxa of fossil organisms covered by this Code.” “Note 1. For the purpose of this Code, a morphotaxon is defined as a fossil taxon based on its mega- and/or microscopic structure, life history stage, or preservational state.”]

Greuter explained that the proposal would replace Art. 1 Prop. A and B, which were withdrawn. It no longer affected Art. 1, but would be placed in Art 3. It introduced a new term, “morphotaxon”. One of the original proposals had used “parataxon”, but the consensus now was that “morphotaxon” was the more appropriate, generally acceptable term. Since the proposal concerned palaeobotanists alone, others might not want to object too strongly.

Dorr felt that, being a motion from the floor, it had to be seconded before being discussed. [This was questionable, as it came from a Committee not from an individual; anyway, the new proposal was seconded.]

Jørgensen pointed out that ecologists were often using the term “morphotaxa” when referring to unidentified, especially sterile specimens in ecological studies of tropical forests. While this would not lead to an actual conflict with the Code, it still might create confusion.
J. Skog was aware of that usage. In fact, fossil specimens, too, were often hard to identify and of uncertain placement. Other terms considered by the ad hoc Committee had a lot of baggage attached. The proposed Note would provide a clear definition of the term "morphotaxon" as used in the Code.

Keil wondered whether it would be clear to which of the names previously published for form-taxa the term "morphotaxon" would apply.

Greuter understood that the notion of "morphotaxa" would encompass not only the present form-taxa and what used to be known as organ-taxa but, in fact, the totality of fossil taxa.

McNeill, not being a palaeobotanist, had difficulty with the "morphotaxon" definition. How did it differ from the whole fossil organism, from the "holomorph", to use the fungal term by analogy. Any totality of a fossil was based on form and structure, probably on many different structures. Was not some focusing needed on particular structures or stages of preservation?

Greuter had initially raised the exact same question as McNeill. He had been given the answer that all fossil taxa were in fact morphotaxa. There might be a conceptual "fossil holomorph", which could be referred to by a morphotaxon name informally: one could speak of a "Sigillaria forest", but the type of Sigillaria would still be a stem impression.

Traverse confirmed this. One of the alternative terms considered had been "palaeotaxa", which just meant "fossil taxa". The Committee had preferred "morphotaxon" because it emphasised the link to what used to be called form-taxa, being simply a translation of that term into Greek.

McNeill concluded that, when one spoke of a Sigillaria forest, it was simply a matter of convenience which of the many morphotaxon names one used for the ecological concept of the whole fossil organism. There was no question of priority, no analogy to the teleomorph vs. anamorph situation in fungi.

Demoulin, having some experience with the problems in fungi with a pleomorphic life cycle, shared McNeill's concern. The alternative term "palaeotaxon" would perhaps be preferable, unless the definition were phrased in such a way as to say that fossil taxa were based on form and structure, irrespective of life history stage or preservational state. If there was no palaeobotanical homologue to the fungal holomorph, he did not see the purpose of this special rule.
Greuter reminded Demoulin that he was not working in palaeobotany. When palaeobotanists from very different areas came up with a consensus as to what they needed, non-palaeobotanists should perhaps refrain from telling them what to do. [Applause.]

J. Skog, taking up the point, felt that wording of the definition could perhaps be tightened up, to read “on a particular form or structure, life history stage or preservational state”. The Editorial Committee would hopefully accept this improved wording.

Greuter confirmed that the Editorial Committee, of which J. Skog would hopefully be a member, was empowered to improve the wording.

Borhidi saw the analogy of fossil plants with “fungi imperfecti”: they were “plantae imperfectae”. He supported the choice of the term “morphotaxa” over “palaeotaxa”, because there were cases where a taxon, Metasequoia for example, was first discovered as a fossil. [However, that name had been subsequently conserved with a non-fossil type.]

J. Skog disputed the fungi imperfecti analogy. Often all parts of a plant were known: reproductive organs, stems, leaves, and roots. But these were dead objects from which one could never extract DNA.

Eckenwalder, rephrasing a previous question, asked what would be the consequence when one wanted to consider flower and pollen form-taxa as the same taxon. Which generic name would one have to use?

J. Skog replied that there was no consequence. The new proposal on Art. 11 would allow palaeobotanists to continue using both names, whereas previously they could not. For the “combined” taxon, they would be able to use either name, whichever they choose.

Chaloner phrased the same answer in slightly different terms. The important point was to retain separate genera for the separate organs. If a particular pollen genus was found in the anther of a flower, this would not raise the question of synonymy of the respective generic names. One would continue to use the pollen generic name for all pollen species that might have been put in that pollen genus, not all of which might in fact be associated with the same flower genus. Fossil taxa could range over a considerable span of time, millions of years. The object of this whole package was to permit the retention of separate names for genera representing separate organs and different preservational states. One of the two examples [in the new proposal to Art. 11] bore that out. What was very probably the same genus could be preserved in quite different
ways, and very different sets of characters were shown by those different preservation states. Their comparability was very limited, which was why retention of different names for them was necessary.

The new proposal on Art. 3 was accepted.

[The follow-up discussion is recorded under Art. 11 Prop. A; here, the record reverts to the normal sequence of events.]

**Article 4**


Greuter noted a 57% Editorial Committee vote. The proposal depended largely on Pre. 8 Prop. A, which had been accepted.

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

**Article 6**

**Prop. A** (15 : 196 : 4 : 2) and **Prop. B** (14 : 197 : 4 : 2) were both ruled as rejected.


Greuter stated that this was a mere “housekeeping proposal”, heavily approved in the mail vote. Art. 6.5 covered only taxa in the rank of family or below: above these ranks the law of priority was not mandatory.

**Prop. C** was accepted.

**Article 7**

**Prop. A** (8 : 211 : 1 : 2) was ruled as rejected.

**Prop. B.** (171 : 44 : 2 : 1).

Dorr introduced a friendly amendment on behalf of the author of the proposal, Pekka Isoviita. He read out both the original and the amended wording; in the latter, the words “for a name that is not typified by original description or indication” were replaced by the phrase “subsequent to the valid publication of a name”. A related point could perhaps be left for the Editorial Committee to deal with, or else needed a formal amendment. In other provisions requiring type designation, e.g., in Art. 37.4, “typus or holotypus, or its abbreviation, or its equivalent in a modern language” were called for. In the proposal, “here designated” was only given in English. Could a Latin phrase be added editorially?
Greuter noted that, since the first amendment came from one of the proposers, it was equivalent to a friendly amendment. The Section would not debate the original text but the altered wording. Dorr’s second point was indeed editorial. The Editorial Committee would take it into account and consider having “here designated” replaced or complemented by the Latin “hic designatus”, to achieve parallelism with other relevant portions of the Code. As to Dorr’s first point, the amended wording might perhaps overcome some slight technical difficulties of interpretation. However, both the original clause and the replacement one were in fact superfluous and could simply be omitted. The clause “for purposes of priority (Art. 9.13 and 10.5)” made it clear that the provision only referred to type designations subsequent to the original publication. So the amended phrase was redundant. This question, too, was editorial, and the proposal could nevertheless be accepted as rephrased, it being understood that the Editorial Committee had full latitude to take his remarks into consideration.

Voss agreed with the Rapporteur that the amended clause was redundant, but there was an additional reason for redundancy. A name was not validly published unless it had a type, and only validly published names were intended.

Greuter reminded Voss that there were pre-1956 names that did not have an original type and were nevertheless validly published.

Brummitt might be in a minority position, but he disliked the proposal. The rules were getting too dogmatic as to what one had to do. Art. 9.8 provided flexibility, specifying that if somebody used the wrong term for a type the error was disregarded and correction was allowed. That sort of flexibility was desirable in typification, as the rules on types were inadequate anyway. Introducing an additional restriction like the one proposed would lead to problems. The proposal would require that, if a typification were published that did not specify “here designated”, a second paper be published giving exactly the same information and, in addition, “here designated”. This was getting out of hand.

Kirkbride was in the minority with Brummitt, but for a different reason. This proposal would create problems similar those caused by Art. 37.4. Sometimes it was impossible to tell whether or not “the word type or its equivalent in a modern language” was used. The proposed rule would require General Committee decisions as to whether a particular phrase was an adequate “equivalent”.

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**Whittemore**, answering Brummitt, pointed out that with the way the *Code* was phrased accidental designation of types was extremely easy and happened constantly. Several Floras listed as “types” whatever original specimen the author happened to have seen, not realising that they were designating lectotypes, which in many cases were not the best that could have been chosen. It was necessary to stop these random citations from being effective lectotype designations.

**Nicolson** thought this to coincide with Art. 9 Prop. K, also heavily favoured by the mail vote. He failed to understand the difficulty it made.

**Greuter** explained that the current proposal would cover generic names as well, whereas Art. 9 Prop. K concerned only names of species and infraspecific taxa. As far as he could recall, the proposal resulted from a last-minute submission of Isoviita to the Committee on Lectotypification, pointing out that there was no good reason to treat generic and specific names differently. That Committee had endorsed both proposals, the more restrictive one (Art. 9 Prop. K) concerning only species and infraspecific taxa, and the present, more general one, concerning names at all ranks. The rationale for both, as stated by Whittemore was: incidental lectotype designations, which were never meant to be such, were a great nuisance. On the other hand, one could not prevent authors of, say, floristic treatments mentioning the word type. One did expect flora authors to state types when they knew them, but one should avoid the risk that, by so doing, they effected what they never intended to do.

**Davidse**, agreeing fully with Whittemore’s and Greuter’s statements, emphasised that the proposed provision would not be retroactive but was meant for the future. It would solidify good taxonomic practice and remove an element of doubt that currently existed.

**W. Anderson**, at the risk of being repetitive, expressed agreement with Davidse, Greuter, and Whittemore. He himself had inadvertently lectotypified names when, without realising that syntypes existed, he had designated one of these as “type”. This was an ongoing problem and should be dealt with.

**Prop. B** was accepted as amended.

*Article 8*

**Prop. A (175 : 29 : 11 : 2).**

**Greuter** introduced this important proposal, which had been positively received. It tackled a problem that had long been haunting nomen-
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Art. 8

The relationship between Art. 8, dealing with the type concept, and Art. 37 on the valid publication of names. Designation of a type was now a prerequisite for valid publication. The current definition of a specimen in Art. 8.1, linked with the fact that types, illustrations excepted, were said to be specimens, rendered many names technically invalid, if one looked carefully. It was like living on a ticking time bomb, but no one had yet dared to raise the problem. Botanists had to be grateful to the Committee on Lectotypification for having come up with a good solution to this complex problem, in no time at all. Among all committees he had known over the years, this had been the one that worked most efficiently. The wording they had come up with was almost perfect—almost, except that Compère had mentioned to him one critical detail. There was a risk that the proposed wording might be interpreted too restrictively, contrary to intent, to mean that mixed gatherings were not specimens. The intended meaning was that, in mixed gatherings, only the parts belonging to the single taxon whose name was being typified counted as the specimen—not, for example, the mould that might grow on the fruit of that taxon. To obviate the risk of misinterpretation, and also to cover the needs of workers in palynology, diatomology, etc., the suggestion had been made that at the end of the second sentence, after “collected at one time”, the phrase “disregarding admixtures” be added. This would make it clear that a specimen, even if a mixture, was still a specimen. If workers on diatoms and pollen grains still felt some concern, they should provide apposite examples to the Editorial Committee, to illustrate how the provision applied to their case. Barrie, the Secretary of the Committee, had indicated that he would accept the addition of these two words as a friendly amendment.

Barrie confirmed this [and no Committee member present objected].

Voss, much as he hated amendments, moved one: to add the phrase “by the same collector”, in the same position [as the foregoing addition], but leaving it to the Editorial Committee to decide where it was to go. Sometimes a team of collectors went out, each member collecting specimens and numbering them with his or her own numbering system. Were all duplicates? He thought not: a type should be restricted to a single collector. [Apparently no action was taken on this motion, which was not seconded, nor were seconders sought; the Editorial Committee took up Voss’s suggestion by slightly tightening the footnote definition of “duplicate”, where the point was already essentially covered.]
Pedley found the proposed specimen definition very detailed and wordy, and pretty clear. However, illustrations were not defined at all. In this day and age, it was ridiculous that an often pretty poor illustration could serve as the type of a name. He moved an amendment, to delete the words “or an illustration”. [Laughter; no seconders were found.]

Barrie explained that the Committee had considered trying to define illustrations, but had decided that with the time allowed and the complexity involved this was hopeless. They had simply left in “or an illustration” as an incidental.

Frodin suggested a small amendment in wording to the second sentence, beginning “A specimen is a gathering, ...”. One should distinguish between specimens known to support published names, that had been actually cited in a revision, and those which had not been so cited. The sentence should perhaps begin as “A cited specimen” or “A specimen so cited”. This was primarily an editorial matter.

Barrie had a problem with that suggestion. One of the strengths of the proposed wording was that it relied on label data, not on citations in publications. The Committee had gone back to trusting the specimen itself and the information associated with it.

Traverse, as a palaeopalynologist, was not at all happy with the idea that a whole slide was to be a specimen. In his field, the same slide often contained 5000 individual spores, pollen grains, dinoflagellates, cysts, etc., with maybe five or six of them having been designated as holotypes of as many different names. For that reason, though only for that reason, he much preferred the present wording of the Code. Special cases such as his did not seem to have been given sufficient attention in this proposal. He would therefore vote against it.

Pipoly asked whether the proposal would eliminate the possibility to designate photographs of the original material as lectotypes when all other original material had been destroyed. He was working with Myr-sinaceae, for most of which the types had been destroyed in Berlin, and often the Macbride series of photographs was all that was left.

Greuter, recognising that the situation was unfortunate, pointed out that it was not affected by the present proposal. Photographs of types were not original material and could not be types, at least not lectotypes.

Briggs thought it desirable to include further advice, by way of an example or a comment, on what to do in special circumstances, par-
particularly in the case of type specimens with several small dioecious plants including individuals of different sex. In the Restionaceae and Anarthriaceae, in which males and females of the same species were often notably different in appearance, one had a choice. The Code should specify whether lectotypification by a single individual was desirable in such a case, or, by contrast, whether, e.g., the holotype specimen of Anarthria scabra R. Br. was a specimen comprising male and female plants. She did not want to push one way or other, but felt that, with the proposal’s strong thrust against unnecessary narrowing of types to single individuals, further advice was needed.

Greuter answered that, even at present but especially with the proposal’s improved wording, the answer depended on whether or not one was confident that those plants of different sexes did belong to the same taxon. If one was, one would not choose an individual plant as lectotype; when in doubt, one was entitled to restrict the type to an individual by lectotypification. This would often be a question of two-step lectotypification, a matter that would come up in one of the following proposals [Art. 9 Prop. L], so discussion of this point should be postponed? The Editorial Committee was always grateful for examples to illustrate the kinds of questions that caused difficulty to users of the Code.

Briggs had another question: what constituted a gathering in cases of poorly localised old specimens? Did “Robert Brown, Gulf of Carpenteria” represent a single gathering? Brown probably thought of it as such; but the Gulf of Carpenteria, with a shoreline of over 1500 km, included several of Brown’s known collecting locations. This kind of question would tend to arise quite generally if the Code was going to discourage unnecessary lectotypification.

Greuter tentatively answered that if it was proven or at least likely that plants on the same sheet and with the same label were gathered at different sites or dates, having, e.g., different soil attached to them, they were not a single gathering, and one would have to lectotypify among them. However, the Code could not go into too much detail.

Brummitt had tried previously to come up with his definition of a specimen. He considered the proposal as a very good try, and hoped some of it would get through. He had, however, two reservations. One had been addressed by the amendment to disregard admixtures. He did not like, or indeed know, the word admixture, and doubted that the addition was needed. The original wording referred to a gathering “of a
single species or infraspecific taxon”, which depended on a taxonomic judgment. His preference was to delete the latter phrase, which in practice, he thought, would amount to the same. His other major reservation concerned the phrase “as long as the parts are clearly labelled as being part of the same specimen”, towards the end of the proposal. He had never ever seen two bits of a collection directly labelled as being parts of the same specimen; he had never seen “this is the same specimen as [another sheet]” written on a sheet. This hardly ever happened. When several Section members disagreed, noting the common use of “Sheet 1”, “Sheet 2”, etc., he responded that those annotations indicated two specimens of one gathering, not that they were the same specimen.

Barrie mentioned the common situation where specimen labels were annotated as, say, “sheet 1 of 3”, “sheet 2 of 3”, “sheet 3 of 3”. The first example given in the proposal was of a holotype consisting of two sheets. In the Field Museum there were plenty of sheets annotated with “fruit in fruit collection”, and a cross-reference to the sheet on the box with the fruit. As long as pieces in a separate collection were clearly labelled as belonging to a specimen, they should be considered part of that specimen, otherwise one got into difficulties. He had heard of an editor suggesting that a name was invalid because what had been designated as type was on two sheets, and the text of the current Code gave grounds for so arguing. The proposal would get around that problem. The footnote to Art. 9.3 in the Code, defining a duplicate, stated that “the possibility of mixed gathering must always be considered”. That footnote would hopefully shift to Art. 8 if the proposal was accepted.

Brummitt agreed with Barrie that a specimen definition was needed. He very much hoped the Section would agree on something that would work, so he would not persist with his point. But he wanted to comment on Ex. 4, relating to the situation where a holotype had been divided into two and part had been taken off to somewhere else. The result was that one had a holotype divided between two different institutions. It still was one holotype as it had been one specimen originally. He disagreed with the conclusions of the example.

Barrie, noting that Jeffrey had brought up the same point in his written submission, explained that this example was deliberate on the part of the Committee. There had been many problems with the concept of a holotype being divided amongst several institutions. If the main sheet was destroyed or lost, under Brummitt’s approach one might be stuck
with a holotype consisting of a leaf fragment or part of a flower that had been removed from the original type and moved to some other herbarium. It was preferable if an isotype, a complete specimen, was eligible as lectotype. This was the deliberate opinion of the Committee, a logical approach when one thought about it. A gathering could be divided into parts or portions at several points in time, by the original collector or the preparators in the herbarium; if material was sent unmounted to another herbarium, it might there be split into two or three. It presented no problem to consider that when part of a holotype left its institution, it became a duplicate rather than remaining part of the holotype.

McVaugh noted that a collector might confuse specimens of small plants of different species under the same number. Pringle, the great Mexican collector of the 19th century, used to collect 60 sets of perhaps only one or two numbers in a day, which was fine if he collected material from the same tree; but he also collected at waterholes or temporary summer pools, where many small plants grew together, so he sometimes included as many as five or six species of the same genus of Cyperaceae under the same number.

[Second gap in the tape record.]

Whittemore reiterated what Barrie had already pointed out: the Code (Art. 9.10) gave clear instructions for what to do when the type turned out to be a mixed collection. This could deal with McVaugh's problem, so long as there was a practicable definition of "specimen" such as this proposal provided.

Compère supported the deletion of the phrase "of a single species or infraspecific taxon", as suggested by Brummitt. This would take care of types that belonged to, e.g., a mixed slide with diatoms.

Gandhi referring to Ex. 4, expressed preference for speaking of holotype fragments, rather than calling them isotypes.

Demoulin, like Compère, was still unhappy with the second sentence, in spite of the friendly amendment of it. Could that phrase not, instead, be amended to read: "a gathering supposed to be representative of a single species or infraspecific taxon"? [No formal motion to that effect was, however, presented.]

Prop. A was accepted as amended.

[End of the second gap in the tape record.]

Greuter hoped that the mail vote’s heavy “no” rate (68 %) did not reflect the Rapporteurs’ slightly critical comments. The Secretary of the Committee on Lectotypification could perhaps dispel such misgivings.

Barrie explained that one Committee member had requested such a proposal, to cover the need of people who worked on, e.g., cacti, grown in a greenhouse and of which parts collected at different times might all be pickled in the same jar or mounted on the same herbarium sheet. This was thought to be a common practice of some, whom the proposal would help. A similar proposal made for the Tokyo Congress had received a fairly favourable mail vote, but had not fared well at the Section meeting. Its point was to allow a specimen to be accepted as a type if, under certain circumstances, material collected at different times was mounted as a single preparation. However, the proposal precluded addition of material to the specimen once it had been designated as type.

Voss spoke strongly against the proposal. As worded, it would allow the deplorable practice of designating as type flowering and fruiting material from the same plant collected at different times. He had run into this in connection with Crataegus, a bad enough mess to begin with, where a large number of names had been published with “types” collected at several different dates, resulting in mixed collections and confusion. Even Liberty Hyde Bailey, about whom he hesitated to say anything negative, had been accused by botanists of having primocane and floricanne material from two different species as the basis of some of his Rubus names. This was a bad precedent to encourage. Better stick with a single specimen as type.

Demoulin failed to see why Voss objected so strongly. The important point was, all parts had to come from one individual plant. If the collection was mixed, the article would not apply. There were situations where it might be helpful. When working in New Guinea, he had labelled trees permanently and had sampled them in flower and later in fruit. If the species were new such specimens, if made types, would carry much information. There was nothing wrong with this.

Dorr was bothered in many ways by the proposal. He appreciated the special problems of people working with cacti. But there had been collectors like Pringle, whom McVaugh had mentioned earlier, who used to collect a flowering plant early in the spring then go back and collect what they considered to be the same plant in the fall in fruit, mounting
both on the same sheet without specifying whether or not the samples came from the same individual. It was unlikely that they did, but there was no way of telling. We would create problems by allowing material of different stages and times to be mounted together on a single sheet.

**Farjon** thought that, if the proposal were accepted, the onus of proof that all parts were from a single individual would not be on the collector but on the typifier. Before the proposed provision could work, a whole set of additional rules would be needed, specifying criteria for the collectors to prove that their specimens came from “an individual plant”. He was against the proposal.

*Third gap in the tape record.*

**N. Taylor** added that there were dangers associated with living collections. Their labelling was not secure. Labels were liable to be transferred from one individual to another, which might happen between the times when flower and fruit were gathered. Moreover, as the proposal was retroactive, it would potentially affect previous assessments of (non-)validity when cited type “specimens” included non-contemporaneous elements.

**Phillipson** read the proposal as an attempt to rule the management of herbaria, not as a rule of nomenclature.

*End of the third gap in the tape record.*

**Gandhi** pointed out that the proposed rule would apply not only to cacti or, as had been said, *Crataegus* but also to species of, e.g., *Brassicaceae* and *Apiaceae*. He was against such a provision.

**Moore** noted that the rule that the Section had just passed defined a specimen as being collected at one time. This proposal, stating that a specimen could be collected at different times, seemed to conflict with the previous definition.

**Greuter**, commenting on that “technical” point [laughter], explained that if the Section wished to adopt this proposal the Editorial Committee would eliminate such contradiction, e.g., by a cross reference, “(but see ...)”, in the new specimen definition.

**Moore** would then, logically, change the specimen definition to read “at different times”, when that was what it really meant.

**Greuter** conceded that, alternatively, the Editorial Committee might decide to alter the specimen definition itself. His point was, if the...
Section took decisions that were conflicting in detail, it was the Editorial Committee’s role to eliminate the conflict. There were two issues to be considered. One concerned types of the names of future new taxa. Was it really necessary to introduce such an option for the future, when one already had the possibility of designating the material collected in, say, fruit as a paratype? If the holotype and the paratype would later turn out to belong to different taxa, one would then know with which taxon the name went. The other issue, mentioned by N. Taylor, was the relationship between Art. 8 and Art. 37. As the proposal was retroactive, some names that were hitherto not validly published because their stated type was not one specimen but had been collected at different dates, would now become valid. This might or might not be desirable, depending on the extent to which the rule had been enforced or neglected so far. If such “types” had been repeatedly used, after 1957, for names that were now generally accepted as if they were valid, then there might be a problem; but the point should better covered under Art. 37, not Art. 8, and a different proposal would be necessary. However, judging from N. Taylor’s comment, at least in his case such a new rule would be destabilising, validating names that were now known and considered not to be validly published. So there appeared to be no need for the proposed rule.

W. Anderson agreed with Greuter. The Section should reject the proposal. He did not want to beat a dead horse, but this was mixing up taxonomy with nomenclature. The beauty of the type method was that it tied unequivocally a name to something physical – the simpler the better. The proposal was trying to make the type biologically more complete, but paratypes allowed that to be done in a way that did not dilute the simplicity of the type method.

Prop. B was rejected.


Hawksworth explained the proposal’s aim. It would replace a “voted example” accepted in Yokohama that permitted cultures preserved in a metabolically inactive state to serve as types, and would in effect restrict that option to certain fungal groups, excluding all other groups, algae in particular. Gams might wish to expand on that point. He found it unsatisfactory that the fungi specified in the proposal, e.g., ascomycetous and basidiomycetous yeasts, were not phylogenetic groups.
Gams, as an author of the proposal, stressed the great merit of the "voted example" introduced into the Code by the last Congress, which had saved many names of yeasts that had been published since 1960. In other fungal taxa as well, and perhaps in algae, there was a great need for valid names based on living material that was kept permanently, e.g., by lyophilisation or in liquid nitrogen, but could be revived and re-examined alive, as there was no other way of identifying these organisms. Culture collections were now in a position to guarantee a very stable preservation. Not only mycologists but also many phycologists might benefit from this proposal. It had been difficult to formulate a proposal that all members of the Committee of Fungi would support, as a type must be immutable and there had been concern that living cultures might degenerate or change. There was indeed no absolute guarantee that they would not. Therefore the proposal had been restricted to those organisms for which it was really necessary. He had listed the organisms for which it was to apply, but he realised the risk that others, not now listed, were in need of similar treatment. So he had reformulated the published proposal by replacing the list of organisms by the phrase: "except in certain fungi and algae normally studied in culture, where permanently preserved killed material is deemed insufficient to recognise a species, for which cultures, ..."). This modified version was to replace the one that had been published.

Greuter explained that the original published proposal was off the table, and the Rapporteurs' misgivings expressed in the Synopsis had been taken care of. Only the new text was now being considered. He added some information for the benefit of those who were neither mycologists nor phycologists. There was an old principle in the Code, that types could not be living specimens but must be permanently preserved. One could not typify a new name with the cactus one had in one's greenhouse (a heartbreak for many cactologists who had to sacrifice their only pet if they wanted to describe it as a new species). There was no question of departing from this generally accepted principle. However, many microbiologists, especially those working on the structurally less developed groups of fungi and algae, had felt the need to have permanently preserved cultures as types, because they could not see the relevant characters on dried, dead specimens. They had found a solution to their problem at the last Congress. Not wishing to change the Code too much and weaken the old concept, that living types were not permitted, they introduced a "voted example", declaring that permanently...
deep-frozen, lyophilised cultures, even if they could be revived, were for the time being dead. The rules were perhaps twisted a little bit by this example, but after all, on a geological time scale, herbarium specimens were barely more permanent than such cultures. The agreement reached in Yokohama had solved the previous problem, at least for the time being. The question was, had it not been quite solved? Was there a real need to abandon the perhaps a bit Machiavellian construct of the "voted example", which declared lyophilised, deep-frozen cultures to be dead for nomenclatural purposes? He had liked that nice and expedient way to deal with the problem. Perhaps Gams could explain what the difficulty was; if he could, he would have the sympathy of the audience.

Gams held the opinion that a mere "voted example" was too weak to cover what for many mycologists was general practice. A definite rule was needed. Otherwise, a risk would remain that a name based on permanently preserved yet living material would incur the penalty of being considered not validly published. Another aspect, that should not be ignored, was that DNA was particularly stable and nowadays could often be analysed from strongly degenerated fungi, allowing their origin to be established.

Hawksworth could see no harm in the proposed rewording. It would make more explicit what had been agreed in Yokohama.

Demoulin disliked having cultures as types, which was a fundamental misconception of the type method. Twenty years earlier one might have been able to defend the idea that permanently preserved material was insufficient to recognise a species. Now, he did not believe there was a case where it was impossible to recognise a species by making a DNA sequence from dried material. Therefore, even if the proposal were to pass, he would not consider that any name that made use of the provision was valid. Like the previous proposal, which had just been rejected, this one would rule on biological methods; on how it was possible to recognise a species. Gams had been killing his own proposal by speaking of DNA analysis: modern possibilities of DNA analysis made it possible to revert to the good principle that a type must be permanent, not susceptible of evolving or getting contaminated. Bacteriologists had recognised this. A paper by Sneath, which had been distributed to the members of the Special Committee, showed what interesting things one could do nowadays with dead bacterial cultures. A debate of the issue of living types must consider the full evidence and not introduce them
by the back door, as had been done at the last Congress by means of the
"voted example". The latter was necessary, owing to the bad practices
of yeast taxonomists, so as to preserve the names they had published.
But being practical by allowing all these yeast names to be accepted was
one thing; generalising this acceptance into a legal practice was another.

G. Yatskievych found the idea perplexing, that one had to kill material
in order to preserve it. Lyophilisation required drying material far dryer
than the average herbarium specimen, which was perpetually exposed
to 40 % humidity. Did one invalidate a name in, say, Marsilea if one
germinated a sporocarp from its type specimen, that had perhaps been
collected a hundred years ago, and grew it into a new plant? When
declaring a specimen to be a type, one wanted it to be preserved in as
permanent a condition as was possible by using the standard technology
of the time. The fact that there might be live propagules or living parts
on that type for some years beyond its designation in a formal publica-
tion was irrelevant. The ability to extract strings of DNA from herbar-
ium specimens further complicated the question of when it had truly
passed the point when it could be altered, or be examined in some non-
traditional way. The proposal made perfect sense. It addressed the issue
for those groups in which traditional herbarium specimens or slides
were difficult to use. A specimen that was of no use to a future taxono-
mist was not worth being designated as a type in the first place.

Demoulin regretted to start with this debate yet again, as it had arisen at
every Congress since Sydney. The example of Marsilea was one of the
very reasons why the voted example was good as it stood. In moulds,
conidia stayed alive for many years yet there was no problem with them
being types. Having lost the thread of the argument, he declared himself
to be too tired to continue.

Greuter noted that the proposal now mentioned "certain fungi and
algae normally studied ...". This was a “rubber statement”, perfectly
acceptable so long as it just concerned the nature of types of extant
names. But the provisions of Art. 8 reflected on the validity of post-
1957 names under Art. 37, for which designation of a type was re-
quired. He was wary of possible conflict and uncertainty over the ques-
tion whether a given fungal or algal organism did or did not fall under
the new provision. Was there any objection against making this a Note,
to illustrate the “voted example”, which had force of law; and, further,
to let it apply to fungi and algae in general, not to “certain fungi and
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algae”? Concretely: would Gams accept as a friendly amendment (1) that the proposed clause be transformed into a Note; (2) at its start, to eliminate the word “except”, and to make it clear that permanently preserved cultures are, as a matter of fact, accepted as non-living specimens, and (3) to delete the word “certain”, so that fungi and algae in general would be covered?

Gams agreed.

Demoulin, having recovered, referred to the standard metre of platinum preserved in Paris. It was not used to make measurements, but it was good to know that it existed. Type specimens were of the same kind. They were not used for every-day control of determinations. Those working with organisms that were usually studied in culture, routinely used cultures taken from the type, ex-type cultures, as advised by Rec. 8B. The holotype status, however, should be reserved for something stable, that could not get contaminated, and would be used only in exceptional cases, for sequencing some of its DNA. That was good practice, from which one should not depart. He had no objection, however, to a Note explaining the voted example.

Gams held that Demoulin was overly optimistic on the value of dried cultures for extracting DNA. Even with recent material this could be problematic, and there was certainly nothing better with which to work than a living culture. If, as proposed, type cultures of a given kind were recognised without penalty of invalidity, then as a next step one could demand the deposit of such type cultures in at least two culture collections and establish something like the standard metre. This would stabilise the application of names much better than any dried material. The danger of contamination was minimal in recognised culture collections.

Gams had meanwhile understood that the whole proposal had, with his consent, been replaced by a Note, just displayed on the overhead [which read: “Note: For fungi and algae, cultures, if preserved in a metabolically inactive state (by lyophilisation or deep-freezing), are acceptable as types.”]. He now felt that this would dissatisfy many mycologists, who would see doors wide ajar for living types to be recognised for any fungi. There had to be some kind of restriction, along the lines he had initially formulated, subject to editorial modification.

Greuter noted that the weakness of Gams’s original proposal was that restricted groups to which it applied were ill-defined, whereas the weakness of the present version might be that the groups, while clearly
defined, were perhaps too wide. This dilemma could not be resolved by the Section, it had to be answered by specialists in the groups concerned. Action should better be postponed until Gams could get together a group of phycologists and mycologists to advise the Section.

[The following conclusion came about on Tuesday afternoon, during the Fourth Session.]

Gams reported that the mycologists and phycologists present had met. They had come to the conclusion that it was not feasible to define specified fungal groups to which this ruling would apply, and that it was therefore better to retain the existing voted example and insert the proposed Note. The only change suggested to the text of the amended Note was to insert “e.g.,” at the beginning of the parenthesis.

Prop. C was accepted as amended.

[Here the record reverts to the normal sequence of events.]


Brummitt commended this to those who had got a bit lost on cultures as types but liked a good controversial proposal: one that all could understand, although perhaps not immediately. The provision was about illustrations as types and applied in two situations: when it was impossible to preserve a type, and when a name was without a type specimen. Having read out Art. 8.3, he emphasised that it did not say “Only if it is impossible to preserve a specimen as the type ...”, that there was no restriction by the word “only” in that sentence. However, some people had chosen to interpret the article by reversing its sense, taking it to say that a type might be an illustration only if it was impossible to preserve a specimen. It had never been intended to convey this impression. When preparing the proposal, Forman and he had gone through the history of this provision in the Code, and found that it was an archaic throw-back to the Codes of the 1950s or even earlier, which had no place in the present Code. Deletion would have no effect except to avoid the confusion which the article generated. Most people did like a broad allowance of illustrations to be types. There were cases when an author was known to have used illustrations in preference to specimens when describing a new taxon. Forman had been working on Roxburgh, who was known to have been looking at the illustrations and not at his herbarium specimens. The Committee on Lectotypification had actually voted 7 : 1 in favour of deleting Art. 8.3, which was exactly the present
proposal, but the Rapporteurs in the Synopsis had only referred to that vote under Rec. 8A. Many wanted to allow illustrations to be types, and get rid of the restrictive and, in his opinion, completely false interpretation of this confusing Article.

Barrie supported Brummitt. The Committee on Lectotypification had had several reasons for wanting to delete Art. 8.3. First, how was one to define “impossible”? Was it “impossible” to designate a type specimen when an author had an illustration but not a specimen at hand, although he could go out and collect one? Or, rather, did the author have to wait until a specimen had been collected and then use it as type? This might be a problem if the plant grew in some remote place difficult to access, and illustrations were all that was left because the specimens had been lost on being shipped back home. The second problem was retroactivity. Many names, especially of flowering plants, had been based, for various reasons, on illustrations when it would have been perfectly possible to use a specimen. What was to become of those names?

Greuter, as the Rapporteurs’ comments had been quoted, restated some of these remarks. As Brummitt had said, this provision had indeed a long history, being one of the oldest provisions in the Code. It went back, perhaps not to the Vienna Rules, but certainly to pre-war Codes [in fact, to the Cambridge Rules, Art. 18]. In the past, there had been two distinct rules: one concerning the situation when there was no original plant material, when the type could be an illustration; the second stating that, if for any reason it was impossible to preserve a specimen, then the type could be an illustration. The meaning of these two provisions, in the beginning, had been quite clear. The early Codes considered it undesirable to have type illustrations, and permitted this only in clearly and restrictively defined cases. Later the two rules had been editorially amalgamated, not by decision of a Congress but by an Editorial Committee, and subtle changes of wording, also done editorially, had taken some of the sting out of the provisions, without any positive Congress action to modify them. So much for the historical background. Admittedly, the present Art. 8.3 was less clear than the original rules had been. But still, the two halves of Art. 8.3 had different meanings and different implications, and should be dealt with separately by the Section. Perhaps both would be handled in parallel, perhaps both would eventually be deleted, but better not in a single go. He did support deletion of the obligation to resort to specimens and disallow type illustrations, when there was original plant material available.
Maintaining this old traditional restriction was destabilising, as Jarvis had pointed out to him. Deletion of the second provision, which disallowed validation of names of new taxa based on a type illustration when specimens could be preserved (and were perhaps even cited), was critical. That deletion would have retroactive effect. Contrary, perhaps, to what many believed, many post-1957 names had been considered as not validly published when they were typified by an illustration, when a specimen could have been designated. He knew of several such examples among the orchids. There might be good reasons for this second deletion, too, but caution was indicated. His advice was to keep the two issues distinct, and the substantial “ed.c.” mail vote supported that advice, although the “no” vote was even stronger.

Dorr, even though Greuter had appeased some of his concerns, was still unsure of the possible ramifications of Brummitt’s proposal. He would be very leery of the proposal if it had an effect on other provisions of the Code such as Art. 44.1, stating that “an illustration with analysis” could validate the name of a pre-1908 taxon. That provision that was very important for the names of Malagasy plants first published in Grandidier’s Histoire, of which the text that was to accompany the plates had never been published. These plates served as types for many plant names, and, if he understood Brummitt’s proposal, it would be destabilising for them.

Korf also wanted to eliminate Art. 8.3 from the Code, but out of a different perspective. Iconotypes had always bothered him. He had been unable to convince his students that they could section an illustration, or that they could analyse an illustration’s DNA. It was worth remembering that under the older Codes not only illustrations that had been allowed as types, but descriptions or diagnoses as well, when specimens were no longer available. Type descriptions and diagnoses had been got rid of, and he had cheered, but illustrations had remained as possible types, primarily because many herbarium people cherished illustrations. Luckily, the epitype provisions had solved the problem. Whereas it was not possible to designate a neotype for the name of a taxon when an illustration was part of the original material, the Code now provided for epitype specimens, which could be sectioned and on which one could do DNA work. He hoped to see the day when iconotypes would have lost their importance, and epitypes would have taken their place.

McNeill supported what Korf had said but came to the opposite conclusion concerning action on this proposal. By deleting the provision,
illustrations would be allowed to be types under any and all circumstances, which would be extremely disadvantageous. Historically, it was necessary for many early names to be typified by illustrations, but new taxa should not be permitted to be based on illustrations as nomenclatural types, except perhaps in the exceptional cases when a type could not possibly be preserved – and he did not know what cases these might be. The Section should follow the Rapporteur’s suggestion and split the issue. It might vote to delete the component relating to the past, but should continue to prohibit illustrations as types for the future.

Pedley thought that illustrations should have no part in the Code. He supported the previous speakers who had brought out all relevant points. The earlier discussion on cultures and their preservation suggested that one could preserve almost anything. Typification by a specimen did not pose problems. When all the original dried plant material had been destroyed, one could neotypify. Supposing that all illustrations had been destroyed, again one would neotypify. Why not go the whole way and rule that illustrations were out and specimens were in?

McVaugh asked if there were other provisions in the Code, apart from Art. 8.3, that allowed for illustrations as types?

Greuter replied that in Art. 8.1 illustrations were specifically permitted, and in Art. 9, the definitions of the various type categories (e.g., Art. 9.1-2, and 9.6-7) repeatedly mentioned “specimens or illustrations”. [He might have added Rec. 8A.1-2; Art. 9.14, 10.4, 37.3, and 37.5.]

McVaugh mentioned the fact that a pretty good botanist, A.-P. de Candolle, had named about 300 species on the basis of illustrations alone. He held what he considered good pictures of them, but had no possibility of getting specimens. One could not get rid of these names.

Bhattacharyya mentioned the illustrations published by Rheede (1678-1703) and subsequently used by Linnaeus, by Roxburgh (1795-1820), and by Wight (1838-1853). They been based on wild plants from the Old World tropics, and field studies at the type locality made it possible even today to identify these illustrations with actual plants. Those who were unable or unwilling to do field work might wish to delete Art. 8.3, but he and other taxonomists from India opposed deletion, which was an unhealthy and undemocratic proposal. Proscribing type illustrations for the future, e.g., after 2001, might be acceptable.

Jarvis pointed out that there were about one thousand Linnaean names that were either typified or potentially typified by illustrations. If one
would disallow such types, one would not necessarily be able to desig-
nate neotypes instead. There was often other original material, that
might be taxonomically different from the illustration that subsequent
authors had used for the interpretation of the name. Immense confusion
would thus be created.

Gandhi added to McVaugh’s comment. There were examples in or-
chids of new species described solely on illustrations made one or two
centuries ago, with no plant material extant.

Farjon suggested deleting Article 8.3 but adding to the Code a strong
recommendation, that as of a certain date in the future, names of new
taxa should preferably have specimens as types rather than illustrations.

Greuter noted that such a recommendation, though not using exactly
those terms, had been proposed as Rec. 8 Prop. A by the Committee on
Lectotypification. In view of what Jarvis had said without being contra-
dicted, a consensus was perhaps building up, to allow illustrations to
serve as lectotypes of old names. Would the Section agree to clear the
ground for the central discussion by accepting, as a first step, to delete
the relevant portion of Art. 8.3, the phrase between the commas: “or if
such a name is without a type specimen”? Deletion of the remaining
paragraph, relevant for the question of valid publication of post-1957
names, would then still be open for discussion. This, he said, would
simplify the debate. As there was no objection to this procedure, he
confirmed the present question was deletion of the phrase between
commas in Art. 8.3, and read out what would remain of the paragraph
after such deletion: “If it is impossible to preserve a specimen as the
type of a name of a species or infraspecific taxon of non-fossil plants
the type may be an illustration.” The present provision that limited
designation of “icono-lectotypes” (a nice new term, not yet in the Code)
to cases where there was no plant material available, against which
Jarvis and others had argued, would then be removed. At the request of
W. Anderson, he specified that a “yes” vote would be for deletion, as
recommended by Jarvis, the specialist on Linnaean types, and as he him-
self favoured as it would stabilise the application of Linnaean names

Barrie pointed out that (except for holotypes, which was what some
people were worried about) potential types that mattered in the first
place for lectotypification (i.e. syntypes, paratypes, and isotypes) were
defined in the Code to be specimens. They could not be illustrations.
Greuter agreed that syntypes, etc., by definition, were never illustrations. Lectotypes could be illustrations, but under the present Art. 8.3, only if no other original material was extant. This would be changed by the proposed deletion, so that in the future an illustration could serve as lectotype even if the odd original specimen was extant.

Nic Lughadha felt that her point, that there was no “only” in that provision, had not been addressed. The article had no force, whether the clause between the commas remained or not, because it did not specify “if and only if” but just had “if”, which did not exclude the opposite possibility.

Greuter responded that there was an old controversy on this point. The article’s historical root showed that “only if” was meant. When it would have been disruptive to apply the provision, taxonomists typifying old names had often chosen to disregard it. Clearing the ground by kicking out those words would give Nic Lughadha exactly what she wished.

Nic Lughadha still thought that the deletion would have little effect, but had no objections to it.

D. Ward asked whether the proposed deletion would result in a significant number of Linnaean names, typified by pre-Linnaean illustrations, becoming untypified and in need of typification.

Greuter explained that the contrary was true. If in a Linnaean protologue synonyms were cited that included illustrations, but there was also original material in the herbaria, then a strict interpretation of Art. 8.3 (an interpretation to which Nic Lughadha objected) would enforce the choice of a herbarium specimen, even though this might be disruptive. The proposed deletion would legalise, beyond possible doubt, the designation of the illustration as type.

D. Ward had no problem with that. He was concerned about the situation in which Linnaeus had no specimens, and a pre-Linnaean illustration to which he referred was the only indication of what he had in mind.

Greuter responded that in such a case no change would result. These illustrations could be selected as such even under the current Code, and could continue to serve as such.

Zander recognised that there was a problem but thought that the proposal did not address it. He did not think one could amend a proposition like that by coming up with something complicated. The proposal should be referred to a Special Committee that would address all the...
issues. This was a new proposal; the Section had not had a chance to think through its consequences.

**Marhold** strongly supported the proposed deletion. In a paper he had submitted to *Taxon* he was proposing the selection of an illustration as type, in a case where a specimen that would upset current usage also existed. Two reviewers, both members of the Editorial Committee, were in disagreement over the interpretation of Art. 8.3. Clarification was needed and could not await the next Congress.

The **partial deletion** of Art. 8.3 was **accepted**.

**Greuter** noted that deletion of what now remained of Art. 8.3 was more controversial. Opinions ranged from those supporting illustrations as types to those who would like to have iconotypes abolished altogether. In response to a query from the floor, he explained that the effect of the proposed deletion would be retroactive, it did not only concern future names. To prohibit future iconotypes might be a good move, but would be a different proposal. His concern was that the nomenclatural consequences of the proposed deletion were unclear. No one had tried to assess the presently accepted status of post-1957 names of which the type was stated to be an illustration, although a specimen could have been preserved. Under a normal interpretation of the current *Code*, these names were technically invalid. They had often but not always been considered not to be validly published. If the remaining paragraph were now to be deleted, they would all become valid. This would be a good thing if, but only if, they were now generally accepted as valid. No one knew which option was more destabilising: whether it was the loss of names now accepted [by enforcing the current rule] or the post-factum validation [consequent to the proposed deletion] of names that had been disregarded. Perhaps the decision should await further study.

**Brummitt** again pointed out that the word “only” did not appear in the Article. It only stated that if it was impossible to preserve a specimen as the type (and the difficulty of knowing what “impossible” meant had already been noted), the type might be an illustration. This was already known, because Art. 8.1 stated that a type might be a specimen or an illustration. What was left of Art. 8.3 was completely nonsensical.

**McNeill** conceded that in strict semantics Brummitt and Nic Lughadha might have a point. However, this was part of an article of the *Code*, of which the obvious purpose was to rule on something. The only possible way in which one could attach meaning to this provision was by imply-
ing the word “only”. A past Editorial Committee had combined the two previously distinct paragraphs into a single ill-phrased paragraph, obviously doing a sloppy job. The original two paragraphs, each in itself, had been perfectly clear, and they implied “only”. The Section should address the real issue of iconotypes, not the semantics of the wording.

Compère saw the weak point of the remaining text in the word “impossible”. It was always possible to preserve something, but in the microalgae, the preserved material often did not resemble the living specimen described and drawn by the phycologist. In such a case an illustration was more useful as a type than the actual material. Who did decide that it was “impossible” to preserve a specimen? The author or someone else?

Greuter noted that palaeontologists, who had a similar problem, felt exactly the reverse. They had made it mandatory to provide an illustration, but ruled out illustrations as types. Their type was always the specimen illustrated and not the illustration. Zoologists, too, had never accepted illustration as types. That was a botanical speciality.

Brummitt insisted on the absence of the word “only”. No court of law would ever interpret the wording of the article as if the word “only” were there. Furthermore, historically this had not been the intention. The original wording, which went back to the 1930s [it is found in Art. 18 or the Cambridge Rules: “Where permanent preservation of a specimen or preparation is impossible, the application of the name ... is determined by means of the original ... figure.”], when the type method was introduced, just stated what one had to do when there was no type specimen, and stated it so that it could be an illustration. To interpret the provision as if the word “only” was there was completely false.

Greuter disagreed. The original meaning was clear. Perhaps it had been made less clear by editorial action, but as McNeill had explained, logically the meaning could be one and only one. Discussants should better stay clear of semantics and speak to the facts.

Keil wanted the Section to consider defining illustrations, similar to what had just been done with specimen definition. An illustration could be of many parts. What if one had a series of excellent photographs of a plant from a remote place that was clearly an undescribed species, but had no way getting back to that locality: could one designate that entire series of photographs, put together as a plate, as a type illustration?

Prop. D, i.e. deletion of what remained of Art. 8.3, was rejected. A counted show of cards was requested: 189 : 349, plus a few abstentions.
Prop. E (6 : 186 : 10 : 5) was ruled as rejected.


Greuter noted the mildly negative mail vote. Traverse, the proposer, might have wished to speak to it – but he had left.

Chaloner guessed that Traverse, considering the negative mail vote, might have thought it was a lost cause. However [contrary to what the Rapporteur had inferred from that remark], Traverse should better be present when the matter was decided. Action should be postponed. Meanwhile, the Section should know that the Fossil Plants Committee’s views were open-ended, with equal votes in either direction.

J. Skog, Secretary of the Committee for Fossil Plants, had talked with Traverse. Her Committee was indeed clearly divided, partly because illustrations of fossil plant material could now be designated as epitypes, and also because the term “microfossil” itself was not clearly defined, e.g., as to whether or not it included megaspores. Traverse was resigned to work with that subterfuge for the next six years and see what the palaeontological community would decide. If the Section agreed to wait for Traverse to be present, that was perfectly appropriate.

[The end of the discussion and the vote took in fact place on Tuesday morning, at the beginning of the Third Session.]

Traverse explained the proposal, which addressed a serious problem in palaeopalynology. There were some 30,000 names of plant microfossils which needed types. For most of them, the holotypes that had been designated were individual microfossils among perhaps 5000 similar ones kept on a microscope slide. The slides, once deposited, were usually forgotten, or lost, or broken, and the specimens themselves were chemically alterable and did change over time. Finding the individual designated as holotype was extremely difficult, because its location on the slide was indicated by microscope co-ordinates referring to a microscope that might not any longer be available. Of the 30,000 types, perhaps as many as 29,000 existed only in theory, but no one would designate lectotypes or neotypes for these names. Allowing illustrations as types would be a practical solution. The objections that had been raised mostly concerned the wording and practicability, not the concept. Having failed to convince a majority of the Committee for Fossil Plants of the need for the proposal, he had decided to withdraw it and leave the matter with the Committee, for study before the next Congress.
Prop. F was withdrawn.

[Here the record reverts to the normal sequence of events.]


Greuter asked whether these belonged to the package of proposals on fossils on which action was being deferred. Chaloner answered “yes”.

J. Skog said “no”. [Laughter.] Prop. G and H were favoured without dissent by the Committee for Fossil Plants. Prop. G would clarify a rule that had long existed. Although since 1912 an illustration was required in order to validly publish the name of a new fossil plant taxon, it had never been specified that the illustration must be of the type, nor that it must be indicated which of perhaps several original illustrations was of the type. There had indeed been validating illustrations that were not of the type – a rather ludicrous situation. Regarding Prop. H, she could see that it pertained in part to valid publication, but to Art. 38, not Art. 37 as the Rapporteurs had suggested.

Greuter noted that the largest share of the mail vote was for “ed.c.”, indicating agreement with the proposal, but also with it being placed under Art. 38 rather than Art. 8. This was an editorial matter. Also, 22% of the votes on each proposal were for “Special Committee”, and that Committee favoured the proposals. In cases such as this, when provisions only concerned a special group, the Rapporteurs used to recommended acceptance if the pertinent Committee was in favour, and it was the habit of the Section to comply with the wishes and needs of those specialists. Did anyone have any concerns? [None were raised.]

Prop. G and Prop. H were both accepted.

[The following additional item, pertaining to Art. 8.3, was discussed during the Eighth Session on Thursday afternoon.]

Silva moved to introduce the term “iconotype” into the Code. The term “iconotypus” had been proposed about fifty years ago by Bohuslav Fott, a Czech freshwater phycologist. to designate an illustration that served as a type in lieu of a specimen. During the past fifty years, the term “iconotype” had come into general use among phycologists studying microalgae, but had been given two different meanings. About half of the authors used it, as originally intended, to indicate the type itself; the other half to indicate an illustration of the type specimen. The Committee for Algae believed that the time had come to introduce the term
into the *Code* in some appropriate place, clearly defined to mean the
type itself and not an illustration of the type.

**Barrie** was disturbed by the proposal because it seemed to reduce pre-
cision by not requiring authors to use the appropriate special term when
referring to a type. If an illustration were a holotype or a lectotype, it
should be called a holotype or lectotype. Calling it an iconotype would
blur the distinction.

**Greuter** (1) missed a clearly worded proposal, a difficulty that could be
surmounted. He (2) also missed a suggested placement, though one
could certainly be found. (3), he could not see why this should be solely
a phycological term, when illustrations serving as types were admissi-
ble in all groups except fossils; so advice from the Committee for Algae
alone was insufficient. And (4), though this might be a personal idio-
syncrasy, he felt that the term logically required a complementary term
to designate types that were specimens. Those familiar with Greek
might want to use the term “deigmatotype” – “deigma” being a speci-
men as far as he recalled. The proposal, coming from the floor, seemed
premature and unbalanced, and he was reluctant to recommend it.

**Gams** had understood that Silva’s main concern was to warn against
misuse of the term “iconotype” for an illustration of a type specimen.
This might be inserted into the *Code* as a footnote, in the place where
“an illustration may serve as a type” appeared.

**Silva** explained the proposal was to add “(iconotype)” at the end of Art.
8.3, after the words “… a type may be an illustration ...” The same
proposal had been made and defeated in Yokohama. Higher plant spe-
cialists did not have iconotypes in most protologues as phycologists did,
so they could not see the problem. When indexing, one had to find out
which element was intended to be the type: the figured culture or her-
barium specimen, or the figure itself. As some people used the term
iconotype loosely, this might be impossible to decide.

**Greuter** was reluctant to recommend adoption of a proposal that had
been defeated in Yokohama, when reintroduced from the floor in St
Louis. He recalled having heard another, possibly relevant, objection
from a phycologist: that if iconotype were explicitly defined as an il-
lustration serving as a type, someone misusing the term might thereafter
effect type designations without intending to do so.

**Silva’s motion** was withdrawn.

*[Here the record reverts to the normal sequence of events.]*
Recommendation 8A

Prop. A (82 : 108 : 18 : 3) was withdrawn by Barrie in view of the previous discussion on Art. 8 Prop. D.

Prop. B (133 : 35 : 44 : 2).
Barrie felt that the proposal was still relevant, despite the decision taken on Art. 8.3, and that it would lessen ambiguity.
Greuter recommended that it be referred to the Editorial Committee, to be dealt with in the light of what had been decided earlier on Art. 8.3.
Prop. B was referred to the Editorial Committee.

Greuter pointed out that the proposal covered new ground. He drew attention to the very substantial “yes” vote (84 %).
Barrie explained that the proposal followed from the new specimen definition that had been accepted earlier in the day. It encouraged people to label material appropriately (i.e. unambiguously) when it belonged to a single specimen but was mounted in different preparations. They were asked to make it clear that, for example, a fruit kept in a given drawer belonged with a specimen in a herbarium cabinet.
Prop. C was accepted.
THIRD SESSION
Tuesday, 27 July 1999, 09:00-12:00

Article 9

Prop. A (109: 6: 99: 1) and Prop. B (113: 8: 93: 1) were both referred to the Editorial Committee.

Prop. C (76: 46: 72: 12) was set aside and later [during the Fourth Session] withdrawn by Gams, following discussion among the mycologists and phycologists present.


Greuter apologised for careless reading of the proposed text by the Rapporteurs, resulting in their statement that it was merely an editorial improvement, and in consequent lack of opposition in the mail vote. The proposal would have been merely editorial if the sentence in the Code that presently read “if no isotype exists, the lectotype must be chosen from among the syntypes if such exist” had been maintained. Without this sentence, the proposal would actually do away with a traditional concept, that in lectotypification syntypes take precedence over other original material. He recommended that the proposal not be accepted but sent to the Editorial Committee, on the understanding that the improvements in wording would be used but no change in the meaning of the Article would be made.

Brummitt had been delighted to read the Rapporteurs’ comments and was sorry that they had discovered that the matter was not as simple as they had thought. The intention was to enable a lectotype to be chosen from among all original material, without giving precedence to syntypes as was presently the case. This would increase flexibility in lectotype designation. Perhaps by an historical accident, syntypes as presently defined could not be illustrations. Illustrations were often best suited as lectotypes, particularly for early names.

Greuter explained that the definition of syntypes, of long standing in the Code, was “specimens cited in the protologue”. In Linnaean works, actual specimens were rarely cited. Where specimens were referred to by Linnaeus, for lectotypification purposes these now took precedence
over illustrations and uncited material. The provision as it was now in the *Code* reflected current practice in type designation: even when uncited material or cited illustrations were known to exist, any cited specimens were given preference. He was all in favour of flexibility in typification, but warned against accepting the proposed change as it had not been generally understood.

**Reveal** preferred Prop. D to Prop. E. It allowed illustrations to be selected as types to preserve current usage, especially for Linnaean names where specimens often did not match usage and where illustrations had often formed the conceptual basis for new taxa (specimens came in later during the writing of, e.g., *Species plantarum*). There were many cases in which the only way to stabilise the current application of names was by preferring an illustration that Linnaeus had seen to any specimen he might have had at hand. Jarvis had mentioned the same problem the day before. The proposal would increase flexibility.

**W. Anderson** agreed that Prop. D and Prop. E should be discussed together. They attempted to achieve similar goals but could not logically be both approved. He was impressed by Prop. D because it would add some desirable flexibility to the lectotypification process. If there were hidden problems, he wished to know what they were.

**Greuter** agreed that Prop. D and Prop. E covered similar ground but were incompatible. Reveal’s comments were hardly relevant: Linnaeus cited syntypes only in a few exceptional cases, and when he did, the specimens he cited had been accepted as types.

**Reveal** objected that Linnaeus had visited and studied numerous herbaria, and that by citing the relevant phrase names he made reference to the specimens examined.

**Greuter** disagreed. Syntypes were specimens cited as such in the protologue. Citing literature related to specimens was not a reference to the actual specimens. Linnaeus did occasionally cite a specimen, one that he had received from, say, Löfing; other plants seen by Linnaeus, of which one knew indirectly through his synonymies, were just original material. The question raised by W. Anderson was pertinent, but it was hard to know whether the proposal would have negative side effects, as no one had checked. Lectotypifications made under the present *Code* and currently accepted might have to be replaced or reconsidered, but no one knew how many.
Gandhi wondered why Prop. D and Prop. E could not be merged into a single new concept for lectotypification.

Nicolson first made an historical point. The concept of syntypes as originally defined [in the Stockholm Code] was coextensive with that of original material. In the fairly recent past, “original material” had been redefined to include any element, cited or uncited, that could be connected with the protologue. Syntypes had meanwhile been defined [in the Montreal Code] as specimens actually cited and not just implied. That was the difference “between sin and original material”. [Laughter.] Second, Art. 9.9 was like a big grizzly bear, going on and on. The For\-\man & Brummitt proposal was short, direct, and to the point. Between the two, he currently favoured Prop. D.

McNeill asked the Committee on Lectotypification about the rationale for the wording of Prop. E. He was swayed on the issue, desiring flexibility in the selection of lectotypes, but being reluctant to see a change in traditional procedure well-rooted in botanists’ thinking: to go from the holotype to isotypes and then syntypes. Could the Rapporteur clarify his remark, that acceptance might cause a change in types previously selected under the current Code? Would not lectotypes already designated maintain their priority?

Greuter explained that currently accepted lectotypes, being cited specimens, would be overthrown when an earlier choice had been made of uncited original material. Less frequent but more disturbing would be cases of alleged conflict between the protologue and the presently selected element, which might raise endless debates. The present rule, while more restrictive than Prop. D, gave clear guidance. If specimens were cited in a protologue, a lectotype had to be chosen from among them in the first place. Such certainty would be eliminated by the proposal: any potential uncited specimens would also have to be considered, necessitating extensive herbarium studies and sleuthing for material that might or might not be original. The task of lectotypification would become heavier by an order of magnitude. Also, Prop. D would render the syntype notion redundant, as Art. 9.9 provided the only justification for recognising syntypes as a distinct category. Adoption might therefore lead to further, unforeseen reshuffling of Art. 9. That Article was complex and many-layered. He worried that the Section was facing a situation on which it had no proper guidance, as the real issue had not been properly considered before.
Reveal asked Brummitt to consider adding a Note: “Preference in selecting a lectotype should be given to syntypes and paratypes if available”.

Brummitt had found nothing in Greuter’s remarks to change his views. Prop. D offered more flexibility and did not dictate what one had to do. It did not require authors to look at more material. Reveal’s suggested addition would be acceptable as a Recommendation but not as a Note.

Faegri wondered whether the words “isosyntype” and “isoparatype” in Prop. E were important, or mere embellishments. If there was no syntype there could be no isosyntype. Unless there was some hidden meaning, the Editorial Committee should consider deletion of those terms.

Barrie explained Prop. E as an attempt by the Committee for Lectotypification to describe in a simple if admittedly lengthy provision all possible steps in lectotypification and to provide clear guidance. The term “isosyntypes” was currently in the Code, to allow the possibility of selecting a duplicate, not cited in the protologue, of a cited syntype. Paratypes had been added in the proposal as a logical consequence of a change made at the Tokyo Congress. Prior to that Congress, a paratype could be a specimen or an illustration, but in Yokohama “illustration” had been removed from the definition. Under Prop. E, as long as there were cited specimens, they would take precedence over any illustration. Only when faced with a pool of uncited specimens and illustrations, an author would be free to select either an illustration or a specimen, as appropriate. The Roxburgh example Brummitt had referred to on Monday would be a case in point, as Roxburgh probably never cited specimens, although he published and cited illustrations, so both would just be original material. If there were objections to introducing “isoparatypes”, which indeed expanded the possibilities considerably, they had been added to achieve full parallelism with the “syntype” clause, but they could be deleted without affecting the gist of the proposal.

W. Anderson had found Greuter’s comments on Prop. D very helpful and asked for similar comments on, or perhaps warnings against, Prop. E. That proposal was well laid out and provided clear guidance to those struggling to select a type. Isosyntype was an important notion to retain: when syntypes were missing or destroyed, there were often isosyntypes that the author had never seen but which could be good lectotypes.

Greuter answered that both proposals addressed the same issue but had different aims. Prop. D would allow greater flexibility at the cost of greater responsibility to be borne and greater complexities to be faced.
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by the lectotype designator. By Prop. E, the Committee for Lectotypification had produced a clear recipe for those who wanted to lectotypify names, so that they would know exactly the order of precedence in considering material at hand. First one would see if there were syntypes; if there were, the lectotype would have to be chosen from among them. If syntypes had been cited but none were extant, one would look for duplicates of these syntypes before proceeding to the next step, the paratypes. In the absence of paratypes, an author would then have to dig into the complexities of other original material. The two proposals were in effect incompatible. Prop. D provided for greater flexibility at the cost of more work; the step-by-step guidance of Prop. E was inappropriate for those who wanted maximum flexibility. One could not have flexibility and clear guidance at the same time. At present, the Code followed the step-by-step and not the flexibility route, and the latter would introduce a substantial change in the basic approach of the Code.

Bhattacharyya was opposed to Prop. D, because taxonomic literature did not indicate the location of Koenig specimens in the herbaria of Berlin, Copenhagen, Calcutta, or Kew; or of those of, say, Wallich, Voigt, and others, which were found in several herbaria.

Soreng felt that the two proposals were not totally misaligned. Prop. E could be reduced to a Recommendation, giving users guidelines. Anyone requiring more flexibility in a particular case could follow Prop. D. Was this appropriate as an amendment?

Barrie felt that reducing, essentially, the current Art. 9 to Recommendation status and replacing it with Prop. D would create confusion. There would then be an Article providing for a flexible approach while it was recommended that the step-by-step procedure be followed.

Demoulin advised that, when choosing between more flexibility and clearer rules, the Section take into consideration the aim of a more stable nomenclature. Flexibility was necessary for stability. Automatic procedures could result in unfortunate lectotypifications. He would therefore support Prop. D. In addition there should be a Recommendation, that inexperienced workers refrain from lectotypifying old names. Those undertaking that difficult task should know and follow the procedure described in Prop. E, but also what could and should be done to preserve current usages.

Keil explained that the reason why Prop. E would reduce the flexibility sometimes needed in lectotypification, was that it contained the word
“must”. In a Recommendation, “must” would be replaced by “should”. Such a Recommendation should be followed by a statement, that any departure from it ought to be fully justified. This would leave the flexibility occasionally to use an illustration, while the current Prop. E would dictate that illustration come last on the list.

Moore wondered what the case would be, under Prop. E, if there was no original material but there was an epitype. Would it not then be logical for the epitype to become the type?

Barrie pointed out that an epitype had no standing in the absence of a “principal” type.

Moore thought of a situation in which the type supported by the epitype had been lost or destroyed, and there was no other original material.

Barrie agreed that the epitype could then be switched into a neotype, but saw no need to legislate on that particular case.

Reveal moved an amendment to Prop. D, to add a new Recommendation: “Preference in selecting a lectotype should be given to syntypes, isosyntypes, paratypes, or isoparatypes (duplicates of paratypes), if extant.”

Barrie definitely favoured Prop. E over Prop. D, but if Prop. D were approved, a Recommendation based on Prop. E would be preferable, as it would indicate the sequence in which syntypes, isotypes, paratypes, etc., were to be considered, whereas Reveal’s amendment motion lumped them all together. However, if Prop. D were accepted then the traditional concept of “syntypes” would essentially be removed from the Code. [Reveal’s motion was however seconded.]

Voss was of the opinion that the word “should”, used in Reveal’s motion, was incompatible with a Recommendation as recommendations did not proscribe what an author should do.

Greuter explained that Reveal had moved an amendment to Prop. D. If the motion was carried, Reveal’s new Recommendation would become part of Prop. D. Would it not be wiser to uncouple these two items? Reveal’s amendment could be treated as a motion on its own, to be considered if Prop. D were carried. Reveal agreed to this suggestion. [As Prop. D was defeated, Reveal’s motion was technically withdrawn.]

McNeill foreshadowed a route forward. The two competing proposals really considered two quite different situations. Prop. D arose from Forman & Brummitt’s work with early typifications and was primarily concerned with 18th century names. Prop. E, the traditional stepwise
procedure, mainly addressed 19th and (pre-1958) 20th century situations, when people were out collecting and specimens were being cited. The flexibility in Prop. D would be advantageous so long as there was also something on the lines of, but not identical with, Reveal’s motion. Were Prop. D approved, the Section could then look at Prop. E, amending it by requesting the Editorial Committee to reword it as a Recommendation.

**Prop. D** was rejected on a card vote (357 : 312; 53 % in favour).

**Prop. E** (120 : 75 : 18 : 1).

**Barrie** [after action on Prop. F-I, taken while the count was conducted] resumed the discussion on Prop. E, stressing the need for clear guidelines for those who wanted to designate a lectotype. Some people had great experience in designating types, and might prefer flexibility, but those who were doing it infrequently, or for the first time, would appreciate the guidance that this proposal would provide.

**Pedley**’s one main difficulty with the proposal was with the word “must”. It would place a huge burden on those designating a lectotype, forcing them to chase around for all original material, an impossible task if one was sitting 12,000 miles from Europe where most of that material was kept. How could one know what material an author had studied if it was not referred to in the publication? He would be happy if “must” were changed to “may”.

**Barrie** pointed out that even currently, typifying authors were required to choose from among the material known to have been used by an author, whether cited or uncited, before designating a neotype. The “must” was already in the current rule. Before designating a lectotype, most typifying authors made as sincere an effort as they could to find all the original material relating to a name. Deciding what uncited material an author had studied was a matter of judgement on a case by case basis.

**Garnock-Jones** believed that it would remove some of Pedley’s difficulties if isosyntypes and isoparatypes were given less importance. Isosyntypes, which the author might not have seen, should not take precedence over paratypes. In Australasian material it was often difficult to establish whether a specimen was a duplicate or not. Otherwise, he supported the descriptive and helpful proposed wording.

**Soreng** felt the proposal would create problems unless a date were specified from which it would take effect. Making this retroactive could cause damage to past type designations.
Barrie pointed out that most of the proposed wording was already in the Code, and had been a rule in many previous editions.

Keil moved an amendment to Prop. E, so that its first sentence would read: “If no isotype, syntype or isosyntype (duplicate of syntype) is extant the lectotype must be chosen from among the paratypes, if such exist, unless it can be demonstrated that such a choice would destabilise the application of the name.”

Keil’s motion was seconded but defeated.

Greuter summed up the issues. Whether to say “must” or “is to” was a matter of taste, but “may” would be inappropriate because it implied a statement of fact that would not even deserve the status of a Note. If Prop. E were voted down, the Section would be stuck with what was now in the Code: the sequential precedence of holotype, isotypes, syntypes (specimens cited by the author), and isosyntypes (their duplicates, whether or not they had been seen by the author), over other original material. The only difference of substance was the newly introduced reference to paratypes and isoparatypes, which would give them precedence over other original material. The author of the name, by citing paratypes in addition to the holotype, had shown that he considered the latter to be of particular importance. It was therefore logical that, if the holotype was lost or destroyed, the lectotype should, in the first place, be taken from among the paratypes. Whether this should be extended to isoparatypes as well was less certain. It would add the complication that Pedley had raised, having to hunt for supplementary specimens in an unknown number of herbaria. The proposal would perhaps be even less controversial if the privileged status for isoparatypes were taken out.

Barrie had no problem with the suggested deletion if it was generally thought to be useful.

Whittemore considered isosyntypes and isoparatypes to be quite important in some circumstances. If a name had been lectotypified by a syntype or paratype that was later lost or destroyed, and usage based on that lectotype was well established, it should be possible to use an isolectotype to typify the name. Removing isosyntypes or isoparatypes from the progression would be undesirable, as some other element, that might not even be conspecific, might then have to be selected in preference to an isolectotype.
Korf wished to clarify that even if isoparatypes were removed from the list, this would not mean that one could not still use them, as they would be part of the remaining original material.

Greuter added that this was true only if the isoparatype had been seen by the original author [or, he should have added, if it was explicitly designated as a paratype in the protologue]. As there had been no formal motion to delete the word “isoparatypes”, it was still the original wording of the proposal that was being considered.

Prop. E was accepted.


Barrie pointed out that Note 3 was not only confusing but conflicted with the new specimen definition accepted the day before. Deletion of the Note would eliminate the problem.

Prop. F was accepted.


Barrie explained that the concept of epitypes had been introduced into the Code by the previous Congress, but the circumstances under which a problematic epitype could be replaced had been disputed, so the matter had been referred to the Committee on Lectotypification. Replacement of epitypes based on arbitrary taxonomic interpretations by different authors would eliminate the stabilising effect of the epitype concept. The Committee’s proposal would allow epitypes to be replaced or superseded only in conjunction with the type that it was supporting. A major concern of the Tokyo Congress had been, what would happen if it was later discovered, e.g., by applying some new technique, that the epitype and the supported type were taxonomically different? The answer was, the epitype could be replaced by a conserved type if this was necessary to prevent a change in the use of the name.

Greuter drew attention to the overwhelming “yes” vote on this and the two following proposals, which did all flesh out the epitype concept. This proved how much this concept was favoured by the user community, how quickly it had been accepted, and also that the proposed solution was considered acceptable among practising taxonomists.

W. Anderson agreed to Prop. G-I, but asked that the Editorial Committee correct the grammar in Prop. G: “apply” should be “applies”. This was accepted by Barrie as a friendly amendment. [Laughter.]
Prop. G was accepted.

Prop. H (202 : 8 : 6 : 0) was accepted.


Korf felt that Prop. I differed from the two preceding ones in a major respect. The Committee on Lectotypification had laboured under the delusion that an epitype, a term not even decided upon by the time the Tokyo Congress was completed, could be an illustration. Art. 9.7 had been essentially written by the Editorial Committee and had bothered him when he first saw it. A proposal to remove the statement that an epitype could be an illustration should have been made. When the idea of epitypes was accepted in Yokohama, everyone had thought of something incontrovertible that, contrary to an illustration, would serve the needs of modern taxonomy. Illustrations might be important, but to be illustrative of a taxon they did not need to be called types. The Code did permit illustrations as types. As epitypes, however, they were inappropriate. Prop. I referred to epitypes that were published illustrations: he would then want to designate a specimen as an epitype, to interpret that illustration, i.e., an epitype of an epitype. This was illogical. He could not understand how the Editorial Committee could have allowed it.

Greuter was sympathetic to Korf's concern, but could guarantee that “epitype illustrations” had not been an editorial addition. They must have been part of the original proposal, else they would never have entered the Tokyo Code. While it was indeed illogical to allow illustrations as epitypes, that was what had been accepted. Deletion of the words “and illustration” in Art. 9.7 would require a motion from the floor, which he would gladly second. If such a motion were carried, the second clause of Prop. I would as a matter of course be removed editorially, as it just reflected the fact that illustrations were presently allowed as epitypes. The two issues should be dissociated, and discussion ought to revert to Prop. I.

Demoulin (speaking to the not yet moved amendment, to kill it before it was moved [laughter]) conceded that the epitype concept had been introduced specifically to please people who did not want illustrations as types. However, there were fields other than the small macromycetes (e.g., discomycetes) with which Korf was concerned, organisms for which type illustrations were desirable, such as the plant microfossils mentioned earlier by Traverse.
Voss addressed Prop. I, which used the same phrase presently in Art. 9.14 and 37.5, requiring citation of the institution or herbarium in which types were conserved. How could this be enforced? What did the wording really mean? Authors might cite a herbarium months or years before the type was actually deposited in that institution. He had no answer to offer, but felt that this phrase, which was now proliferating in the Code, was unfortunate. Was the name invalid or the type designation ineffective until the specimen was actually inserted in the collection? Nobody ever checked whether the specimen was really there.

Greuter pointed out that the situation in lectotype designation differed from that addressed in Art. 37. When a lectotype was designated, an author would usually know where it was, unless he or she had not seen it. If so, it was just as well if the lectotype designation was not considered effective, as the author did not know what he or she was doing.

Prop. I was accepted.

[The following debate, which logically belongs here, took place on Tuesday afternoon at the beginning of the Fourth Session.]

Korf moved a new proposal from the floor: that in Art. 9.7 [defining “epitype”] the words “or illustration” be deleted. He had had a very spirited discussion with Demoulin, J. Skog, and others on problems with the preservation of types of names of fossil plants and algae, or of flagellates in general. During the break he had drafted a much more complex proposal, that would have excepted the fossils, but Greuter had convinced him that, under the present rules, epitype illustrations could not be used for fossils. Epitypes were a kind of type, and no illustrations were permitted to be types of names of plant fossils. Perhaps a special provision was needed for epitypes of names of fossils (there were already many wonderful special provisions regarding fossils, including the faculty to describe new species in English). The proposal now stood as a very simple one, to tie the epitype concept to specimens alone. [Korf’s motion was seconded.]

Demoulin recalled that, when the epitype proposal had been accepted in Yokohama, it was very deliberately phrased so as to take into account all kinds of needs concerning all kinds of organisms. It was true that with the kinds of fungi with which Korf worked there was no point in having epitypes that were not specimens. However, nothing in the present wording should cause problems to mycologists. If somebody
should be foolish enough to designate an illustration as epitype for a di-.
comycete, and mycologists considered that this did not fulfil their
need for something on which they could study characters, that designation
would be superseded by somebody else who would designate a
specimen instead. The reason why this provision mentioned illustrations
had nothing to do with fleshy fungi but with the groups in which illus-
trations had always been used as types: unicellular algae and fungi that
could not until now be cultured or preserved in a satisfactory way, e.g.,
euglenids and labyrinthuloid chytrids. Why not allow, e.g., that the last-
century holotype drawing of a Euglena name, drawn under the optical
microscope, be interpreted through a photographic plate made with a
transmission electron microscope and designated as epitype? Such a
practice did not interfere with that of mycologists working with fleshy
fungi. It was not appropriate to change what had been accepted in Yo-
kohama upon a proposal from the floor that had not been widely dis-
cussed, nor submitted to the mail vote.

Faegri added that with fossils, it happened that one had to destroy the
type specimen to study it, and the only thing left was the illustration. If
one could not use that, one was left rather naked on the floor.

Greuter considered the question of fossils a distinct issue. The fossil
specialists would have to come up with a solution of their own, perhaps
through the ad hoc Committee on Fossil Plant Proposals, if they really
needed an exception for epitypes to what was stated in Art. 8.4 and 8.5.
It would obviously serve the needs of some palaeobotanists if epitypes
of fossil plant names could be illustrations. But to achieve this, an ex-
plicit exception to the general rule, that fossil types must be specimens,
had to be proposed. The Korf motion had to be seen independently.
There was much to be said for tying names to types that were factual
material, providing tangible information. Much information one would
need in the future, if not yet now, could only be obtained on biological
matter. As Korf had aptly said earlier, one could not dissect an illus-
tration, nor sequence its DNA, nor look at its hidden parts. Demoulin’s
point was a serious one, but perhaps the emphasis was wrong. A Rec-
ommendation mirroring Rec. 8A.1, as presently in the Code, might be
the answer. It would read: “When a holotype, a lectotype or a neotype is
a specimen, the illustration or illustrations based upon that specimen
should be used to help determine the application of the name.”

Demoulin’s point, however, was about organisms of which one could
not keep a specimen, a naked flagellate that would burst. Many publica-
tions described what had been seen, and illustrated it by photographs, but nothing was left of the plant, as it could not be grown in culture either. These unicellular beings, algae and some fungi, always had been described and named based on an iconotype; there were no specimens.

**Barrie**, one of the authors of the original epitype proposal, confirmed that the inclusion of illustrations had been deliberate and opposed its deletion. An epitype did not have the same function as a holotype or neotype or lectotype, it was used to interpret a holotype, neotype or lectotype, serving as the nomenclatural tag. An epitype should be a specimen whenever possible, but there were not only whole groups but individual cases in which an illustration was the element best suited for the precise interpretation of an ambiguous type. That flexibility should be permitted.

**Greuter**, while still supportive of the proposal as such, advised the Section not to approve it, as it was not yet perceived as generally acceptable. Wider discussion was needed before it could be implemented. **Korf’s motion** was withdrawn.

*Here the record reverts to the normal sequence of events.*


**Barrie** explained that this was the first of two proposals that would force authors to make it explicit that they were newly designating a type. The phrase “type or equivalent” presently used in the *Code* made it difficult at times to know whether or not a typification had been effected. Worse, authors sometimes designated a type inadvertently, by using the general term “type”, which then [under Art. 7.11] was defined to mean “lectotype” or “neotype”. The proposal would require an author to actually use one of the latter terms to effect typification.

**Greuter** reminded the Section of a relevant comment by the Rapporteurs, in the Synopsis of proposals. If the proposal was accepted, the Editorial Committee would make it clear that the new provision was subordinate to Art. 9.8. This had been the intent of the Committee on Lectotypification, although the proposal did not spell it out.

**Prop. J** was accepted.


**Barrie** considered Prop. K to be even more to the point than the previous one. The use of “here designated” made explicit the author’s intent to designate a type.
Dorr wondered why the English phrase alone was used, when Prop. J referred to types in Latin in the first place. Should not "hic designatus" be included? That phrase could then be used regardless of the language of the rest of the paper. The Code should be as precise and specific here as it was elsewhere in providing Latin phrases to designate nomenclatural acts, e.g., describing a new species or proposing a new name.

Barrie admitted that the phrase "equivalent wording" perhaps did not fully cover that need.

Greuter pointed out that the point addressed by Prop. K was completely covered by the more far-reaching Art. 7 Prop. B, which had been adopted. Prop. K was now redundant and might better be withdrawn. Barrie agreed on behalf of the Committee on Lectotypification. Prop. K was withdrawn.


Brummitt explained that the proposal, which had been positively received, would cover the situation where a type had been designated simply as, e.g., Eklund 1234, without specifying the herbarium of deposit. As long as only a single specimen was known, that lectotypification was acceptable. But if 50 years later a duplicate was found elsewhere, it was no longer a lectotypification: a gathering represented in two different herbaria did not qualify as type, as a type was a single specimen. This was an impossible situation. The Rapporteurs had suggested addition of a phrase, which he was happy to accept. The Section should preferably accept the proposal so amended rather than referring it to the Editorial Committee.

Greuter, pointing out that the friendly amendment was now part of the proposal, agreed. The "ed.c." votes (supporting the amendment) and the "yes" votes taken together constituted a 78 % favourable majority. As amended, this was an important and welcome proposal to which there was little if anything to object. It would enforce what had been called "two-step lectotypification", a practice that sensible botanists had followed in the past although it was not mandated by the Code.

Jarvis feared that, particularly with respect to older names, adoption of this apparently innocuous proposal could cause some disruption. For Linnaean names, it was not uncommon for authors to state "type: LINN" or "type: herb. Hermann" without specifying a sheet, i.e., they specified not the collector but rather the herbarium. Such typifications...
often had been inadvertent but, while in the future they would have no standing, they might for the past have to be considered as type choices under Art. 7.11. Such choices could now be rejected as ineffective if they failed to specify one sheet from a number of possible contenders, and subsequent typification could still be made from amongst all original elements. The present proposal would restrict this freedom of choice. For example, the type of *Bromus giganteus*, the basionym of *Festuca gigantea*, had been stated in a publication statement to be in the Linnaean herbarium, where three possible sheets were present. Under the current *Code* that choice was ineffective, and a different sheet in Royen’s herbarium, which agreed with current usage and would maintain the application of the name, was eligible as lectotype. Prop. L would make the choice of one of the LINN sheets unavoidable, resulting in *B. giganteus* being the earliest legitimate name for *Vulpia geniculata*, and a conservation proposal would be the only means to maintain the accepted names of the two species involved. Without the proposed change, it would still be possible to follow the same typification procedure, as had been sensibly done in the past. By making the procedure mandatory, disruption to a significant number of old names would occur.

**Greuter** pointed out that this negative side effect was taken care of by the Rapporteurs’ amendment that was now part of the proposal. Different sheets in the Linnaean herbarium did certainly not represent a single gathering. The point raised, which was perfectly valid for the original proposal, had now become irrelevant.

**Prop. L** was accepted.


**Faegri** suggested that “described” be editorially replaced by “identified”. “Described”, a diffuse word, was better avoided.

**Greuter** noted the Rapporteurs’ comment, that the second sentence of Prop. M was incompatible with Prop. L, now adopted. The Rapporteurs had suggested that an “Editorial Committee” vote might express a preference for the first sentence alone, but the mail vote was negative. The second sentence should no longer be considered, but as the mail vote had expressed at least some support for the first sentence alone, this might still be debated.

**Trehane** wondered whether “effective publication” really meant “effective typification”.

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Greuter agreed that this was a straightforward error, which had been corrected by the Rapporteurs in their comments.

Reveal was concerned by the proposed wording: “Mere citation of the place of conservation of a type” (say US) “or the locale at which the type was collected” (say Utah). To effect lectotypification of the name of a plant that Palmer described while employed by the Smithsonian Institution, it was sufficient to indicate “Palmer 111 (US)”. If one wrote “LINN” and only one sheet was present, that was clear enough. Would one now have to state “Anonymous (LINN)” to effect lectotypification?

Voss wondered whether it was necessary to beat a dead horse. The proposal would need a starting date or it would create great chaos.

McNeill considered the proposal defective on a number of editorial counts. The Rapporteurs’ comments had already indicated that the context of the proposal was lectotypification not holotypification. Despite the proposal’s technical deficiencies it was basically a good idea, which he commended to the Section, as it did not seem that the various steps taken by the Committee for Lectotypification had totally dealt with the issue. The problem Reveal had raised was not real. If an author wrote “type in LINN” and did not cite the specimen number, there was no reference to a specimen, so this would not be effective lectotypification. He agreed with Voss that there had to be a starting date.

Barrie wondered whether the issue was covered by the revised Prop. L, with the now added “single gathering”.

Greuter agreed: this had been partly covered, but not completely. One still might write “type in LINN” without citing the concrete sheet, e.g., by its number, provided there was only one relevant specimen in LINN. Perhaps such statements had so far been accepted as effective type designations, but different authors would likely give different answers. Making the proposal effective from a specific date did not appear appropriate because it addressed accidental type designations of the past, not of the future. The question was whether the matter was of sufficient importance, and its consequences sufficiently clearly perceived, to make it advisable to adopt the proposal. The answer for him was “no”, not because of an objection of principle to the proposal but because its implications had not yet been sufficiently well considered.

W. Anderson endorsed Greuter’s comments. The second part of the proposal would cause great instability, and he was not sure that the first
part was sufficiently well expressed to be worthy of consideration. The whole proposal should be defeated at this time.

Brummitt took the opposite view, supporting the remarks of Voss and McNeill. If the proposal were completely retroactive it might well be disruptive, but if effective from a relatively recent starting date, say 1990 which was the date in Art. 9.14, the it would answer a real problem.

McVaugh feared some danger, even with a 1990 starting date. There were many 19th century publications in which an author had cited several collections in the protologue, one of which would have to become the lectotype. Would the proposal not affect those names?

Greuter noted that there had been a suggestion, but no formal motion, to add a starting date into the proposal. The suggestion had been of a date in the past. Therefore, little would be lost by waiting six years more and meanwhile devoting some supplementary thought to the matter.

Prop. M was rejected.

Prop. N (8 : 190 : 11 : 0) was ruled as rejected.

Art. 10

Prop. A (22 : 161 : 22 : 4), belonging to the deferred hybrid package, was eventually withdrawn (and referred to the Special Intercode Committee ICBN/ICNCP).

Prop. B (49 : 151 : 7 : 3).

Barrie considered the question of "mechanical typification" a fairly uncontroversial issue [laughter] that had been discussed for over 30 years. There were problems on both sides, and a decision had to be taken one way or another. Names had been retypified because a previous typification had been mechanical and was therefore superseded. Except for Linnaean generic names, for which there was a list, it was unknown how many names were involved and might be upset by this proposal. He knew of none, but he had not searched thoroughly.

Davidse had understood that a decision on the issue finally had been made at the Tokyo Congress. Upsetting it now would be a new flip-flop. Primarily for that reason he favoured voting down the proposal.

Barrie recollected that in Yokohama it had been made clear that this was a voted example and therefore the law. Prior to the Tokyo Congress it had been considered as any other example, some had followed it and some
had not. In Yokohama the issue had been resolved and one was now obligated to follow the example. The question was, did one really want to follow it or did one prefer flexibility. If the proposal was rejected, the proscription of mechanical typification would remain in the Code.

McNeill agreed with Davidse. The controversy had a long history, extending back much further than Tokyo. It had started in Seattle with a proposal from the Committee for Spermatophyta, consequent to Rol- lins’s discovery that a number of major changes in the usage of very familiar names, mostly in Brassicaceae (e.g., Nasturtium and Sisymbri- um), would ensue if the earliest typifications by Britton & Brown were followed. In a paper of his own [in Taxon 36: 350-401. 1987], which had considered only Linnaean generic names, a large number of parallel, similarly disruptive changes had been documented. After the Cambridge Congress in 1930, botanists had begun to adopt type designations in the “standard lists” that Hitchcock & Green had put together, which reflected the then current taxonomic usage rather than the earliest lectotypifications under the American Code. The provision had been in the Code ever since Seattle, only its wording had been changed in Lenin- grad or Sydney [in fact in Sydney, and also moved from Art. 8 to Art. 10 in the Tokyo Code]. There could be no question but that the rule had been interpreted differently by different authors. The Special Committee on Lectotypification that reported to the Sydney Congress had pointed out that if the rule was read logically it really meant that American Code lectotypifications had no standing, because they could always be superseded. The present Committee on Lectotypification was correct in stating that these type designations were not for the most part largely mechanical. But the Code had put a kind of ban on American Code lectotypifications since the Seattle Congress. This ban had been recognised for many names by many authors. To undo it now would be very disruptive, even though the current wording was defective and only the voted example really made it stick.

Zijlstra reiterated the point that, as the Rapporteurs had admitted, American Code lectotypifications were not really mechanical. The last step should now be taken and the phrase deleted. There were cases of lectotypification that were in fact mechanical although they were not based on the American Code. Many American Code typifications posed no problem, yet those made by Britton and Brown in 1913 were regularly neglected by some authors who accepted the same typifications when proposed ten years later by Britton and Rose. This was nonsense.
Brummitt could see good arguments both ways, but was inclined to favor deletion of this controversial provision, because of the difficulty of the wording which did cause problems, and even though it would add work for the Committees concerned with conservation proposals.

Demoulin also supported the proposal. The Code should be logical, and to preserve usage one could still use conservation. One should not forget that there were typification problems outside the phanerogams, e.g., in the fungi. His mentor in nomenclature, Donk, had shared that feeling. There were many typifications of fungal names that one might reject as having been made under the American Code, but were better accepted.

Barrie felt that the real problem was defining mechanical typification. The only criterion that had been considered was the automatic designation of the first species in order as the type. The precursor of the Hitchcock & Green Cambridge list was a 1924 paper by Hitchcock on the typification of the first hundred generic names in Linnaeus’s Species plantarum, in which it was stated that any species name with the epithet officinalis was to serve as the type of the corresponding generic name, without further consideration. That was arguably a mechanical way of selecting a type. If authors stated the way in which they selected types, that could be attacked, but without such an explanation, names could be drawn out of hat and no one could supersede the choice.

Greuter agreed that the provision now in the Code was awkward, as it must be interpreted through a “voted example” when in fact it should be spelled out in the Article that American Code lectotypifications were supersedable. Better still, one could revert to what an earlier Committee on Lectotypification had proposed: a starting date for generic typifications of 1929, the date of the Hitchcock & Green list, which would make it easy to know which typifications at the supraspecific levels were effective and which were not. Regrettably, such a proposal had not now been made. Acceptance of Prop. B would mean that from one Congress to the next opposite decisions were taken on the same matter, without any new arguments being produced, which would negatively affect the credibility of the Code. Nothing had been said now that had not been argued at the Tokyo Congress, where the Section had decided to leave the rule and the voted example unchanged. Afterwards, some had decided they did not like that provision and would not follow it, and they were now pretending it was not clear. This was not the appropriate way of dealing with provisions in the Code. For instance, a long
list of generic names had been proposed for conservation, but more than half of these proposals were known to be superfluous. Every name typified differently by Britton & Brown and Hitchcock & Green had been placed on the list, irrespective of whether it was now used in Hitchcock & Green’s or in Britton & Brown’s sense. Pichi Sermolli had made a search of early typifications of names of fern genera, of which a number had been first proposed by Underwood under the American Code, and they had all been disregarded. At least for pteridophytes, a reversal of the current position would be very destabilising.

Hawksworth reminded the Section of Art. 10 Ex. 5, dealing with an important lichen work, of which the type designations had not been accepted since the current rule was introduced. Acceptance of the proposal would result in the need for many new conservation proposals for generic names of lichen-forming fungi.

Whittemore objected to the proposal, which had the potential to change the application of names but had been put forward without a proper investigation of the magnitude of those changes. It was unknown what exactly would need to be done in terms of conservation or rejection to preserve the present usage of names. In the absence of a thorough study, the Section had no idea which decision would best stabilise nomenclature. It would therefore be premature to change the Code.

G. Yatskievych argued that Pichi Sermolli’s investigations had not led to nomenclatural stability. Superseding some of the older lectotypifications of Britton and Underwood had been destabilising. Notholaena comprised three discordant elements when described by Brown. Britton had typified it by an American element, but Pichi Sermolli had argued that the type was European and belonged to an entirely different genus. The result was that European and American floras used the name in different senses. As no stabilisation had so far been achieved, the mechanical designation provision should be eliminated from the Code.

Perry considered that the situation was unambiguous when it was clear that a type designation had been made under the American Code, but what when a typification had been accepted but was later found to have been effected under the American Code? That would also be destabilising. Better bite the bullet now and accept all typifications.

Gandhi pointed out that Britton & Brown had designated types for hundreds of generic names. As long as Art. 10.5(b) existed, there would be a lingering doubt on whether or not it was correct to follow their
designations. *Euthamia* Nutt. 1818 had two original species, *E. graminifolia* and *E. tenuifolia*. In 1913 Britton & Brown had designated the former as type, but in 1981 a monographer selected the latter, without referring to the previous designation. Should one now reject Britton & Brown’s or the more recent choice?

McNeill restated the opinion that he had voiced at the Sydney Congress. It was unsatisfactory to have a voted example that did not really reflect the wording of the rule. One would have to come to grips with the issue, which should be examined promptly and seriously in the next couple of years. The proposal of an earlier Special Committee, that priorability of lectotypifications should start from 1930, when the concept of type had come into the *Code*, was logical and might not be destabilising. No one had looked thoroughly at names other than Linnaean ones, at the sort of examples Gandhi had referred to. It was premature to make any change in the *Code* at this time.

Frodin supported McNeill's comments.

Voss pointed out that the proposal had been rejected by 72% of the mail vote. With 3% more “no” it would not even be discussed.

Zijlstra asked how far American *Code* typifications, now said to be mechanical, extended in time. Many typifications of the 1930s and 1940s, for example by Woodson, were American *Code* typifications, and no one had a problem with them. And how about the Britton & Rose typifications made in the 1920s? In her work for *Index nominum genericorum* she had seldom found such type designations to present problems. The problematic cases were mostly those in which Britton & Brown and Hitchcock & Green disagreed, which had now been considered by Jarvis’s Committee. There were some other cases with problems, but they were often due to other reasons.

Demoulin knew the mail vote was negative, but the Committee on Lectotypification, a Committee that had worked well and seen most of its proposals approved, first by the Rapporteurs and now by the Section, had voted unanimously in favour. The present proposal had been killed by the Rapporteurs, whose negative comments had influenced the mail vote. Faced with a choice between the Committee’s proposal and a mail vote influenced by the Rapporteurs, he would opt for the former.

Prop. B was rejected.
Greuter, bearing in mind McNeill’s earlier plea, moved that the appointment of a Special Committee to examine early generic lectotypifications, in all groups, be authorised.

Greuter’s motion was seconded and carried.

Upon a question by J. Skog, Greuter confirmed that the new Committee was also mandated to consider fossil plant genera. Anyway, a Special Committee had always some latitude in defining its own mandate.

[The following question arose at the very end of the last (Ninth) Session on Friday morning, amidst the votes of thanks.]

Zijlstra, to her great surprise, had discovered among the sign-up sheets for the future special committees one for a “Special Committee on the Lectotypification of Old Generic Names”. She had considered adding her name, but had no idea on whose initiative that committee was being established, or what it was intended to do.

Greuter replied that if she signed up and was appointed, she would find out. The initiative had been the Section’s, which had authorised the set-up of the Committee earlier that week.
FOURTH SESSION

Tuesday, 27 July 1999, 14:00-18:00

Article 11

[The following debates actually took place during the Seventh Session on Thursday morning, after those reported under Art. 3 Prop. A.]

Prop. A (47: 68: 15: 60) was withdrawn in favour of the following new proposal moved by J. Skog on behalf of the ad hoc Committee on Fossil Plant Proposals, which was also to replace Prop. G and Art. 59bis Prop. A [the full text had been distributed]: Modify the last sentence of Art. 11.1, to read: “However, the use of separate names for the form-taxa of fungi and for morphotaxa of fossil plants is allowed under Art. 3.4 and 59.5.”; and add a new paragraph: “Fossil taxa may be treated as morphotaxa which for nomenclatural purposes comprise only those parts, life-history stages or preservational states represented by the corresponding nomenclatural types. Names for morphotaxa, for purposes of priority, compete only with names based on a fossil type representing that same part, life history stage, or preservation state.” The two Examples of Prop. G (numbered 29 and 30 in the ‘Synopsis’), slightly reworded, were appended.

Greuter explained the new proposal. It was the longest and perhaps the most substantial of the whole fossil package. Replacement of “form-genera” by “morphotaxa” in Art. 11.1 was an editorial consequence of action previously taken on Art. 3. The important part was the new paragraph to be added to Art. 11. For palaeobotanists, taxa were not whole organisms, they were pollen grains of a given kind, or a given kind of flowers, or leaves, or wood, or bark, irrespective of whether or not in life they had belonged together. When it became known that they belonged together, they might do so in a reticulate pattern: there was no one-to-one congruence between leaf genera and flower genera and wood genera. This was the source of the problem, and called for an exemption from the normal operation of the priority rule. Was this sufficiently clear? and was the new proposal seconded? [It was.]

Dorr asked what the consequences were of uniting two taxa relating to different life history stages or preservation states. If their names did not
compete with each other, how would one choose? Was the first author to choose one or the other to be followed, just as for two names published simultaneously? This needed clarification.

Greuter explained that if two pollen species were being united, priority would apply. When a pollen species and a flower species, which would normally belong to different morphogenera, were discovered aggregated in the same fossil, there would be two options: either to say the fossil included flower X and pollen Y, or to give it a new generic and specific name. These would then belong to a different fossil category, flowers with pollen. [Palaeobotanists present confirmed this statement.]
The new proposal was accepted.

Prop. B (44 : 61 : 19 : 65) had, with the agreement of the proposers, been amended by the ad hoc Committee on Fossil Plant Proposals. It now concerned Art. 11.7, to be reworded as follows: “Names of fossil plants (Bacillariophyceae excepted) based on a non-fossil type are treated as having priority over names of the same rank based on a fossil (or subfossil) type.”

Greuter pointed out that this was a published proposal modified by a friendly amendment, so it did not require seconding. As one of the proposers, and in the absence of the specialist concerned, Fensome, he explained the proposal’s aim. There was now an exception in the Code to the general rule, that names based on a fossil type did not compete for priority with names based on a non-fossil type: the algae. An algal name based on a fossil type could, if it had priority, displace a name based on a non-fossil type. Now that fossil taxa had been defined to be morphotaxa, this was illogical. If a fossil leaf were to be considered to belong to an extant taxon, even if the name of the “leaf taxon” was older than the name of the extant taxon, there was no question of applying the name of the fossil leaf to the whole extant plant. Large algae, in which several organs fossilised independently, were in no way different. The same was true for many small algae, e.g., the dinophytes, which had life history stages (cysts) that were considered to form their own genera. In such cases, the current algal exception was problematic. Some old names of dinophyte fossils threatened younger names based on non-fossil types. The full story had been published in Taxon in the context of the Skog & Fensome proposals.

Examination of the list of generic names in current use [Regnum Veg. 129] had shown that fewer than a dozen were based on fossil types.
yet were currently used for extant plant genera. All were *Bacillariophyceae*, i.e., diatoms. Obviously diatoms, for which the border between fossil and non-fossil was very fluent (think of lake-bottom sedimentation; the term “subfossil” in the *Code*, presumably referred to diatoms), were the only reason for the current algal exception. A majority of the Committee for Algae obviously would be happy to assign responsibility for their fossil non-diatoms to the Committee for Fossils, which would be a logical, if implicit, consequence of the proposal.

A relevant example of the proposal’s consequences was *Tasmanites*, a fossil genus of “small bits” [meaning acritarchs; more correctly, *Prasinophyceae*] that went through all strata and ages beginning in the Silurian or Cambrian. *Tasmanites* was based on a type of very early age. As had been published [in *Taxon* 41: 529-531. 1992], and was now generally accepted if not yet proven, *Tasmanites* still survived as a prasinophyte genus, with a different, junior name [*Pachysphaera*] that had therefore been displaced by *Tasmanites*. This might appear as a straightforward name change; but what would happen if specialists of extant prasinophytes started looking at the protoplasm, using transmission or scanning electron microscopy, and found that their “beasts” belonged to, say, different subgenera or sections, or worse, different classes? How would they apply *Tasmanites*, when they could not examine the protoplasm of its type? This kind of problems would be avoided by narrowing the exception presently in the *Code*, from algae to *Bacillariophyceae*.

Compère reported that the Committee for Algae had voted 12: 2 for limiting the exception to *Bacillariophyceae*, or diatoms.

Silva did not object to restricting the ability of fossil names to overthrow extant names, though he felt sorry as he was responsible for Boalch & Guy-Ohlson’s adopting *Tasmanites* for *Pachysphaera*. However, he simply had been following the *Code*. The argument that names of extant genera of dinoflagellates would be endangered was a red herring: almost all of the common extant genera, but only one genus of fossil cysts, *Spiniferites*, had been named in the 19th century.

Demoulin asked whether there were any problems with the numerous calcified red and green macroalgae. Also, he objected to the proposed amended wording, in which the term “diatoms” had been replaced by *Bacillariophyceae*. True, the latter name was used in the Appendices of the *Code*. However, some people used *Diatomophyceae*, and were
entitled to do so as priority was not mandatory at higher ranks. Some
day, one might even raise the *Centricae* and *Pennatae* to the rank of
classes. “Diatom” was a term that everyone understood. While use of
the formal name was appropriate for the Appendices, the body of the
*Code* should stick to “diatoms”.

**Greuter** thought that the Committee for Algae might advise the Edito-
rial Committee on which term was most appropriate. He did not know
the calcified algae, but could only repeat that, outside of the diatoms,
there was no name based on a fossil type that was in current use for an
extant genus. He did not know about the species level – but even there,
if synonymy should occur, letting the non-fossil name prevail might be
beneficial. There might be examples in the dinophytes.

**J. Skog** believed that the *Tuberculodinium* example (Ex. 30 of the
“Synopsis”) was a case where the name of a fossil dinoflagellate cyst
would usurp the name in current use. [In actual fact the relevant exam-
ple, cited in *Taxon* 46: 559-560. 1997, is *Spiniferites* vs. *Gonyaulax.*]

**Prop. B** was accepted as amended.

[Here the record reverts to the normal sequence of events.]

**Prop. C** (7: 204 : 5 : 2) was ruled as rejected, as **Zijlstra’s motion** to
postpone action until after the discussion of proposals on Art. 15 was
not seconded.


**Greuter** noted that the mail vote supported the proposal itself and, more
decidedly, the Rapporteurs’ statement that it was largely editorial. If it
was accepted, the Editorial Committee would take care of the wording.

**Brummitt** disagreed that the matter was purely editorial, but did not
want to argue as long as the proposal went through. It should not
merely be referred to the Editorial Committee, as it addressed a situa-
tion not covered in the *Code*, which, nonsensically, appeared to require
the simultaneous publication of two homonyms.

**Voss** made a suggestion to the Editorial Committee [which, being spo-
ken off the microphone, was unfortunately lost].

**Greuter** added that the Editorial Committee would, at the request of one
of the proposers, correct ‘candelleanum’ to *candollei* in the Example.

**Hawksworth** had come across several similar cases. The proposal
reflected current practice but, while the example might be useful, the
value of spelling this out explicitly as a Note was not obvious.
Greuter agreed that the question here addressed had a straightforward answer, but to reach the conclusion to which Brummitt, he himself, and others had come independently one had to fit together provisions scattered throughout the Code, as that particular case was not concretely mentioned. It was useful to spell this out, as the proposal would, but phrases like “in rare cases” were less useful. The Editorial Committee would as a matter of course make the wording more stringent. Whether the proposal was accepted or referred to the Editorial Committee was irrelevant, the result would be exactly the same.

Jansonius felt that this was a simple and common-sense situation. He wondered how anyone could go astray and do something else than what was given in the example. What other solution could there possibly be?

Greuter explained that alas, yes, there had been such a case, which had been the reason for the proposal. The author in question apparently still maintained that he was right in rejecting Sedum candollei as illegitimate and proposing a replacement name [S. candolleanum] for it.

Prop. D was accepted.


Greuter drew attention to the 44% “ed.c.” vote, the largest share [he thought], which had been suggested by the Rapporteurs to bear a special meaning. Rather than accepting the very complex new text, that even the Rapporteurs had failed to fully understand, an “ed.c.” vote was to favour an editorial improvement of the wording in Art. 26.3, along the lines suggested by the Rapporteurs. The best course would probably be to vote down Prop. E as presented, on the understanding that the Editorial Committee would look after the wording of Art. 26.3.

Gereau respectfully observed that 48% was larger than 44%, and Greuter apologised for having mixed his notes.

Prop. E was rejected.


Hawksworth explained that the International Committee on Bionomenclature was responsible for the wording of the proposed provision, which addressed problems with naming protists that could be treated under more than one Code, so-called ambiregnal organisms. The new paragraph would also cover cyanobacteria. Therefore, it mentioned both the bacteriological and the zoological Codes. At present, de-
pending on whether a taxonomist considered an organism as an animal or a "plant", applying the corresponding Code could result in different names. The proposal, that arose from bacteriologists and zoologists as well as botanists, would remedy this situation by offering a satisfactory solution. D. J. Patterson from Sydney would speak on this subject the following week at the Symposium on Bionomenclature.

Greuter, without being a specialist in the groups concerned, realised that there were serious problems with names of organisms that were treated under different Codes by different authors. The proposed solution would lessen the urgency for a common BioCode. [Laughter.]

Eckenwalder cautioned against the danger of introducing tautonyms into botanical nomenclature. Would tautonyms be accepted if a name came from under the zoological Code?

Hawksworth replied that the proposal covered that point. A tautonym would not be accepted, as it was contrary to the botanical Code and thus was not "acceptable under any Code". The Committee on Bionomenclature hoped that the same wording would also be included in the zoological and bacteriological Codes.

Davidse asked whether similar proposals were being considered for inclusion in the other Codes. This would be a logical way to handle the situation.

Hawksworth explained that revision of each Code was operated on a different cycle. The current provision would not be in the forthcoming edition of the zoological Code, as it had been developed too late for being considered for inclusion. Bacteriologists would meet in Sydney the following month; they would presumably discuss the issue there, and were likely to support it.

W. Anderson read the proposal to imply that tautonyms accepted in zoology would be acceptable in botany unless they were homonyms. There seemed to be a built-in conflict with other rules in the Code. Perhaps "any Code" should be changed to "any and all Codes", in which case he would have no problem with the proposal.

Hawksworth answered that the intention was clear: a name had to comply with the criteria of both Codes under which it had been treated. If the wording caused problems, it would have to be clarified. He was happy to accept W. Anderson's suggestion as a friendly amendment.

Demoulin urged that the proposal be considered irrespective of what there was and would be in the other Codes. The problem had long been
recognised for the algae and had led to a special provision for them [in Art. 45.5], but now turned out to affect other organisms as well. For example, *Pneumocystis carinii*, a deadly pathogenic organism that until recently had been considered a protozoan and dealt with under the zoological *Code*, had been shown through DNA sequencing to be an ascomycete and now fell under botanical jurisdiction. It would be extremely irresponsible to change that name if it should happen to satisfy the zoological but not the botanical rules. What was really needed was that the exception presently applying only to the algae be expanded to cover all kinds of protists that had been flip-flopping between *Codes*.

**Greuter** saw a difficulty with the friendly amendment. The phrase “any *Code*” tried to cover two different situations. Regarding tautonymy, it indeed meant “all *Codes*”, but in other respects “any *Code*” was appropriate. Post-1935 zoological names had hardly ever been published with a Latin diagnosis and were not acceptable under “all *Codes*”.

**Voss** preferred the wording in Art. 45.5 which, he believed, already allowed an organism classified as an alga to bear a tautonym if so published originally under the zoological *Code*. This was not a new issue.

**Compère** confirmed this view. Under Art. 45.5, an algal name “need satisfy only the requirements of the pertinent non-botanical *Code*”. He knew of no concrete example but was not shocked by the thought.

**Greuter** felt this did open a can of worms. There appeared to be a conflict between two paragraphs in the *Code* [Art. 45.5 and 23.4] that had not been recognised previously. The Editorial Committee might consider inserting a reference under the tautonym rule, to acknowledge the exception of Art. 45.5. In Prop. F, the original wording, with “any *Code*”, should be restored. [This was agreed.]

**Prop. F** was rejected.

**Prop. G** (35 : 75 : 12 : 67) was withdrawn (see above, under Prop. A).

**Article 14**

**Greuter** suggested that Prop. D, then C, be considered first, as they would entail the most important change. [For clarity, the normal sequence has been restored.]

**Prop. A** (129 : 78 : 8 : 0).
Greuter [after Prop. C and D had been rejected] explained that Prop. A would bring about an improvement in wording, providing more flexibility for permanent committees to act on conservation or rejection proposals in specific cases.

Prop. A was accepted.

Prop. B (34 : 163 : 14 : 1) was ruled as rejected.

Prop. C (52 : 147 : 13 : 1)

Greuter noted that Prop. C was virtually coextensive with Prop. D [meanwhile rejected] but slightly less precise. Logically, it should be acted on in the same way.

Prop. C was rejected.


Greuter explained that this proposal, which would remove any limitation of rank for conserved names and thus extend the faculty to conserve to all ranks of family and below; was almost the same as Prop. C, but was more explicit by also spelling out contingent changes in some other places of the Code.

Nic Lughadha explained her proposal, which aimed to bring conservation procedures into line with those for rejection by permitting any name to be proposed for conservation, not merely names of families, genera and species as was presently the case, but also infrafamilial, infrageneric, and infraspecific names. The idea was not entirely new, as before the introduction of the conservation of species names, it had been possible to propose the conservation of names of subdivisions of families. As far as the proposers had been able to establish, removal of those ranks was an accidental side-effect of the rewording of Art. 14.1 to include names of species, and did not seem to have been discussed by any Nomenclature Section. [In fact, the deletion had been deliberately proposed in the context of Art. 14 Prop. D adopted at Sydney.] At present, botanists who wanted to preserve the current usage of names at ranks other than family, genus, and species were limited to proposals of rejection of competing names, often a less appropriate solution than conservation. There could be no question of the proposal "opening the floodgates" and creating a great burden for committees, as a relatively small number of names was concerned. Conservation of names of subdivisions of families had been possible for 30 years, between Stockholm and
Sydney, yet she was not aware of any such proposal. [There had in fact been six, none successful: see Nicolson in *Taxon* 49: 547-552. 2000.] However, some familiar names of subdivisions of families and of infraspecific taxa of economic importance were in need of conservation.

**W. Anderson** was opposed to any extension of conservation. As he served on one Permanent Committee, he knew how many names were being conserved and considered that the conservation procedure had got out of control. It should be cut back rather than extended.

**Brummitt**, whose views on conservation usually coincided closely with W. Anderson’s, did not agree with him on this occasion. There were occasional names at ranks subordinate to family, or genus, or species, that were widely known in the user community. For these, Committees should be given the power of evaluating relevant conservation proposals. While in support of the proposal, he also sympathised with W. Anderson’s concern. If it were accepted this should not be seen as an invitation to conserve any name at the secondary ranks. The Committee for Spermatophyta, on which they both served, would predictably be very selective about proposals at these ranks. Yet there were cases where the impossibility to conserve a name was unfortunate; and, as Nic Lughadha had said, it was an anomalous historical accident that one could reject names at all these ranks but not conserve them.

**Gams** observed that if a varietal name was later raised to the rank of species it could already now be conserved. He therefore questioned the need for the proposed change.

**Greuter** agreed that such cases existed, but there appeared to be names of varieties that had never been raised to species rank, and also tribal or subfamily names, that were important enough to warrant conservation.

**Frodin** favoured the proposal. There were many well established names of subdivisions of genera whose conservation would improve nomenclatural stability in large genera. In *Eucalyptus*, Johnson had despaired of resolving infrageneric nomenclature and had proposed a new set of names. This did not catch on, but neither was there any procedure to stabilise the old, familiar names by conservation. The proposal needed amendment to make it clear that it mainly concerned ranks between genus and species, not the infraspecific ranks to which Gams had referred.

**Reveal** moved an amendment to the proposal, to replace “nomenclature” by “the nomenclature of taxa at the ranks of family and below”. This would make explicit that only ranks at which priority applied were meant.
Nic Lughadha accepted this as a friendly amendment.

Prop. D was first apparently approved on a show of cards (300 : 196; 60.5 % in favour), then rejected on a card vote (333 : 350; 48.75 % in favour).

Answering a complaint by Dorr, who objected to the fact that abstentions were disregarded in majority assessment when in the mail vote they had been counted, Barrie noted that mail vote abstentions had been deducted before the tabulated percentage figures were calculated. He later explained the difference between the two votes by the fact that some had not held up their cards long enough for them to be counted. In future, cards of different colours should better be counted separately.


[The following two additional items, pertaining to Art. 14 Note 1, were discussed during the Eighth Session on Thursday afternoon.]

Reveal moved deletion of Art. 14 footnote 2, which had been introduced by the Tokyo Congress in order to maintain the current bibliographic references for family names listed in App. IIB. These had been retained because of the uncertain fate of the Names in Current Use proposals and in view of the stabilisation that they would have offered to bibliographic references. Inasmuch as this Congress had not approved NCU, he urged for support of his motion. [The motion was seconded.]

Greuter offered background information. In Yokohama, after a rather interesting alternation of events, of which the details could be read in the Proceedings, this footnote had been introduced as a reaction to a list prepared by Jim Reveal and presented in his absence. According to that list, many entries of family names of Spermatophyta in App. II were technically incorrect because there was an earlier place of publication, and often a different author, for the conserved name. As the Code did not contain any provision enabling the conservation of authorship, or place or date of publication, of a conserved name, the Editorial Committee would have had the task of altering the authorships and places and dates of publication of well over one hundred family names of Spermatophyta listed in App. IIB, with (1) the inherent risk that some date shifts might cause name changes under broad family concepts (when the relative priority of the conserved names of two or three segregate families would suddenly have change). Also (2), it was felt that the
exploration of old literature for family names was still in a state of flux; this had since improved had hardly yet come to an end. This would have been a purely academic exercise. Botanists knew what family names to use, as they had stood for many years in App. II, with their authorship, date, etc., and nothing would be gained by altering these data simply because new evidence out of old literature had turned up. The situation had not changed substantially since Yokohama. He therefore moved an amendment to Reveal’s motion: that the footnote should remain as it stood but with the word “temporarily” deleted, so that the present exception to the rule would remain. [Greuter’s motion was seconded.]

Reveal agreed that in 1993, when the question was put to the Section on behalf of the late Ru Hoogland and himself, the situation was uncertain. They had then requested that a starting date of 1789 be established for family names, which had been turned down. The 122 family names then listed as having incorrect entries increased to 186 after the names published prior to 1789 had been evaluated. A list of necessary changes had been published the previous year [in Taxon 47: 851-856. 1998]. Two years had passed since he had last found an earlier publication date or a different authorship for a name listed in the revised App. IIB that he had submitted to the Rapporteur. Greuter was correct in stating that, in 1993, there had been concern over the possible need for “superconservation”: statements in App. IIB such as: “If Abietaceae (1820) are united with Pinaceae (1836), the name Pinaceae must be used.” He had gone through all presently used systems of classification for possible instances where additional “superconservation” would be needed, and had found three such cases, but two cases in which current “superconservations” could be eliminated.

Burdet asked speakers to address Greuter’s amendment motion.

Brummitt, instead, supported Reveal’s original proposal. There were now two lists of [Spermatophyta] family names: one was as bibliographically correct as far as Reveal after very deep researches could tell, the other was in App. IIB of the Code. This was a serious problem for everyone who took family names seriously. The two lists should combined by incorporating Reveal’s bibliographic results into App. IIB.

McNeill, definitely speaking to the amendment, supported it very strongly. By accepting Reveal’s suggestion, the Section would make nonsense out of the action of the 1959 Congress, for which Bullock, one of Brummitt’s predecessors, had established the present list of
conserved names. True, Reveal’s thorough work had led to the discovery of a substantial number of earlier places of publication. But after all, the present list of conserved names had been accepted as correct and relied upon for the past 40 years. Did the Section really wish to go back and change a list that it had tacitly endorsed for 40 years, simply because there were some bibliographic errors in it? Did it want to show that it changed its mind for no good reason? Removing the word “temporarily” would confirm the present exception in the Code and leave botanists with a single list, the one that had been in the Code since 1959.

Demoulin noted the case of Tricholomataceae in App. IIA, with a one-and-a-half pages list of rejected names that had required extensive bibliographical work, as an example of the complexity of family nomenclature. In view of that complexity, McNeill was probably right in arguing for keeping the list incorrect but stable.

Furnari noted that if “temporarily” were removed, “Tokyo” would have to be changed to “St Louis” – which Greuter acknowledged as an editorial suggestion.

Zijlstra favoured the motion. Deleting “temporarily” might be a first step toward a future rule, presently still premature, permitting conservation of authorship and place of publication under the Code.

Greuter, upon a question from Magill, explained that his was a motion to amend Reveal’s motion, not accepted as a friendly amendment.

Gereau opposed the amendment and supported the original motion. Correcting the dates and places of publication of family names had been a tremendous job, done by a very scholarly person. Was the Section to enshrine the mistakes of the past or to gratefully accept the present results of Reveal’s careful scholarship?

Greuter’s motion was defeated.

Reveal’s motion was carried.

Greuter asked for advice from the Committee for Spermatophyta. If Reveal’s changes were implemented in the next edition of the Code, the necessary new “superconservations” would not be included, because they had first to go through the committees. Assuming Reveal had forgotten none, four additional “superconservations” were needed, for Adoxaceae, Caryophyllaceae, Cordiaceae, and Sapindaceae. How should they be handled?
Reveal promised to provide the information immediately to the Committee for Spermatophyta, which, he trusted, would act prior to publication of the *St Louis Code*.

Greuter noted that the times when it took three years to publish a new Code were long past. Before a conservation entry could appear in the Code it was necessary to publish a proposal in *Taxon*, vote on it in the Committee for Spermatophyta, publish that Committee’s report in *Taxon*, and have the General Committee approve that report.

Reveal replied that Committee for Spermatophyta voted quite rapidly on many matters, often without a corresponding published proposal. The requirement of prior publication of proposals in *Taxon* would be a de novo rule imposed by the Rapporteur.

Greuter pointed out that conservation proposals [under Art. 14.12] had to be submitted to the General Committee, which by common consent was effected by publication in *Taxon*, to be then referred them for examination to the Permanent Committee concerned, which in turn published its conclusions in *Taxon*, upon which the General Committee took action. He was not aware of any case where this had taken less than two years – which was very quick.

Brummitt sought information on mechanisms by which names had been listed in the Code before they were ratified by a Congress. What was the time schedule for getting out the next Code?

Greuter explained that such names had indeed been listed, but not before the decision had been ratified by the General Committee [see Art. 14.14]. Those names that had been approved by the General Committee between the Congress and publication of the Code were asterisked. There were none in the purple Code, but there had been in previous Codes. Both the last Code and the one before had been published within one year.

Brummitt thought that the Committee for Spermatophyta could process the proposals simultaneously with their publication in *Taxon*, reach a decision and obtain the General Committee’s approval within that time frame. As the Committee’s Secretary he would certainly do his best.

Reveal introduced a second motion from the floor: “The Editorial Committee is instructed to make all known bibliographic corrections to the text of the Code and the appendices of the Code, so as to reflect current knowledge.” This motion, if adopted, would imply that Section
members, and the whole taxonomic community, was asked to actively examine the Code and its Appendices, for bibliographic correctness. The wording was carefully chosen, the word “known” meaning that the Editorial Committee was to make changes only if the errors were made known to it. [Reveal’s second motion was seconded.]

Greuter was utterly surprised at the motion, as it implied that in the past the Editorial Committee had not done its job. The motion described the Committee’s duties. As far as he was aware, and as long as he had been responsible for it, the Editorial Committee had done exactly that, and a lot more. If Reveal wanted to tell the Editorial Committee it had not done its job, he might do so in the first place and then have his motion passed. If the Section felt the Committee had tried its best, then would it please vote down the motion, as it was rather insulting.

Reveal demurred. The proposal did not intend to reflect poorly on the Rapporteur, or any member of the Editorial Committee, or any past edition of the Code. It was a request that those engaged in the evaluation of names for nomenclatural purposes look at the Code and the bibliographic references therein and, if there were errors, inform the Editorial Committee of them. It was hardly a criticism; it was a request to assist the efforts of the Editorial Committee in doing the best job possible.

Greuter thought his English must be very poor if he were to interpret the proposal in that way. The instructions, as worded, were clearly addressed to the Editorial Committee.

Demoulin mentioned the change of the starting date for fungal names by the Sydney Congress as an example of how the Editorial Committee worked. After the Congress, the Editorial Committee, in consultation with the Committee for Fungi, had changed all fungal entries to make them agree with the new rule. It had been a lot of hard work but, he agreed with the Rapporteur; for the Editorial Committee it had been standard practice. The motion was superfluous. He had abstained on the main issue, whether or not there should be an exception to the general principle in the case of family names of phanerogams. Now that this was decided, was he to understand that the Committee for Spermatophyta considered that the decision taken implied too much work?

Egorova thought it quite impossible to correct all entries in the Appendices for the next Code. This was very difficult work.

P. Stevens agreed that the proposal was wrongly phrased. If one were to make a motion such as Reveal had explained it, it would have to
read, perhaps, “The membership of IAPT is instructed to make all needed bibliographic corrections known to the Editorial Committee.”

Stuessy thought that the addressee should not be the “membership of IAPT”, but rather the “members of the Nomenclature Section” or “interested taxonomists”. Did Reveal intend to have this inserted in Division III of the Code?

Reveal replied that the motion was of a resolution, to be printed in the Proceedings of the Nomenclature Section, not of a provision to be entered into the Code.

Stuessy, in that case, tended to agree with P. Stevens; or rather, one might better simply say that the Editorial Committee make all necessary bibliographic corrections; and if so, he really could not see what Reveal was trying to achieve.

Reveal insisted that he was trying to get people to contribute their knowledge, to point out the errors of which they were aware to the Editorial Committee so that they might be corrected in the next edition of the Code. That was all.

Greuter advised to leave it at that. Presumably, before the proceedings of the meeting were published the Code would be on Reveal’s desk.

C. Anderson thought the whole discussion ridiculous. She agreed with the Rapporteur: stop talking and vote the motion down.

Reveal’s second motion was withdrawn.

[Here the record reverts to the normal sequence of events.]

**Article 15**

**Prop. A (35 : 162 : 18 : 4).**

Greuter introduced the once famous, controversial cause of Names of Current Use (NCUs). The issue had been amply debated before, during, and after the Tokyo Congress, but as many present were attending a Nomenclature Section for the first time, some background information was needed. The NCU concept went back to a bright idea of Brummitt, who in principle was still happy with it although he was worried by the proposed implementation. The issue was relevant to the stability and maintenance of names. It was conceptually linked with conservation and, even more intimately, with sanctioning. The basic difference between protection of names on stabilised lists, as proposed, and the
present conservation of individual names was that the latter was a therapy, to be applied when one discovered a problem, but the former was prophylactics that would cure the problem before it arose. The procedure envisaged for the establishment of an NCU list was similar to that used in monographic work: establishing the names that were correct under the Code and resorting to conservation and rejection when the results of one’s searches would otherwise be destabilising. Now, once one had done this it would be accepted for a while, but the next monographer would have to redo the same work all over again. The new idea was that, once such a list had been carefully prepared (not hastily and sloppily prepared, as some had feared), future redundant work should be eliminated, and the listed names be stabilised. The NCU principle, as proposed, was an enabling provision: it would enable the General Committee to recommend the adoption of lists of protected names to a forthcoming Congress, lists that groups of specialists would have worked out and which the appropriate Permanent Committee would have scrutinised. Once such a recommendation were made, the maintenance of the listed names as protected would be authorised in the same way as was now the case for names to be conserved. There was a close analogy between the present conservation procedure and the proposed protection of listed names. However, the novel concept would work with whole lists of names, and it also offered options that conservation did not offer: it would not only establish the legitimacy, type, and if relevant the spelling and gender of the listed names, but could also fix, once and for all, date and place of valid publication as well as authorship. Conservation could not fix the valid publication of a name, or could it fix its date or authorship; yet these parameters, even for conserved names, were often in doubt and open to dispute. Also, conservation was only envisaged, on a case-by-case basis, when an actual need arose. This described in essence, though not exhaustively, what was being proposed.

Funk referred to the 74 % negative mail vote on the proposal. This had been debated and debated, and discussed and discussed, and there was no need for this to continue. The Section should just vote. She called for the question. [Applause.]

The question (calling for a vote without further discussion) was approved, and Prop. A was rejected.

[The following motion, relevant in the context of Art. 11 Prop. C and Art. 15 Prop. A, was discussed during the Ninth Session on Friday morning.]
Stuessy, at the risk of trying the Section’s patience and tolerance, moved to establish one more committee, a Special Committee on the Restriction of Past Names. There had been two sets of proposals addressing the problem: the NCU proposals, with which he had always felt uncomfortable, and a set of proposals from Zijlstra. The former attempted to sanction names in current use; the latter to exclude names not in use. When Vienna botanists had worked through the proposals, he had been taken by the deeply felt positive attitude of his colleagues toward the NCU proposals. As unfortunately they were not present and he had been the one to represent the Institute and cast the vote on NCU, their views had not been adequately expressed.

When consulting the Institute’s library, he had been amazed at the number of local and regional Central European floras and journals, dating back centuries, that he had never heard of before. Central European literature was truly profound in comparison with North American literature. His colleagues saw this as a serious problem, and felt that NCU would be a step in the right direction. Perhaps, however, there might be a better solution, yet to be discovered, apart from doing nothing and let time, conservation and rejection take care of the problem. The feeling in Vienna, where this had been discussed, had been that such alternatives would cost too much time and would entirely depend on the work of European botanists. The special committee of his motion was to look at this issue in a broad way. Was it possible to limit past names by means other than NCU lists? Could one restrict by criteria based on the literature, or on indexes, or by time, or by author? Was there a formula that would allow to restrict the past? And if not: could the committee devise a means of efficiently searching the obscure literature, which was, after all, finite? [Stuessy’s motion was seconded.]

Brummitt asked whether Stuessy was referring to generic names only. He had thought the proposal was for a Committee on early generic names.

Stuessy replied that the committee would have to consider problems at the generic and lower ranks, particularly species, because that was where much of the difficulties existed.

Brummitt, over the years, had seen many proposals aimed at restricting early publications, and they had never worked. The committee would be beating its head against a brick wall. There was now an efficient system of conservation and rejection that was applicable to almost any name, and even though these processes admittedly took time, they
worked very well. He much preferred to deal with the cases as they arose, so that one could look at the implications in detail. One should avoid to introduce broad, sweeping proposals of which one could never predict the effect in practice.

**Frodin** agreed with Stuessy that the literature of Central and Eastern Europe, especially local floras and botanic gardens catalogues, was very rich. He wondered whether a sample had been taken, to find out how well they were covered in Index Kewensis. Would one have to register many additional names?

**W. Anderson** recognised that plant taxonomists dealing with the European flora were in a particular situation in facing the horrible problem of all these old publications and obscure names, and they had his sympathy. However, they were dealing with a tiny percentage of the world’s flora; most of the world’s plants did not occur in Europe. The solutions they proposed were tailored to their situation but tended to include botanists and plants in the rest of the world, where the problems tended to be less serious. He was very nervous about any solution tailored specifically to the European situation but which might have unforeseen consequences for the rest of the world’s botanists.

**Demoulin** pointed out to W. Anderson that what he had been seeing through the window of the bus along the road, between the hotel and the session room, were mostly introduced European plants. Major weeds of importance for agronomy, throughout the temperate regions of the world, were mostly introduced European plants. The nomenclature of European plants were of major concern to botanists around the world.

**Trehane**, speaking of cultivated plants and on behalf of agriculture, forestry, and horticulture, demanded that the taxonomic community supply lists of stabilised names. One of the great achievements of the past few years had been publication of *NCU*-3, listing names of genera. Those dealing with cultivated plants relied on a system of databases, with named taxa and related data relevant in a number of areas, such as marketing, intellectual copyright, and basic cultivated plant taxonomy. One of his great disappointments in these sessions had been the demise of the NCU concept, especially with reference to *NCU*-3, because it had at a stroke removed what could have become a standard by which to hang names. He very much supported Stuessy’s proposal. One needed to look at methods of protecting names at least at the generic level. Changing the name of a genus had a massive impact on cultivated plant
disciplines. A list of generic names was badly needed that would provide ratified spelling, agreed gender, and, preferably, statement of type, enabling exact interpretation of each name’s meaning. Drawing upon such a list, promoted and authenticated by the IAPT and the Nomenclature Section, would obviate once and forever the need to go back to the literature. He urged that the work already done not be lost, so that back home he could give those in the cultivated plant disciplines a message of hope for the future, hope that the taxonomic community was trying to help solving the problems in cultivated plant nomenclature.

Funk exploded. Had Stuessy been listening at all? Discussion was going around and around and around on the same old arguments. The Section had voted these ideas down, over and over again. She did not know what to add – it was all déjà vu. In Yokohama on Friday morning, when all thought the nomenclature sessions were completed and the Section Meeting would be adjourned, there was an apparently innocuous proposal written on the blackboard which, having been passed, had led to great conflicts later on. Stuessy’s motion was the same sort of thing and should be voted down. There was no need to go over this ground again. “Please, everyone, not another six years!”

Hawksworth pointed out that the NCU proposals had been voted down by a very unrepresentative assemblage [loud protests], thinking in particular of groups other than flowering plants. Stuessy felt there were problems with higher plants, but let him try to work with the literature on microalgae, fungi, or fossils! There were enormous problems there. At present, the Code addressed neither the needs of taxonomists, who had to spend much time on fruitless searches, or the needs of users of names. That was the sheer reality.

Stuessy insisted on separating his proposal from NCU. It was not an attempt to bring NCU back, but to view the problem in a broader way. NCU focused on one particular solution, with which personally he had never felt comfortable, but the problem was real. He could not understand why anyone would object to authorising a group of people to look at the broad issue. It was healthy to seek other possible solutions; having looked at them, one might accept them or not. This was nothing to fear.

Marhold shared Brummitt’s pessimism, but wondered what should then happen. Two days before it had been said that the number of conservation cases was to be reduced. In the absence of NCU lists, a reduction in conservation cases meant more changes of names. Apparently the
situation was less complex in North America than in Central Europe. So the American side had rejected NCU and wanted to limit conservation. This meant, to Europeans, being forced to change names.

Farjon asked Stuessy whether having been so impressed by discovering the rich floristic literature of Europe, he had put it to the test. Had Stuessy, in other words, surveyed several of the minor Central European floras, pamphlets, and journals, checking whether they included binomials or generic names not listed in Steudel’s Nomenclator, Index kewensis or other compilations of names? He himself had recently published a world checklist of conifers and had done exactly that. He had used IK, Steudel, and other indices, and out of 4300 names, excepting infraspecific names, had found very few that had not yet been indexed. There were, however, numerous examples of “nomina nuda” that had been indexed in place of valid names. If his search, which had taken more than a year in libraries, had any indicative value, one must conclude that the problem was not as big as had been stated.

McVaugh echoed Brummitt’s and W. Anderson’s sentiments. He did not wish to discriminate against Europeans, but thought they were mainly dealing with problems that went back to Linnaeus. If they would typify all Linnaean names satisfactorily, and a good start had been made, many problems would be solved. Those working on tropical floras, where most new names came from, had almost no problems of this kind. He hated to say it because it put a lot of work on his European friends, but the problem really was theirs, it was not world-wide.

C. Anderson reminded Hawksworth that the mail vote on the NCU proposal had been 74 % negative.

Hawksworth replied that the mail vote was not on the issue of limitation of priority but on the NCU proposal, which was a separate issue. If Farjon had spent nearly a year to find a few more names, one might wonder what had been the rationale and actual cost of that work, and whether the money might not have been better spent on real science.

Farjon had expressed himself wrongly if he had given the impression that he had wasted his time in dusty libraries for a whole year. A very useful publication had resulted from that year’s work, but in compiling it he had found only a handful of names that had not been indexed.

Hawksworth acknowledged the correction, but it still implied that a whole year’s part had been spent doing the background bibliography.
Reveal had spent much time crawling through obscure literature, looking for suprageneric names for which no aids such as IK or the Gray card index could serve as guide, at no cost for the community except for support by IAPT, his University, and the National Agricultural Library. There was a huge amount of literature of which monographers, who used suprageneric names, were unaware. The service he had provided might not always be appreciated, especially not when it upset accepted priorities. At some time in the future there would be resource limitations on this kind of work, not in six years, perhaps not even in twelve years – but eventually, botanists would have to stabilise their nomenclature. The primary focus of the to-be Committee, if it were accepted, should therefore be on ways and means to investigate thoroughly the literature. The nature of the problems could be defined and resolved over the next ten to twelve years, and then one could formalise the appropriate mechanisms for stabilising nomenclature. The greatest present objection to NCU was awareness of the limited degree to which the literature had been explored. Taxonomists, like it or not, would have to devote more time and efforts to nomenclature. This was a nomenclatural not a taxonomic meeting: Those present should now and then devote some time to nomenclature, should assist European botanists with their problems, which far exceeded what was commonly recognised. In time, a stable nomenclature would develop; not, however, before scholarship had been used and the work had been done. [Applause.]

Greuter concluded from the foregoing, interesting debate that much unresolved conflict persisted. He wanted to highlight some points. First, this was not a European problem, nor was the Linnaean Typification Project at the Natural History Museum primarily addressing European issues, as Linnaeus’s works covered a much wider area. Secondly, most who had commented had accepted as a fact that botany consisted of flowering plants or vascular plants alone. It had been said that most of the botanical diversity resided in the tropics, which was true; what no one had mentioned, but was equally true, was that the largest share of the unexplored botanical diversity was in groups other than vascular plants. Speakers should be careful not to let their own particular concerns obscure their view of the general problems that existed. Thirdly, he did not see much hope for this committee coming up with something useful. But a chance there was, and taking it would cost nothing. Why not let a Committee put their heads together? They might come up with rubbish, but there was the odd chance that they might find a real solution.
Filgueiras asked for a simple clarification: what was a “past name”?

Greuter agreed that Special Committee on the Restriction of Early Names would be a better name. [This was accepted as a friendly amendment.]

Stuessy’s motion was defeated on a card vote (239 : 349; 40.6 % in favour).

[Here the record reverts to the normal sequence of events.]

Recommendation 15A

Prop. A (35 : 162 : 18 : 4) was withdrawn.

Article 16

Greuter noted that, of the many proposals concerning suprageneric names, some might be accepted directly, but others were controversial and complex. The Rapporteurs had therefore suggested that a Special Committee be set up, to consider all such proposals as were defeated or referred to it, and to report to the following Congress. In the mail ballot on some of these proposals a large majority had favoured the idea of such a Committee. He moved that the Section authorise the setting up of a Special Committee on Suprageneric Names.

Greuter’s motion was seconded and carried.


Reveal indicated that his whole set of proposals on suprageneric nomenclature had arisen from the work of Hoogland and himself on family names, started in 1987 and subsequently extended to all ranks above genus. Prop. A-C, which would combine features of Art. 16, 17, and 18 into a single Article, had received a positive mail vote. They should be accepted then and there.

Greuter observed that Prop. A-C, taken together, would bring about a clarification that was slightly more than just editorial. Prop. A, to begin with, offered a clear and useful definition of descriptive as opposed to typified names, at the higher ranks. There was no harm in accepting it.

Demoulin moved a (hopefully friendly) amendment, to delete the parenthesis “(‘typeless names’)” after “descriptive names”. The question of typification was independent of the way in which names were formed. Descriptive names were not equivalent to typeless names.
Some day he himself might propose that descriptive names above the rank of family be typified, just as family names like *Papilionaceae* had a type. [The motion was seconded.]

**Reveal** pointed out that the current *Code* defined descriptive names as names based on a certain circumscription not on a type. There were special provisions, in Art. 19, to typify ten well-known descriptive family names. To enable the use of descriptive names as an alternative and equivalent to typified names, special provision in the *Code* was needed. Names above the rank of order were frequently descriptive and in widespread use. There would be enormous problems of homonymy if names such as *Gymnospermae* and *Angiospermae* were to be typified.

**Demoulin** maintained that, at higher ranks, the *Code* essentially discussed automatic typification of names based on the name of a genus. By default, some had assumed that it was impossible to typify a descriptive name. Those who preferred to work with typified names must not be prevented from retaining well-known, classical names such as *Ascomycetes*, but should be permitted to typify them. Some authors had strayed from the principles of stability and priority and had introduced useless new names in the belief that typified names, based on generic names, were preferable.

**Greuter** advised Reveal to accept the motion as a friendly amendment. It did not imply that descriptive names could, let alone must, be typified, it left the issue open, and if any fixing was needed it could be done later. No precedent would be created by not mentioning explicitly “typeless names” (an awkward phrase requiring quotation marks).

**Reveal** agreed [so Demoulin's motion became a friendly amendment].

Prop. A was accepted as amended.


**Greuter** qualified Prop. B, which had been even more heavily favoured in the mail vote than Prop. A, as a useful corollary to the latter.

Prop. B was accepted.

Prop. C (140 : 8 : 6 : 62) was also accepted.

Prop. D (64 : 45 : 32 : 74).

**Greuter** noted the Rapporteurs’ critical comments, which mainly concerned editorial issues but might have caused the lack of enthusiasm
reflected by the mail vote. The proposal could be safely accepted, on the understanding that the Editorial Committee would take care of the Rapporteurs’ suggestions. Essentially, the proposal would be split into its suprafamilial and lower-rank components, i.e., its text would be duplicated and would appear in two places in the *Code*. Also, the last example would not be taken up by the Editorial Committee, as it was not in fact covered by the rule.

**Prop. D was accepted.**

**Prop. E (34 : 37 : 42 : 102).**

*Greuter* noted the strong “sp.c.” vote in the mail ballot, but also a pronounced “ed.c.” component. The latter was in support of a suggestion by the Rapporteurs. While the proposal, in line with the example, spelled out sound practice, it would introduce the concept of legitimacy at ranks above family – implying that there was illegitimacy, too – when under the current *Code* there was no illegitimacy at those ranks. The Rapporteurs had therefore suggested that there was a simpler course to achieve the proposal’s intent: in Art. 16.1, as reworded by Prop. A, one might replace the phrase “adding a termination denoting their rank to the genitive singular stem of a generic name” by “replacing the termination *-aceae* in a legitimate name of an included family by the termination denoting their rank”*. Automatically typified names at the higher ranks would be defined as derived from the legitimate name of an included family [as was already the case for names of orders and suborders], not from a generic name as was presently the case. This would take care of the present difficulty, addressed by Prop. D, with names such as *Caryophyllidae* (Reveal’s example): *Caryophyllus* was an illegitimate generic name, but the family name *Caryophyllaceae*, being conserved, was *eo ipso* legitimate. Would the proposer accept this suggestion as a **friendly amendment**? [Reveal agreed.]

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

**Prop. F (39 : 114 : 10 : 52).**

*Reveal* explained the rationale of his proposal. Many authors, from the late 1700s to Bentham & Hooker and all those following Candolle’s *Lois* of 1867, routinely used *Eu-* as a prefix to designate a subtribe placed in the corresponding tribe. According to the *Lois* [Art. 24], the termination for both tribe and subtribe was *-eae* [or *-ineae*], which explained the practice of using *Eu-* to make the distinction. The Rap-
porteurs had pointed out that designations beginning with *Eu-* were not acceptable under the *Code*, but that provision [Art. 21.3] applied only at ranks lower than genus. He was aware of 69 names, displayed on the screen, that would be lost were the tradition of just dropping the *Eu-* prefix not made legal, including some that were in current use and of which he would not then know the place of valid publication.

Greuter had noted Reveal’s comment, placed on the World Wide Web, stating that the Rapporteurs’ opinion was confused – which he rather disliked. [Laughter.] The Rapporteurs had stated that this proposal, would declare valid some names that at present were not validly published. Reveal disagreed, but he erred. Suprageneric names prefixed with *Eu-* had not the appropriate form as defined in Art. 16-27, particularly Art. 19, and were not validly published under Art. 32.1(b). Unless the Section accepted Prop. F, such names would not be valid, but in their correct form, without the *Eu-* prefix, they could be validly published at any time. If they were indeed in current use, they had doubtless long been validated. Of the 69 names on Reveal’s list, one was of a subfamily, four of tribes, and 63 of subtribes. The latter were hardly of overwhelming importance. Accepting the proposal before careful scrutiny by the new Special Committee was not to be recommended.

Reveal did not mind, as long as there was no objection to a change of authorship and place of publication of many names that were in current use. The present tradition was just to drop the *Eu-* prefix.

Prop. F was referred to the Special Committee on Suprageneric Names.


Reveal explained that the proposal would introduce into the *Code* a termination for names in the rank of superorder, would maintain the terminations for names at rank between phylum and suborder, and would assemble them all in a single place, in the revised Art. 16.

Greuter referred to the Rapporteurs’ published comments. The proposal would upgrade the present Rec. 16A to the status of a rule, combining it with Art. 17.1 in a new paragraph of Art. 16. This was logical and editorially sound. Attention should focus on the introduction of the rank of superorder, which was new matter. Concomitant deletion of Art. 17.3 had been suggested by Reveal in his comments, but was not part of the formal proposal.
Reveal had hoped that deletion of Art. 17.3 could be handled editorially. The inclusion of superorder was indeed new matter. That rank had been adopted first by Bessey, then fell out of use until it was revived by Takhtajan in 1959 and formalised in 1967. Bessey had used the termination -floreae for supraordinal names, but Takhtajan had preferred -anae, which Dahlgren had adopted before he died and Thorne now used routinely. The reason for dropping -floreae was the feeling that it was an inappropriate for algal and fungal groups. The rank was now widely used, at least in flowering plants.

Greuter asked whether it was the general feeling that the rank superorder and the proposed termination be adopted immediately, or whether the matter should better be referred to the Special Committee.

Ochsmann pointed out that superorder was not among the ranks presently listed in Art. 4, and wondered whether it should be added if the proposal was accepted.

Greuter agreed that acceptance of the proposal would authorise the Editorial Committee to add “superorder” to the list of secondary ranks.

Moore observed that if “superorder” were added to the secondary ranks, “subsuperorders” would also become possible, which sounded rather odd. As the Code provided for “sub”-categories at all ranks, it should do the same for “super-”, which might require another paragraph.

Reveal, when drafting the proposal, had considered the question raised by Moore, but had felt it premature to suggest a modification to Art. 4 to extend the use of “super-” to all ranks. That question should be considered by the Special Committee. Adding superorder was a different matter. That rank had been proposed to a Congress 18 years ago but was not then approved, its usage being still rare. Now usage was well established, so it was convenient to have a definite termination, but Art. 4 should not be changed at this time.

Barrie pointed out that “superorder”, or any “super”-rank, was permitted under the Code as a further rank [Art. 4.3]. Botanists were allowed to invent as many ranks as they found necessary to create a classification, even though there was no specific mention of them.

Greuter had meanwhile realised that the question was more involved than he had originally thought. It would be illogical to prescribe a termination for a rank not recognised in Art. 4. Art. 4.1 listed secondary ranks, designated by unprefixed terms, and Art. 4.2 provided for other
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ranks, designated by adding the prefix “sub-” to the terms of the principal and secondary ranks. The prefix “super-” was not now mentioned. If it were introduced, this would authorise the ranks superkingdom, supradsivision, superclass, superorder, superfamily, supertribe, supergenus, supersection, superseries, superspecies, supervariety, and superform—or else, a complete rewriting of Art. 4 would be needed, which was not an editorial matter. It was therefore advisable to refer clause (e) of Prop. G to the Special Committee, to consider it in parallel with its repercussions on Art. 4, and accept the rest of the proposal, which was essentially an editorial improvement. [Reveal concurred.]

Demoulin wondered what would happen with descriptive names. There was no reason to limit standard termination to automatically typified names. In fungi, names such as Ascomycetes, Ascomycotina, Ascomycetidae, etc., were widely used. Unless he had missed something, perhaps due to jet lag, such names would now be outside the *Code*. There was no reason to limit the provisions for rank-specific terminations to automatically typified names.

Greuter confirmed that descriptive names would no longer be covered, but the *Code* would not prohibit them either. This was now a Recommendation. Demoulin’s point might best be considered by the new Special Committee.

Demoulin pointed out that the present Rec. 16A, as the Rapporteurs had recognised in their comments, covered both descriptive and automatically typified names and suggested using the same standard terminations for either. There was no reason to change this.

Reveal explained the reason why he had excluded descriptive names from the proposed provision. As it was inappropriate to write Palmaeae, Labiataeae, etc., at family level, so it was inappropriate to enforce a change from Centrospermae to Centrospermales when referring to carophylls, or to Centrospermaeae to designate Lemnaceae (for which the term had been introduced originally). The absence of a rule would not prevent the use of names such as Ascomycota, Ascomycetes, Basidiomycetes, etc., but when dealt with in an Article, standard terminations should be limited to automatically typified names.

Barrie opposed raising the Recommendation to the status of a rule. It was best left as it was, one reason being that it required the use of special terminations for algae. There were classifications in which the traditional break into groups like the algae were no longer used. Making

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this a rule would require authors to use traditional endings when they might prefer terminations consistent with their classification.

Hawksworth pointed out that in the case of fungi this would actually be helpful. The termination -mycota had been retained regardless of the kingdom to which the organisms were now assigned, as it immediately identified groups traditionally studied by mycologists.

Barrie replied that his concern was not to prohibit such usage, but to avoid making it obligatory.

Faegri wondered whether he was sitting in a school for internal revenue functionaries. Scientists must be allowed a certain degree of freedom, such as using super- and sub- categories without asking for permission.

McNeill was inclined to agree with Barrie’s view, that the whole proposal should be defeated because a recommendation was better than a rule.

Greuter still felt that some might want to accept the proposal without clause (e). He suggested that the Section should first vote on Prop. G without clause (e). If [and only if] this was accepted, there could be a second vote on clause (e). What was defeated would be passed to the Special Committee. [This procedure was agreed.]

Prop. G was referred to the Special Committee on Suprageneric Names.


Greuter advised that, in view of the substantial negative mail vote, the proposal should best follow the fate of Prop. G.

Reveal explained that many authors of phylogenetic classifications now preferred to use unranked names for clades. His proposal, which provided a mechanism for naming such clades, might encourage them to validate the names they were using. Books produced by well-known publishers, and articles in major scientific journals, used non-validly published names at numerous suprageneric ranks, or rankless names that had the form of existing names of families, orders, etc. He had tried to accommodate the needs of such workers by proposing a single common termination for purposely rankless names. He would not mind if this was defeated, but he wanted to point out that there was a growing move to abandon Linnaean nomenclature.

Gams strongly supported the proposal, which was badly needed by molecular taxonomists. Linnaean ranked taxonomy was often in-
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compatible with categories needed for cladistic phylogenetics. Rankless taxa were the best way to express cladistic findings.

Moore disagreed. True, many, following Queiroz & Gauthier, wanted to have rankless names in phylogenetic nomenclature. Others had too many ranks in phylogenetic classification, and it might be useful to leave some unnamed. The problem with this proposal was that, with a single ending for rankless names, the number of names that could be based on any genus was just one. Workers in phylogenetics were concerned only with clades and species, they did not want to be involved with ranks.

Reveal felt that, if a group was rankless, only one name was needed.

Moore was concerned that botanists would run out of names. In Kron’s article [in Aliso 15: 105-112. 1997] that Reveal had cited, different clades had been designated by the same, homonymous name.

Brummitt supported Moore. The botanical Code should not try to cover the nomenclature of clades. Clades and botanical taxa were two different things.

Ochsmann agreed that ranked taxa should not be mixed with categories from molecular cladistics, which were clearly different. For the latter, it was fine to just use “clade” plus the botanical name of the group. The proposed ending -itae might make names such as Compositae ambiguous.

Greuter saw the negative mail vote as a confirmation of the preceding comments. There would be a symposium on phylogenetic nomenclature later during the Congress, and it might be wise to await its conclusions. He seized the opportunity to disclaim co-authorship of a paper recently attributed, on the Internet, to “De Queiroz & Greuter”. [Laughter.]

Zijistra had voted “no” in the mail ballot but in the light of the foregoing discussion had changed her mind. It would be useful to have this provision. She belonged to those who considered that the ending of a name, when otherwise there was no clear indication of rank, did indicate the rank. The reasoning could be extended to rankless names by giving them all the same ending.

Prop. H was referred to the Special Committee on Suprageneric Names.

Prop. I (15 : 74 : 50 : 73) and Prop. J (27 : 56 : 58 : 71) were withdrawn (and referred to the Special Committee on Suprageneric Names).
Recommendation 16A


Hawksworth noted that this proposal, and the three following, would avoid the creation of botanical names mimicking the names of viruses. No problems for existing names of botanical groups would arise. The mail vote was supportive.

Greuter added that the main issue was Art. 20 Prop. A and concerned the generic termination -virus, which was the standard termination for all generic names placed under the virological Code. That proposal, being retroactive, would invalidate all botanical generic names ending in -virus – but as no such name existed so far, the proposal was completely harmless. It was a safety measure that would eliminate any future risk of interregnum homonymy between viruses and plants, at no cost. The first three proposals of the series, concerning suprageneric ranks, were barely needed. The suprageneric names that would be proscribed were those formed from generic names ending in either -virus, or -vira, or -virum. There were two such generic names in existence, both ending in -vira [Elvira Cass. (= Delilia Spreng.) and Ivira Aubl. (= Sterculia L.)], and neither was in current use. It was probably an overkill to insert three new provisions into the Code to cover a quite improbable risk, but if the Section so decided, no harm would result.

Hawksworth still felt it was sensible to include these provisions, as they would deal with the problem before it had actually arisen.

Prop. A was accepted.

Article 17

Prop. A (82 : 74 : 52 : 4) was similarly accepted.

Article 19

Prop. A (82 : 74 : 50 : 7) was similarly accepted.

Article 20

Prop. A (96 : 7 : 32 : 7) was similarly accepted.


As Greuter pointed out, this was the first of a long series of proposals by the Committee on Orthography and some of its members, mostly
affecting Art. 60. Several of these proposals, including the present one, had received substantial votes for being referred to a Special Committee on Orthography. The Section should therefore first decide whether it would be wise to reappoint such a Special Committee by authorising the General Committee to do so. In so moving, he appreciated the fact that few of those who had ever served on a Committee on Orthography were keen to renew that experience [laughter], which perhaps was just as well. [The motion was seconded.]

Zijlstra asked that any decision [on Prop. B] be postponed until after discussion on provisions on the hyphen, in Art. 60.

Greuter spoke to the motion. There was an incredible number of proposals on orthography before the Section, submitted mainly as three sets: one by the Committee on Orthography, one by its Secretary, and one by its Chairperson, which were often conflicting in minor or major ways. The Committee had been unable to provide clear guidance to the Section, which would be tempted to give up in despair and leave matters in their unsatisfactory present state. Better try again with a new crew, which might perhaps take a different approach in designing sensible orthography rules. Individual proposals that deserved acceptance now could nevertheless be acted upon immediately, as had just been done for suprageneric names, but all defeated proposals would be referred to the revived Special Committee.

Zijlstra doubted the value of a new Committee, especially if it was constituted in the same haphazard way as the previous one. She knew of no way in which a more representative Committee, that might reach conclusions acceptable to a majority, could be set up.

W. Anderson would oppose the terribly tempting idea to sweep the orthography issue under the rug by referring it to another Special Committee, well knowing that the same would happen again in another six years. No Committee should be appointed if it would foreseeably fail.

Greuter believed that if a new Committee went more in the way of standardisation and gave less weight to original spelling it would come up with sensible solutions. This route had not been sufficiently explored by any previous Committee. Whether the approach was acceptable would be for the following Congress to decide, but at least there was hope for more acceptable future results.

Demoulin repeated what he had said in Yokohama and perhaps in Berlin. Having spent a lot of time, for many years, as Secretary of an
Orthography Committee, he knew this had been the worst waste of time in his life. It was perverse to propose a new such Committee. Earlier Committees had discussed the problem of original spelling versus standardisation. It was surprising that Greuter favoured standardisation, as this would drive further apart the botanical and zoological Codes, zoologists placing emphasis on original spelling. He was not opposed to more standardisation, but such proposals should be made by individuals rather than being discussed in a Committee, which would be useless. In Yokohama he had first refused to be on the new Orthography Committee, but when pressed he had finally accepted. However, when receiving the first mailing of the new Committee he had an allergic reaction [laughter] and could never bear to open it. It was still sitting unread on his computer [more laughter].

Brummitt supported what Anderson, Demoulin, and Zijlstra had said. Opposition to a Special Committee was infrequent, but another Special Committee would get nowhere. Past Orthography Committees had always failed to come up with solutions, they had inhibited and not promoted progress, being bogged down in hopeless bureaucracy without reaching an agreement. Decisions on the proposals should be taken now, without the temptation of referral to a Special Committee.

Gereau echoed the words of Cronquist, “let’s cut through this thicket and get to the other side”. A pile of proposals had been made, resulting from detailed and tedious work. Those who cared about the details should stay and vote on each of them, those who felt unconcerned should go to the Herbarium or Gateway Arch and let the others vote. Perhaps some things could be standardised in St Louis so that there would be fewer proposals on orthography at the following Congress. [Applause.]

Greuter saw that his motion for a Special Committee was either ill advised or premature – which of the two, would become clear when the Section had acted on the orthographic proposals. If all proposals were voted down and there still was a feeling that orthography provisions were in a messy state, someone else might again propose a new Orthography Committee. If the Section wanted to adopt some or all of the proposals, so be it. He therefore withdrew his motion. The orthography proposals should be considered as they arose, each in its turn, and none should be postponed. This would enable the Rapporteurs to keep track of changes and of the situation as it developed. Prop. B, he noted, had received a 52 % “no” vote in the mail ballot.
Strother observed that Art. 60.9 referred to epithets, not to generic names. [This would have changed by adoption of Art. 60 Prop. HH.]

Prop. B was withdrawn.

Prop. C (78 : 98 : 10 : 12), belonging to the deferred hybrid package, was eventually withdrawn (and referred to the Special Intercode Committee ICBN/ICNCP).

Article 21


Greuter had looked at many names newly proposed during the last 18 months, and had found that authors had great problems as to the way in which names of subdivisions of genera were to be formed. There were difficulties in the present provision and its interpretation. Epithets in names of subdivisions of genera were required to be either plural adjectives or have the same form as a generic name, i.e., be substantives in the singular. One of the problems was that, in the past, “plural adjectives” had been understood to mean “plural adjectives in the nominative case”, but the Code did not explicitly say so. This proposal would make this explicit in the Code. It had had a positive mail vote of 65 %.

Prop. A was accepted.

[The following matter, relevant here, arose during the Eighth Session on Thursday afternoon.]

Greuter mentioned a problem that had surfaced in his correspondence with Jacques Melot, an active nomenclaturalist resident in Iceland and unfortunately not in attendance. The matter might look trivial on the surface – unless one was a hair-splitter and took the wording at face value, as one perhaps should. Art. 21.1 read, “The name of a subdivision of a genus is a combination of a generic name and a subdivisional epithet connected by a term ... denoting its rank.” There was a parallel wording in Art. 24.1 for infraspecific taxa. These provisions were fully retroactive, back to 1753. In many cases, however, no term denoting rank was inserted between the genus or species name and the final epithet. Rank was indicated, instead, by a typographical convention of some kind: different type faces, Greek or Latin letters, numerals, or asterisks. These typographical devices were placeholders for the connecting terms denoting rank, but the terms themselves were not present.
Art. 21.1 and 24.1 were among those defining the form of names, referred to in Art. 32.1(b) as a prerequisite for valid publication. Hair-splitters would conclude that, for instance, no Linnaean varietal name was validly published because there was no connecting term indicating the rank, simply a Greek letter.

The problem could be solved neatly by splitting the present Art. 21.1 into two sentences, to read, “The name of a subdivision of a genus is a combination of a generic name and a subdivisional epithet. A connecting term is used to designate the rank”. This would mean that the connecting term, while of course used in normal practice, was technically not part of the name. A parallel correction was needed in Art. 24.1. He proposed these amendments as a motion from the floor, which was seconded.

Voss heartily endorsed the motion. He had not noticed the problem before. Indeed, the connecting term was not part of the name. Sometimes an editor would insist that it was and, therefore, must be italicised like the name, but the name consisted only of the two or three Latin words.

Greuter’s motion was carried.

[Here the record reverts to the normal sequence of events.]


Greuter drew attention to the favourable (5 : 3) vote by the Committee for Bryophyta. This was an alternative to Prop. C, on which the vote by the same Committee was negative (2 : 6). The mail vote was parallel but even more decisive, 83 % positive on Prop. B but 84 % negative on Prop. C.

Prop. B was accepted.

Prop. C (13 : 175 : 9 : 12) was ruled as rejected.

Article 22


Greuter introduced this as a “housekeeping proposal”. An alternative was Art. 55 Prop. A by Isoviita, who had written that he was happy with the present solution. This had also been favoured by the mail vote.

Prop. A was accepted.
Article 23

**Prop. A** (137 : 71 : 10 : 0).

**Greuter** introduced this as an issue that would be familiar to all present and could be discussed even in the absence of the proposer, who had temporarily left. The question of capitalising specific and infraspecific epithets had long been controversial. Rec. 60F currently advised that all such epithets be written with a lower case initial letter. The proposal, supported by the mail vote, would upgrade this to the status of a rule, in line with current practice. He was personally reluctant to abolish the liberty of authors to use capitalisation, as he had used that liberty himself. A capital initial letter in an epithet could be meaningful. It indicated a substantive word, that would not change when transferred to another genus with a different gender. It explained why *Lythrum Hys sopofolia* and *L. Salicaria* were not correctable to ‘*L. hyssopifolium*’ and ‘*L. salicarium*’. Acceptance would result in a loss of information not easily available, or rather, in the loss of the liberty of expressing and conveying that information. He recognised, however, that he was in a minority position in this question.

**W. Anderson** sympathised with Greuter, agreeing that this would lose information, but was afraid the battle was long since lost, like “data” and “datum”. He did, however, hope that “small” would be changed to “lower case” [which, Greuter interjected, was a battle won].

**Demoulin** also felt that the liberty to use capital initials should be left. Some traditionally minded authors considered that an epithet commemorating a person be capitalised, and felt offended when obliged to use lower case. This liberty was a small peculiarity that caused no harm, as someone had remarked at the Sydney Congress.

**Toolin** hated to see a Recommendation he could live with changed into a rule. He knew several botanists who liked to capitalise epithets commemorating a person’s name, a long-standing practice that should not be ruled out.

**Faegri** feared that few, when deciding whether to use a lower-case or capital initial, would know the reason explained by the Rapporteur. They would feel rather helpless. If the proposal were rejected, the rationale for using capital initials should be added in a footnote.

**D. Ward** had been explaining for 40 years that the spelling of one plant name was *Rhus Copallinum*, although the gender of *Rhus* was feminine.
This ought to stay a recommendation, reminding the future that there was a past one should give credit to.

Kirk feared that if the proposal was accepted it would have implications for Art. 32 on validity. Would it be a correctable error if someone published an epithet with a capital letter?

Greuter confirmed that, unless an exception was stated under Art. 32.1(b), or in Art. 23.1, to the effect that the use of capital initials in an epithet did not affect valid publication, there might be a problem. Future names published in contravention to the rule might be deemed not to be validly published. Answering a question by Barrie, he corrected himself. As no starting date was mentioned, the rule was retroactive and so would affect previously published names as well. Names such as *Lythrum Salicaria* would probably be invalidated, unless one took care to avoid that risk by some special provision, which could be done.

Gereau by now had probably a reputation for supporting his points by quoting from the Preamble: “This Code aims at the provision of a stable method of naming taxonomic groups, avoiding and rejecting the use of names which may cause error or ambiguity or throw science into confusion. ... Other considerations, such as absolute grammatical correctness, regularity or euphony of names, more or less prevailing custom, regard for persons, etc., notwithstanding their undeniable importance, are relatively accessory” [Pre. 1]. The Section was debating a relatively accessory point. It should sanction the now prevailing custom, giving up all capital letters and calling it a correctable orthographical change.

Davidse suggested to introduce an amendment to solve the problem, stating that when an epithet had been published with an initial capital letter, this was automatically correctable.

Greuter, trying to be helpful against his better sentiments, moved the corresponding amendment. Its appropriate form, in Art. 23.1, would parallel the second sentence in Art. 18.3. “Contrary to Art. 32.1(b) such a name is validly published if it complies with the other requirements for valid publication”. As the author of the proposal was not present to accept this as a friendly amendment, Davidse’s motion had to be seconded [which it was], then voted. It would make the proposal acceptable.

Zijlstra spoke against the amendment, which would make the Code even more complex than it already was. She was also against Prop. A.

Filgueiras feared that, if the matter was left as a Recommendation, there would be names spelled both ways, causing problems in databases.
Greuter no longer saw this as a problem. Databases had long learnt to alphabetise capitalised and non-capitalised words in a single sequence. The motion was, that the Editorial Committee be empowered to add a sentence analogous to the second sentence now in Art. 18.3, suitably reworded, to ensure that, if the proposal did pass, names published with an initial capital letter to their epithet would remain valid. Otherwise, as had been pointed out, their status would be threatened.

**Davidse's motion was carried.**

Voss considered the discussion largely irrelevant. A simple cross reference to Art. 60.2, in Art. 23.1, could take care of the concern raised. Art. 60.2 stated that “spelling” did not refer to the use of an initial capital or small letter, this being a matter of typography.

Greuter agreed. The Editorial Committee could handle the matter in that way. No new vote was required.

**Keil** would vote against the proposal. The current recommendation, which covered what most people were doing, carried sufficient weight. Better not tie the hands of those who wanted to do it differently.

**McVaugh**, at the risk of prolonging the very interesting discussion [laughter], said that leaving this as a recommendation would encourage confusion. An increasing number of people did not know any Latin and could not care less. Sixty years ago he had been delighted to learn that in *Lobelia Cardinalis* the epithet should be spelt with a capital C, as *Cardinalis* was an old generic name. Since his thesis had been published in 1935, no one had used that capital C, including himself. [Laughter.] If this remained a recommendation, people would have to look at every case, which they would not do. This should become a rule. Forget sentiment, forget the information one was going to lose – which was too bad, but then, it had already been lost in the popular mind, and the Section might as well make it official.

**Wiersema** disagreed. As had been said, there was no problem from a database point of view. If there was a basis for retaining capitalised epithets, people could use it to indicate when they made a substantive epithet. If the recommendation were made mandatory, the explanation as to why this was done would be lost.

**D. Ward** pointed out that *Cardinalis* was and would remain a substantive, a word with an ending inherently different from the ending of an
adjectival specific epithet. If the derivation of the word was hidden, that fact would be increasingly obscured.

**Jansonius** was all in favour of using a lower case standard for specific epithets. The loss of information by not using a capital initial could be compensated by providing a full derivation of a name on publication. This might encourage people, when they felt uncertain, to seek help to make their names properly.

**Nic Lughadha** pointed out that the present proposal would promote harmonisation between the *Codes* much more strongly than any of the harmonisation proposals. Capitalised epithets were the single biggest stumbling block for the average student. If one were in a position to say, capital letter genus, small letter specific epithet, many mistakes would be eliminated.

**McCusker** supported the proposal, but noted that there were surnames which had capital letters not only at the beginning, but within the name. One should get rid not only of the first capital letter, but of all.

**Prop. A** was rejected on a card vote (397 : 273; 59.25 % in favour).


**Greuter** echoed what the Rapporteurs had pointed out, the illogicality of introducing a new Note in Art. 23, but not in Art. 24 where it would be equally appropriate. This dissymmetry would make the Note, which would not introduce any new concept, confusing rather than helpful. One of the suggested examples was faulty, as it miscorrected Desfontaine’s neutral noun *laurocerasum*, correctly referring to the fruit, to feminine *laurocerasus*, meaning the tree. Desfontaine knew his Latin better than most present-day botanists.

**Zijlstra** objected that Desfontaine had written the epithet with a capital initial letter. Did this not imply that he meant the generic name *Laurocerasus* rather than the fruit?

**Gereau** noted a word designating a fruit was as much a substantive as one for a tree. Desfontaine would spell either with a capital letter.

**W. Anderson** requested that, if the proposal were passed, the Editorial Committee correct “which” to “that”, twice in the Note.

**Demoulin**, to catch up some of the time that had been lost owing to his request of a card vote, moved that Prop. B, C, and D, all relating to
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substantive epithets and all essentially editorial, be referred to the Editorial Committee.

Greuter recommended to deal with the proposals one by one, or more time would be lost in procedural discussions than one might ever gain.

Voss concurred with Demoulin. Prop. B and C dealt solely with Notes and Examples, which were the province of the Editorial Committee. They would effect no substantive change in the Code.

Greuter noted that the Editorial Committee would be free to introduce parallel statements in other articles, as the Rapporteurs had suggested.

Prop. B was referred to the Editorial Committee.


Perry challenged the Rapporteurs’ statement that her proposal would not change any substance. The present example of *Rubus amnicola*, which went back to the Paris Code, was confusing. The statement that *amnicola* was a Latin substantive was added without discussion in Edinburgh [in fact in Montreal, upon a proposal by Morton]. Later [in the Berlin Code] an editorial addition made it explicit that the original spelling was ‘*amnicolus*’, but the mention remained that this was a substantive. However, it must be assumed the original author thought he was using an adjective, not a substantive. The statement that it was a substantive was inappropriate, but if it was removed, one had to explain what to do with epithets ending in *-cola*, which in classical Latin could be either nouns in the first declension or one-ending adjectives. To prevent discussions and obviate the need to consult the original publication, it was advantageous to state in the Code that one must use *-cola*.

Greuter would have thought that the Rapporteurs’ statement, that Perry’s welcome proposal implied no change of substance, was rather helpful to make the proposal pass. The Code dealt with names not with grammar, and the proposal would make no change as far as names and their endings were concerned. The intended clarification would indeed be useful, as epithets ending in *-cola* were frequent and errors in their formation were widespread.

Jansonius had been told at the Latin Department of the University of Calgary that *-cola* was only substantive in Latin. The information that it could be an adjective was incorrect.

Greuter promised that the Editorial Committee would omit any unnecessary grammar and stick with the nomenclaturally relevant facts.
McVaugh suggested that a “bad” example, of an epithet in -cola treated adjectivally, be added in the Code. [The present Rubus ‘amnicolus’ is such a “bad” example.]

Demoulin concurred with Jansonius. Some adjectival usage of words ending in -cola could probably be found in post-classical Latin, certainly in botanical Latin. To simplify things, one should standardise according to good practice and always treat -cola as a substantive.

Filgueiras stressed the fact that it was irrelevant whether an epithet was contrary to classical Latin, because botanical Latin as it was now used was quite distinct from classical Latin.

Demoulin replied that in good botanical Latin epithets in -cola were treated as substantives. He had been referring to minority usage that one might possibly find in church Latin or some botanical texts.

Prop. C was accepted.

Prop. D (48 : 65 : 98 : 0), concerning an example, was referred to the Editorial Committee.

Prop. E (10 : 193 : 7 : 1) was ruled as rejected.
FIFTH SESSION

Wednesday, 28 July 1999, 9:00-13:00

Article 27

Prop. A (117: 24: 69: 0), being exactly parallel to Art. 22 Prop. A, previously accepted, was similarly accepted.

[The following actions were taken during the Seventh Session on Thursday morning, when the deferred hybrid package was dealt with.]

Section 5bis (new)


Article 28

Prop. A (58: 88: 50: 5) was withdrawn (and referred to the Special Intercode Committee ICBN/ICNCP).

Prop. B (10: 159: 31: 3) was ruled as rejected.


Trehane requested that Art. 28 Prop. C be referred to the Editorial Committee.

Greuter noted the “ed.c.” mail vote of 55 %. The Rapporteurs had asked for clarification as to why the second sentence of the note should be deleted and not just updated. The relevant provision was still in the cultivated plant Code [as Art. 17.9].

Trehane replied that the concept of fancy names was no longer in the cultivated plant Code. That Code did demand that, as of 1 January 1995, cultivar names be markedly different from botanical epithets, but they were no longer termed fancy names. The Article to which the present Note 2 referred was obsolete.

Prop. C was referred to the Editorial Committee.

[Here the record reverts to the normal sequence of events.]
**Article 29**

Prop. A (26 : 190 : 3 : 1) was ruled as rejected.


Greuter noted that Prop. B, from the Special Committee on Electronic Publishing, would make it explicit that on-line publication or the dissemination of distributable electronic media was not effective publication. As Zander confirmed, it would clarify that, for the time being, publication of text on servers or by other electronic means was not effective.

Prop. B was accepted.

Prop. C (13 : 202 : 1 : 5) was ruled as rejected.

**Article 30**

Prop. A (157 : 15 : 41 : 2) was accepted.

Prop. B (13 : 194 : 5 : 7) and Prop. C (6 : 210 : 1 : 2) were ruled as rejected.

Prop. D (74 : 129 : 8 : 5).

Farjon explained the main purpose of his proposal: to restrict effective publication to such printed items as were effectively distributed, and to ask for some kind of proof that more than a couple of copies existed and had been deposited in a library or otherwise made available to the public. In many countries, to obtain a doctoral or other university degree, only one or two copies of the thesis were needed. New names might be included, but with the intention of publishing them later, with the whole thesis or parts of it, in a journal, flora, or other venue. At present, they might have been validated already in the original thesis, if effectively published, which could lead to disputes about priority, especially when such names were not picked up by the compilers of *Index kewensis* or other relevant indices. By requiring an ISBN number, the proposal would ensure that the thesis was intended to be effectively published. In the Netherlands, where theses normally had a printing of 150 to 300 copies, they already often carried an ISBN number.

Gandhi, while in favour of Prop. D, asked for a clarification on the status of works published prior to 1 January 2000. There were theses of which copies had been distributed to libraries and friends. Would these still be regarded as effectively published?
Brummitt strongly supported the proposal. Problems of names published in theses had been advanced as an argument in favour of registration. Here was a chance to overcome these problems with a simple solution that would maintain the present situation for a great majority of names and provide an immediate answer in ambiguous cases. The comments of the Rapporteurs were not objective, they were unnecessarily biased against the proposal, and they had led to a negative mail vote. They had claimed that the proposal “would severely restrict the future possibility of effective publication by means of theses”, but it would do nothing of the sort. Anybody could ask for an ISBN [International Standard Book Number]. He and Farjon had investigated the situation and had found that virtually all theses that one would expect be regarded as effectively published already had an ISBN, whereas the ones one would not expect to be effectively published invariably did not. The proposal would rule against theses produced in very few copies, those available from microfiche companies with the possibility of printing off copies and placing them in one or two libraries, then claiming effective publication under Art. 29, as had happened in the past. A great advantage of this proposal was that a decision on its effective publication could be immediately obtained from the publication itself without the need to establish how many copies were printed and where they were deposited. It might come as a surprise to many that was such a simple and practical solution to the problems of theses existed. Here was a chance to take a positive step forward on the problems of effective publication and simplify the work of indexing services at a stroke.

Hawksworth objected to the proposal. Unless one were a publisher, one could not be allocated an ISBN directly. As the system worked, this could only be done through a publisher, university, or other body which actually had that arrangement.

Nic Lughadha disagreed. Any individual could obtain an ISBN, as a number of members of the Section could confirm.

Dorr opposed Prop. D on principle. He knew of no other instance in which the Code delegated responsibility for nomenclatural decisions to an agency over which it had no control, such as the one that issued ISBN. There was at least one instance of a published flora in which the ISBN was completely fictitious.

Faegri agreed that the ISBN was no panacea, that by this criterion one might lose or gain some works unwittingly. However, he agreed with
Brummitt that it was necessary to clear up the foggy borderline between publication and non-publication. He recommended adoption of the proposal, which was an important step in that direction.

W. Anderson explained that indeed anyone could get a set of ISBN by just writing to the agency, as he himself had done. However, an ISBN was no guarantee of effective distribution. One could produce a book with an ISBN in a single copy. The agency did not follow up or make any demands as to how many copies were printed or distributed. Those who thought that an ISBN was some kind of magic guarantee of effective publication were mistaken.

Gams saw the proposal in relation to the issue of registration of names. The concept of registration had mainly arisen from problems in defining effective publication in the field of grey literature. Unless registration was implemented, the proposal was indispensable.

Pipoly suggested a modification of the proposal, to include ISSN as well, to cover some serially published Dutch theses.

Nic Lughadha, responding to Dorr, felt that it did not matter if an ISBN was made up. The important issue was the expression of the author’s intent to publish. In some cases, it would indeed still be necessary to confirm that there were at least two printed copies.

Zander knew that one of the principles on which botanical nomenclature operated was that the intention of an author could be figured out through detective work, intuition, and clairvoyance. In the past it had generally been assumed that a dissertation or thesis was not intended for publication but to fulfil the requirements for a degree. Additional information, such as an ISBN, was needed to indicate the intent that it be treated as a publication. The proposal implied that before 2000 all theses were intended as effective publications, which was not the case.

Farjon responded to W. Anderson and other critics of the ISBN number. True, any individual could ring up the issuing institution, get an ISBN assigned, then produce a single copy. However, part of the ISBN indicated the publisher. Major libraries had a directory of publishers and their ISBN, and in the odd case in which one might have some doubt, one could look up the publisher via the ISBN. When the publisher was coincident with the author, one could contact him or her and enquire how many copies had been issued. In the case of institutional or commercial publishers, there would be no such doubt.
Gandhi reinforce his previous comment [on pre-2000 theses]. He had come across several cases of new names that existed only in dissertations of which he was uncertain whether they were effectively published or not. Clarification was needed.

Filgueiras supported the proposal’s idea but, as Gandhi, was concerned over names published prior to 2000. If the proposal was approved as presented, they would still have to be dealt with. In Brazil it was common practice to produce dissertations and theses in 10 to 20 (or 30) copies, some deposited in different libraries. A large number of names had been so published, on which a ruling was needed. The date from which this Article was to take effect was therefore important.

W. Anderson noted that any small entity could be a publisher. The herbarium of Kleinkleichersdorf could be a publisher, even if there was only one person there. The fact that the publisher was not a person but an “institution” was no guarantee against abuse.

G. Yatskievych drew attention to the situation in the U.S.A., where a commercial firm, University Microfilms, made generally available through sale copies of dissertations and theses from most of the institutions that offered such degrees. Students were required to deposit a copy or more at the home institution’s library, and also to send a copy to University Microfilms. If in the future University Microfilms were to start standardly issuing ISBNs to all the items it distributed, many of which were copyrighted, then under this proposal every dissertation and thesis produced in the U.S.A. would become effectively published. The system could be easily abused. Students, at least in the U.S.A., should be forced to process the information and polish the format, not be allowed to take shortcuts to make their names validly published. In other countries there were serial publications in which dissertations were standardly published, ensuring a wide distribution. In the U.S.A., however, the proposal might open the floodgates for many theses to become effectively published with little or no effort on the part of their author.

Demoulin pointed out that presently, the major way of keeping out of circulation names published in theses (a problem to which he himself had once contributed) was by applying Art. 34 and relying on the (lack of) intent of authors to decide on the (in)validity of their names. Some theses were a kind of manuscript distributed in several copies to members of a jury. Until now, Art. 34 permitted us to keep such names out, no matter whether the thesis was later duplicated in large numbers by
University Microfilms or Koeltz or any other publisher. The proposal would, for some items, take care of the difficulty in deciding what the author’s intent had been, always a subjective decision. In other cases (including all pre-2000 ones), validity under Art. 34 would remain the criterion. It would be better to encourage students to include a disclaimer in their thesis, specifying that any new names were not intended to be validly published there. The proposal was still useful. The question of the number of copies was not so relevant. Usually, theses were produced in at least as many copies as there were jury members, many of whom would later deposit their copy in a public library.

Jansonius suggested that the problem might be solved turning around the first clause in Art. 34.1, that now read "a name is not validly published when it is not accepted by the author", but should instead require that authors state their intent to validly publish names in their work when they fulfilled all other conditions for valid publication. Such a statement, in a paragraph preceding the descriptions, would not be more onerous than the required statement of the place of deposit of the type.

Voss lent strong support to G. Yatskievych’s point. What was being proposed was a patchwork way of approaching the real issue, which was the definition of printed matter, difficult though this might be. Picking out items that were stated to be a thesis for obtaining a degree – whether accepted or rejected was not specified – did not address all other kinds of grey literature, such as contracted reports and environmental impact statements, which their author might have used to validate new combinations.

McNeill wondered whether Pipoly's suggestion of adding the ISSN option could be accepted by the proposer as a friendly amendment.

Farjon refused. Adding ISSN would unduly widen the scope of the proposal. A thesis published in a periodical was just a publication in a periodical, not a thesis in the sense of the proposal, which was aimed only at theses that were one-off productions.

McNeill contradicted Demoulin’s interpretation of Art. 34. The situation with regard to names published prior to 1 January 2001 (rather than 2000 if the usual practice was to be followed) had been analysed by Nicolson [in Taxon 29: 485-488. 1980], and Weresub and himself [in Taxon 29: 471-476. 1980]. There was nothing in the Code to prevent the conclusion, however distasteful, that any name published and accepted by the author in an effectively published work (one that was
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distributed to at least two libraries accessible to the public generally) was validly published. It had to be accepted as such if it was accepted by its author. It was hard to imagine a thesis in which an author did not accept his own conclusions. The Art. 34 question was not whether or not an author wanted the thesis to be the primary place of publication, it was whether the author accepted the names. The suggestion Jansonius had made was a possible solution, also alluded to by Weresub and himself, but it had never been formally proposed, let alone incorporated into the Code.

Gandhi suggested simplifying the proposal, to state that no names in theses and dissertations were effectively published, regardless of date.

Kolterman agreed with previous suggestions – as one way of getting around the problem of ISBNs not requested by the author of the thesis but added later by, say, University Microfilms – to require an explicit statement in the thesis, to the effect that names appearing there were intended for publication in the thesis as such.

Brummitt commented on the concerns raised by G. Yatskievych and Voss, about the possibility that a microfiche company might add ISBNs. Those applying for an ISBN had to provide fairly detailed information. He was sure there was no way a company could claim an ISBN for microfiches, as it was necessary to state, e.g., how many copies were being distributed. [Several contradictions from the audience.]

Hawksworth confirmed what W. Anderson had said. In the U.K. one had to be a publisher to obtain an ISBN number, and the publisher’s identification was one element of the number itself. Individuals could act as publishers, but when registering with ISBN they would have to list a number of future works (irrespective of numbers of copies to be printed), and a whole series of numbers would be issued. Adoption of Prop. D might thus encourage some people to act as their own publisher and side-step refereed outlets, which was the last thing that was wanted.

Faegri pointed out that there were two different issues that should not be confused and had both to be covered. One was requiring that authors state their intent to introduce new names. This alone did not fulfil the second condition, to make the publication in which the name appeared available to the public.

Greuter had taken particular interest in the comment that gave much credit to the Rapporteurs’ influence on the mail ballot. He doubted that the Rapporteurs’ advice was generally followed by those voting, on
some it might have the contrary effect. In the present case, those participating in the mail vote had obviously carefully considered their answer. Dorr’s point about delegating authority to other agencies was well taken: the only non-nomenclatural bodies mentioned in the Code, which all had at least some involvement with nomenclature, were the IAPT, the International Botanical Congresses, and the IUBS. The mail vote used to be better balanced geographically than attendance at Section meetings, and it was likely to reflect a wider experience with theses in various countries than was assembled in the room. The nature of theses varied widely from one country to the next. In Germany and Switzerland, for instance, most universities still demanded the submission of 50 printed copies before granting a doctoral degree. In other countries, a substantial number of copies had to be printed or duplicated ahead of the defence of the thesis, and distributed to the audience of that defence, again resulting in effective publication under the Code. In other countries, the candidates had to produce various numbers of more or less identical copies of the manuscript for review by the supervisors. The situation was too complex to be solved by the simple criterion of presence of an ISBN. It had been stated repeatedly that anyone could obtain an ISBN number, but all who had confirmed that this was so were from the U.S.A. or the U.K. It was unlikely that the situation would be the same in Russia, Cuba, or China. The proposed criterion was likely to be seen as discriminatory and prohibitive for authors in these countries, and this concern had been the basis of the Rapporteurs’ remarks. Incidentally, the ISBN system, being outside of the Section’s control, might be abolished, changed or renamed at any time, making the proposed rule obsolete. He appreciated that there was a problem, which was particularly obvious in the case of theses but in no way restricted to them. A possible solution might be found by rephrasing the final part of the proposed text, eliminating direct reference to ISBN from the provision itself, to read: “... is not effectively published unless it includes an explicit statement to the effect that it is regarded as a publication by its author or publisher”. An example could then be added, to the effect that the attribution of an ISBN was accepted as equivalent to such an explicit statement. Relocating the mention of ISBN to an example might take care of Dorr’s concern, and the suggested rewording might fulfil the Section’s wishes in a more general way than the present proposal did.

Farjon, accepted Greuter’s suggestion, again explained, as a friendly amendment to his proposal.
Faegri feared this to be an extremely dangerous proposal, as it would give authority to the author to declare that his one copy was to be considered effectively published. There would be no restrictions.

Greuter replied that the single-copy issue was covered separately in the Code.

Voss had heard it stated that University Microfilms, a private company located in his town but not associated with his University, did consider publication of theses on microfilm to be publication. This was explicitly forbidden in the Code – another little problem that had to be considered.

Snow favoured the proposal as modified by Greuter, requiring a statement of intent, but was concerned over the place in a thesis where this would be expressed. Ten years on one might wish to verify this, but the statement of intent might be hidden in the introduction, perhaps in a foreign language, or it might be difficult to obtain a copy of the entire thesis to search. He suggested a modification, stating that use of the Latin terms “comb. nov.,” “gen. nov.,” etc., or their equivalent, were considered as a statement of intent to validly – sorry, effectively – publish a name.

Farjon did not consider Snow’s suggestion as a friendly amendment.

Reveal reverted to Gandhi’s remark, that the problem preceded the year 2000, and moved that mention of the year 2000 be deleted.

Greuter hoped this would not be friendly amendment as it would retroactively invalidate many names in published theses. The date had been introduced very deliberately by the proposer. To make the proposal retroactive would be extremely destabilising.

Farjon concurred, and Reveal withdrew his motion.

Gandhi objected that, while retroactivity might be destabilising for some theses published in Europe, non-retroactivity would be destabilising in the U.S.A. All these years, [Gray Card] indexers had assumed that theses were not effectively published. Failing retroactivity, it would be necessary to go back and check many theses for the possible inclusion of new names in them. The date should be removed.

Kolterman referred to McNeill’s statement that the date might be changed from 2000 to 2001. It was preferable to have a starting date posterior to publication of the new Code, as many students who were about to publish their theses were not present.

McVaugh felt that much of the discussion had missed the point. Effective publication had in the past been defined as publication that was
available to other botanists who wished to use it. It was understood that a certain number of copies must be made available to the public, although a firm consensus as to how many copies had never been reached — certainly at least a few. The primary objective of effective publication was not that the author intended that it be published, but that the names in it were available. Presence of an ISBN did not by itself result in making names available, as it did not relate to copies being made available to the public. He would vote against the proposal unless it made that point, which was the essence of effective publication.

**Greuter** reminded McVaugh of the basic provisions in Art. 29.1, where the conditions under which Art. 30 could apply were set out. As it was now worded and intended, Prop. D was a supplementary restriction to those already in Art. 29.1. It affected the special case of theses or other works submitted to obtain a university degree. McVaugh’s concern was justified, but was outside the scope of the present proposal. A similar concern had been expressed over microfiches, also a problem of long standing, which the mention of microfilm in Art. 29.1 was intended to cover. Addition of an example under Art. 29.1, making it apparent that publication on demand made from microfilm was not effective publication, might be useful; if the Section so wished, this could be done editorially. This point was not covered explicitly in the Code, but it had always been assumed that production on demand, from microfilm, of individual paper copies was not effective publication.

**Demoulin** contradicted Greuter’s statement that it would be destabilising to suppress the date 2000. He supported that deletion, because it would remove the menace of much greater instability. If McNeill’s interpretation of Art. 34 was accepted, the floodgates would stand agape for some Professor McGinty searching for old roneographic copies of theses, in small universities, with terribly destabilising results. It would be less destabilising to lose a few theses that were presently admitted. Anyway, as the proposal now stood, i.e. without the ISBN number mentioned, it would not necessarily rule out theses presently accepted as effectively published, which clearly had been intended for general distribution.

**Orchard** strongly favoured the intent of the proposal, but was concerned about its last part, as it provided a loophole for theses to be accepted if they had an ISBN. He would be more comfortable if all words following after “effectively published” were deleted, making it clear that no works that were primarily theses were effectively published.
Freire-Fierro noted that when a description of a new species was sent to a journal, it would likely be edited by someone who knew the rules of Latin and nomenclature. A thesis committee might be less familiar with these aspects, allowing the publication, in a thesis, of names of new taxa that did not comply with the rules. This might create confusion.

Rabeler added that the selection of lectotypes and neotypes in dissertations was a matter for indexers to look for, if theses were effectively published. If only a few copies existed, it would be difficult to access them.

Gandhi noted that since 1970 the Gray Index had been listing all new type designations. If it were now necessary to go back and check theses for such acts, this would open the floodgates.

Vincent moved an amendment to Farjon’s proposal, that everything after the word “published” be deleted. [The motion was seconded.]

Farjon opposed the motion. To consider any and all thesis as not effectively published would eliminate dissertations in many countries in which they were published according to normal standards. There appeared to be no reason why the Section should do away altogether with the possibility of publishing new names in a PhD thesis.

Friis stressed Greuter’s point about the great diversity of dissertations. Eliminating the starting date from the proposal would invalidate, among others, all names published in the dissertations of Linnaeus’s pupils.

Sousa opposed Farjon’s proposal [sic!]. In Mexico there were twelve schools in biology, each producing theses at the level of bachelor, master, and PhD. Theses reflected a learning process and were not finished products. Considering them as publications was very dangerous and would require a change in the way of thinking in the schools producing theses in taxonomy. Also, the theses were paid for by the students: some might produce 10 and others 100, depending on the money they had.

Jansonius urged that the word “thesis”, which obviously had different meanings for people from different parts of the world, be clearly defined. It was essential to know what was being discussed. Some theses at present were definitely effectively published, but others, run off on a xerox in five copies, were definitely not.

Greuter requested that the debate return to the motion. The proposal, as amended by the motion, still included the starting date: it was for the future, not for the past. The amendment would rule out, for the future,
that names could be validly published in theses. In future, even if 100 copies of a thesis were distributed through a bookseller, as happened in Germany, e.g., with the series *Dissertationes botanicae*, they would not be effectively published for the purposes of the *Code*. That amendment, to delete everything after "effectively published", had to be acted upon first.

**Faegri** had difficulties in accepting the proposal as amended. There were indeed theses that were parts of regular publication series. Addition of the word "thereby" before "effectively published", or some other rewording to the same effect, was necessary to ensure that such regularly published theses were not automatically disqualified.

**Phillipson** disagreed with Faegri's point. When a thesis was published in a more formal way, it was no longer an "independent publication", so the proposed new text would not apply to it.

**Woodland** heartily supported the amendment. He used to tell his students that going through the process of valid botanical publication was a very important aspect of scholarship. Just preparing an individual master, or bachelor, or PhD thesis or dissertation should not result in something that was validly published. The amended text was the best statement that could be found to deal with a problem going across the broad spectrum of cultures, languages, and countries.

**Vincent's motion** was defeated.

**Farjon** restated the wording of his proposal, including the friendly amendment. After the word "published", it now read: "unless it includes an explicit statement to the effect that it is regarded as a publication by its author or publisher". The phrase "unless it bears an ISBN number" had been deleted. Instead, the Editorial Committee would add, as an example, a thesis with an ISBN number.

**Stuessy**, in view of the failure of Vincent's motion, moved that Farjon's proposal be substituted by the following: "On or after 1 January 2001 theses produced and distributed locally and presented to a university or other institution of education with the objective of obtaining a degree are not effectively published". This would remove the [former] reference to an ISBN number. Instead, the criterion for effective publication would be more than just local distribution. [The motion was seconded.]

**Greuter** asked how one was to established whether a thesis was only distributed locally.
Stuessy thought this to be a good question. [Laughter.] He would regard distribution to the degree committee only to be clearly local; distribution by sale or in a regular serial publication would not be just local.

Barrie still wondered how locally was defined. His thesis had been presented in Austin but a copy was sent to University Microfilms in Ann Arbour. Was that still local distribution? [Laughter.]

Gandhi pointed out that in India it was customary for one examiner to be from a foreign country, often Australia or Britain, to whom a copy of the thesis would be presented on conclusion of the examination. This was certainly far more than local distribution.

Farjon opposed the motion. It would be very difficult to define “local”.

Palacios-Rios was unhappy with the motion. In Mexico and other Latin American countries, some new species were described in bachelor theses distributed very locally, often only within the state, in 10-40 copies. The students knew that they were expected to publish in journals or regular works, and did not consider their theses as effective publications.

Stuessy recognised that the term “local” would not work and withdrew his motion. There did not appear to be a clear solution. He wondered if “theses of limited production and distribution” would be more acceptable. [Laughter.]

Hawksworth observed that the debate had been making an excellent case for registration.

McNeill called for support of Farjon’s proposal. He had aired the problems to define effective publication before, and had found they were very serious. The proposal, as now rephrased and with the ISBN number relegated to an example, would work, and so would plug at least one of the many important problem areas.

Orchard was still worried over the proposal as it stood, as every publisher would consider what he produced a publication. He moved an amendment, to add the words “for taxonomic purposes” after “published” and ahead of “by its author and publisher”.

Farjon considered Orchard’s amendment as unnecessary. Botanists knew what was meant by effective publication in the context of the Code.

Greuter dispelled Farjon’s misunderstanding of what was being proposed: the additional words would come in after the last, not the first mention of “publication”. His concern was that if anyone forgot to use
the words “for taxonomic purposes”, the thesis would be rendered ineffectively published in spite of the author’s stated intent. This would be splitting hairs. [Orchard’s motion was not seconded.]

**Farjon** confirmed his agreement with the suggested change of date, from 1 January 2000 to 1 January 2001.

**Phillipson** suggested a further amendment, to change the wording to “is not effectively published unless it includes an explicit statement to this effect by its author or publisher”.

**Farjon** did not feel the change to be really necessary.

**Gandhi** requested a clarification. Did acceptance of the proposals mean that publication of all names and type designations included in theses prior to that date was effective?

**Greuter** confirmed that, if adopted, the provision would take effect as from 1 January 2001. It would not affect the status of names published prior to that date, for which the situation would remain unchanged. Problems with the status of works of the past, and there were many, would have to be addressed and solved separately – certainly not by discussing them at a meeting of the size and complexity of nomenclature sessions. [Laughter.] The matter would have to wait until the next Congress.

**McCusker** regarded a change of phraseology as critical: to replace the words “presented to a university or other institute of education with the objective of obtaining a degree” by the phrase “accepted by a university ... for the purpose of awarding a degree”. If a student’s thesis was not accepted by a university, it would have no status with the university and never find its way into the university library or be distributed by the institution. This was more than a mere editorial improvement.

**Farjon** found it hard to decide whether it was desirable to require that the thesis be not only presented but also accepted by a university. If the suggested change could help to get the proposal approved, he would not object.

**Nic Lughadha** explained that in most cases, certainly in the U.K., one could not tell by looking at a thesis whether it had been accepted or not. She did not favour the amendment.

**Farjon** did not accept McCusker’s motion as a friendly amendment. [It was, however, seconded.]

**Middleton** spoke against the amendment. In the Netherlands theses were published and widely distributed prior to examination. Names in theses
might be effectively published and widely distributed to libraries, for example in a *Blumea* supplement. In the unlikely event that the thesis was subsequently rejected, the names in it would suddenly lose validity. **McCusker's motion**, having been read our again, was **defeated**.

**Nee** felt that, in spite of the long argument, there was general agreement on a number of points. The main one was to restrict the number of theses that included new names. However, in spite of their merits, none of the proposals or amendments would really effect what was required without adverse consequences. An Article was needed that would state some of the following: “Publication of a thesis ... presented to a university is not effectively published, unless ...”, placing the onus to fulfil the stated conditions on the student. Examples of effectively published theses could then be provided, e.g. Linnaean theses, Dutch and German theses distributed in large numbers. This would effectively cut out all New World and Indian theses which, he understood, neither the professors nor the students would regard as effectively published. Such an Article, with a limited number of examples, would do a better job.

**Greuter** explained that Nee’s suggestion, which was not presented as a formal motion, foreshadowed future moves to restrict the effective publication of the theses of the past. It did not impinge on the vote on the present proposal, on theses to be presented in the future.

**Prop. D was rejected** on a card vote (354 : 349; 50 % in favour).

*The following action was taken during the Eighth Session on Thursday afternoon.*

**Keil**, considering the defeat of Art. 30 Prop. D and the registration proposals, and in view of the uncertain status of names and typifications proposed in theses, dissertations and other so-called grey literature, moved to establish a Special Committee, to study the question of effective publication of such literature.

**Keil's motion was seconded** and **carried**.

*The following matter, relevant here, was brought up during the Sixth Session on Wednesday afternoon, after the tea break.*

**Recommendations 30B and 30C (new)**

**Gams** moved that two Recommendations be added, to be numbered Rec. 30B and 30C. [The texts were shown on the screen. “Rec. 30B.
Authors are urged to publish new taxa in taxonomic and related journals. Otherwise they are urged to submit their work to the respective indexing centres.” “Rec. 30C. Authors and editors are urged to make sure that new taxa and new combinations be listed in the summaries of their work.” Both motions were seconded.

Gams had been stimulated by J. C. David, editor of the Index of fungi, to make these proposals. When asked whether registration would help in producing the Index or would duplicate his work, David had replied that registration would help, but that its main benefit would be to provide easy access to all publications containing new names. This was what was the proposed Recommendations expressed.

Greuter queried the proposed placement of the recommendations, which would be carefully examined by the Editorial Committee. He had no answer ready, but they seemed rather misplaced in a chapter on the condition and dates of effective publication. The suggested placement should be considered as provisional. The proposals themselves, especially the first, made perfectly good sense.

Buck opposed Rec. 30B. Taxonomists working in small institutions should not be discouraged from publishing in their institutional journals, which might be general biological or general science journals. This was often their only possible outlet. When questioned, he confirmed that he was well aware of the proposed second sentence, but that it did not disclaim the urging expressed in the first sentence when it was unreasonable.

Barrie suggested that, to take care of that concern, “urged” be changed to “encouraged”, and the first sentence omitted. [This became Buck’s motion, which was later seconded.]

Gams accepted the change to “encouraged” as a friendly amendment, but not deletion of the first sentence as it would eliminate the essence of the proposal. As the first sentence was less stringent, “encouraged” should go there, and “urged” be left in the second sentence.

Nic Lughadha, to avoid the difficulty of seemingly discouraging authors from using their home journals, suggested a change from “taxonomy-related journals” to “journals which regularly publish taxonomic articles”. The important point was knowing where to look for names of new taxa, which need not be in a journal devoted to taxonomy alone.

Gams accepted this as a friendly amendment.
Hawkesworth would have added “and publishers” after “authors”, so as not to leave the whole responsibility with the authors.

Greuter enjoyed watching an audience of 250 doing an editorial job. This was a fascinating experience. [Laughter.]

Freire-Fierro wondered where the appropriate indexing centres might be.

Greuter thought that, as she was working with Monina (Polygalaceae), this would be the Royal Botanic Gardens, Kew. [Someone from the audience corrected this as now being Harvard.] At any rate, addition of “if any” after “indexing centres” might be appropriate. [Laughter.]

Keil suggested that a Note listing the indexing centres be added.

Gereau, referring to Greuter’s doubts, thought he had found the correct placement for the proposed Recommendations. They belonged in the list of proposals on Art. 32 previously withdrawn, and should be likewise either defeated or withdrawn. They meant registration by the back-door.

Voss liked the looks of the [amended] proposal on the screen, except that it was very incomplete. Why were authors encouraged to publish only in journals? What of theses, monographs in book form, etc.?

Henderson wondered whether the recommendation should apply only to names of new taxa. Were new combinations not to be covered as well?

Marhold countered Gereau. There was nothing wrong with sending publications to indexing centres. This had nothing to do with registration, but with easing access to the data.

Keil supported Henderson’s suggestion and, in addition, wanted to include new type designations as they were also of concern.

W. Anderson, as one who had opposed compulsory registration, agreed with Marhold that there was nothing odious about asking people to send their publications to indexing centres. This could be extremely helpful to indexers. Buck’s simplified version was better than the original one.

Greuter felt that in view of the foregoing debate it would be wise to return the text to the proposer and ask him to come back with an improved version, taking into account the proposed amendments, unless he should give up in despair. Meanwhile, both the original text and the amendment motion would be considered as temporarily withdrawn.

Gams agreed, but wished some discussion of his second proposal.

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Dorr liked the proposed Rec. 30C, but suggested an amended wording for it: “Authors and editors are urged to list the names of new taxa, nomina nova, and new combinations in the summary of the publication.” This was accepted as a friendly amendment by the proposer.

Frodin suggested a further simplification: “Authors and editors are urged to list new taxa, new names and new combinations in the summary of the publication.” Juxtaposing “names of new taxa” and “new names” was grammatically unsound.

Greuter objected that one could list names but not taxa.

Demoulin suggested the phrase “taxonomic novelties”. [This was corrected from the audience to “nomenclatural novelties”].

[Continuation and conclusion of the discussion took place during the Eighth Session on Thursday afternoon.]

Gams reintroduced his proposed new Recommendations 30B and 30C, postponed from Wednesday afternoon. The first had definitely nothing to do with registration. It would benefit the indexing centres and make their work as efficient as possible. Considering some alternative wordings received, the revised version of Rec. 30B, displayed on the screen, now read: “Authors proposing new names or new combinations are urged to choose periodicals that regularly publish taxonomic articles or else send copies of their work to the appropriate indexing centre(s).”

W. Anderson noted that one of the problems raised yesterday subsisted. Many nomenclatural novelties were not published in journals but in monographs and sometimes in Floras. The wording was too narrow.

Gams explained that books would fall under the second clause of the recommendation.

Gereau considered this an innocuous and helpful proposal as presently worded, needing no further amendment. The Editorial Committee would reflect upon what the Section had said. He called for the question.

Hawksworth had previously suggested addition of “and publishers” after “authors”, but was satisfied by Gams’s assurance that publishers were covered by the second proposed Recommendation.

Henrickson restated the desirability of adding a Note with the addresses of the appropriate indexing centres (or, better perhaps, indexing projects).

Gams replied that most people would know which indexing centres were appropriate.
G. Yatskievych disliked the idea of placing the whole onus on individual authors. Many large books had multiple authors. Individual authors, who might produce but a small portion of a book, would not want the responsibility for sending in the whole work. Hawksworth’s suggestion, to write “authors and editors”, adding perhaps “publishers”, should be taken up.

Hawksworth agreed. The Recommendation, ideally, concerned authors, editors, and publishers alike.

Greuter advised to keep the text simple and not making it worse by patching it up. Editors did not introduce new names or combinations. This was advice, not a mandatory provision. An author publishing with others might ask the editor or publisher to send in a copy, but this would still need the author’s initiative. Perhaps “a copy” would be preferable to “copies”, which might look as if multiple copies were intended. This and any other linguistic amendments could be left to the care of the Editorial Committee.

W. Anderson had one more linguistic suggestion. Nowhere else in the Code was the word “introducing” used in the proposal’s sense. Botanists talked of publishing names, or proposing them.

Greuter promised that the point, which was well taken, would be addressed by the Editorial Committee.

Fife endorsed Henrickson’s advice to list the “appropriate indexing centres”. It might be true that a majority of authors knew where they were, but others would be confused. It would be a simple matter to provide an example listing the indexing centres.

Barrie feared that listing indexing centres in the Code might imply some kind of official endorsement by the Section. The matter required careful consideration.

Greuter said that the Editorial Committee would consider the suggestion, but might decide not to implement it.

Friis wondered whether it would be appropriate to mention lectotype designations: “Authors proposing new names, new combinations, or lectotypifications ...”

Greuter replied that this might depend on the eventual placement of the Recommendation, which was perhaps misplaced under Art. 30, dealing with effective publication, and better at home under Art. 32 on the valid publication of names.
Kirk pointed out that “nomina nova” were not mentioned. The phrase “nomenclatural novelties” would cover it all.

Greuter agreed that “nomenclatural novelties” was probably preferable; again, an editorial point.

Korf asked whether it was the intention to include lectotype and neotype designations, etc.

Greuter replied that, if the Section so wished, it should instruct the Editorial Committee to introduce parallel Recommendations under Art. 32, and presumably Art. 9. The usefulness of such a double Recommendation was doubtful: probably no editor or author would care.

Zijlstra suggested placing the Recommendation after Art. 29. Lectotype designations must be effectively published.

Greuter responded that the appropriate placement was part of the Editorial Committee’s mandate. His earlier remarks had only been guesses.

Gams’s first motion was carried.

Gams introduced his second motion, which was seconded. It also was designed to facilitate the work of the indexing centres, and it now read: “Rec. 30C. Authors and editors are encouraged to list nomenclatural novelties in the summary, abstract or table of contents of the publication.” With regards to type designations, he knew of no indexing centre listing them as yet. It might be appropriate to point them out to the Bibliography of systematic mycology, and comparable bibliographies for the higher plants, but not to the indexers.

Zijlstra pointed out that the Index nominum genericorum did list type designations and welcomed any such information.

Stuessy felt that “index” would be more appropriate than “table of contents”; or else, one should directly state: “title of the publication”.

Gams accepted the change to “index” as a friendly amendment.

Gams’s second motion, so amended, was carried.

[Here the record reverts to the normal sequence of events.]

Article 32

P. Stevens presumed that the Editorial Committee would ensure that there was no mention of registration left in the Code.

Greuter replied that, without an explicit mandate, the Editorial Committee would certainly not feel entitled to delete wording now in the Code.

Turland moved that all three references to registration be deleted from the Code, i.e.: from Art. 32.1, the clause: “in addition, subject to the approval of the XVI International Botanical Congress, names (autonyms excepted) published on or after 1 January 2000 must be registered”, Art. 32.2, and Art. 45.2. [The motion was seconded.]

Greuter considered this “very interesting suggestion” to be quite far reaching for a motion introduced from the floor. He seized the opportunity to explain why the proposals on registration had been withdrawn. The first reason was his concern over the way in which the Section had been considering a proposal [that on Art. 15], the day before, refusing to listen to any arguments. This was not what he considered as normal democratic procedure. He used the word democratic deliberately, as in a different context his understanding of democracy had recently been questioned, which anyone was entitled to do but which he felt free to resent. As a citizen of the oldest extant democracy in the world, Switzerland, he felt entitled to his personal view of democracy: deciding by majority vote but in full cognisance of the pros and cons of a cause, having listened to everyone’s arguments. The second reason to withdraw the Art. 32 proposals was his and his co-proposers’ recognition that the time was not yet mature for making registration mandatory. There must be a consensus among those applying the Code to implement new provisions, and if this did not exist it was purposeless to introduce and try to enforce a novel concept. At the Tokyo Congress (as those who had read the Proceedings would know) there had been consensus on the usefulness and appropriateness of registration. He and others had then said that it was appropriate to ask the St Louis Congress to reconfirm what was then generally agreed. The Section in Yokohama had believed that if feasibility of registration was demonstrated, if a system that worked would be in place by the time of the next Congress, registration would then as a matter of course be adopted; but that the St Louis Congress would decline ratification in the event that registration proved unfeasible, that its mechanisms were unsatisfactory or did not work. There had been a swing of mood, in parts of the world, on the appropriateness of registration, and it was wise to postpone decision by six years.
But why, now, delete registration from the *Code*? Why reverse a decision taken six years before? The reversal would be decided by a Section which could not consider itself unbiased, nor could it avail itself of advice from the world at large. The taxonomic community had voted on the proposals as they stood: to make registration mandatory by the year 2000; it had not been asked to express its opinion on registration per se. He hoped that the registration provisions would remain exactly as they now were in the *Code*, with only the date editorially postponed by six years and the Congress renumbered one higher. This would in essence mean that (1) the XVII IBC would reconsider registration on its own merits; and (2) there would be a continued mandate for the IAPT, and others co-operating with the IAPT, to test and improve the practice of registration, perhaps to make it acceptable in six years’ time.

The Section ought to consider why six years ago a gathering of botanists, admittedly smaller than the present one, favoured the concept of registration almost unanimously, but now, six years afterwards, it should be an anathema – to be weeded out of the Code. When W. Anderson, earlier in the meeting, had used the word “cancer” it was in a different context, but he might well have had registration in his mind. But there was no reason to be emotional. The withdrawal of the registration proposals meant that registration would undergo a new test period, six years to find out whether it was a good idea, as many in the bottom of their hearts knew it was. The cheers that Turland’s motion had raised were hardly appropriate.

Funk took issue with the statement that the taxonomists of the world had not spoken on this issue. The mail vote had been overwhelmingly against registration. There had been less than 100 people at the Tokyo nomenclature sessions while there were almost 300 at St Louis – a far wider representation of views now than was the case then. Also, the actual proposal to implement registration at the last Congress had been defeated. What was passed was a watered-down version to allow a trial period. The last Congress in Yokohama had actually voted against the recommended form of registration.

Greuter pointed out that this was untrue, as Funk perfectly well knew. There had been no vote in Yokohama on the original proposals, which had been amended with the active support of the proposers themselves. Funk did not press the argument. Her point was that 73% who voted in the mail ballot had voted against registration. She called for a vote on Turland’s motion.
Hawksworth drew attention to the low participation in the mail ballot. Evidently those not responding had no strong feelings against registration. Also many users of names were not vascular plant taxonomists, and many of them were not members of IAPT [and were not therefore entitled to vote]. Talking about democracy, registration had not been voted upon by the wide range of people it concerned.

Nic Lughadha had noted Greuter’s statement, earlier that morning, that the mail vote was geographically better balanced and therefore more representative of the taxonomic community than the Section. If the mail vote had been against registration and the Section was, too, this should suffice.

W. Anderson found it a curious way of handling democracy that when an overwhelming defeat was faced the decision should be put off until a day when one might win. That was good strategy, but not particularly democratic. [Applause.] If the Section eliminated every mention of registration from the Code now, and in six years should decide that this had been a mistake, it would be a simple editorial matter to put it back. He would not mention cancer again, but wished to talk of vampires. [Laughter.] The only way to kill a vampire was to drive a stake through its heart: “let’s do it”.

Trehane felt the argument was getting a bit heated and might turn into an entertaining discussion. There were two developments in the background that should be borne in mind. First, IAPT was going through a change of stewardship: its new leadership would be looking into the effects of all sorts of options in nomenclature. Secondly, the Plant Name Project was about to start. Hopefully the various initiatives might be brought to merge, and in six years’ time registration might be implemented as part of a package that would bring the concept closer to the world. IAPT should take time to consider the various options and come to a solution acceptable to everyone at the next Congress. It was therefore desirable to maintain the status quo, insofar as in Yokohama the Section had voted for the principle but not for the implementation. The mail vote had opposed implementation at the moment, but he hoped that registration would be left on the table for due and careful consideration in six years’ time.

Faegri fully agreed with the Rapporteur’s point of view, even if with sorrow in his heart. Withdrawal of the proposals and maintenance of the status quo was the only possible course of action. He had been the Chairman of the Committee who made the registration proposals, and it
was the wording of that Committee which had been passed. In discussions during the last six years, he had again and again been amazed at the immensity of the misunderstandings surrounding the registration concept as envisaged. He would probably not be at the following Congress, but he hoped that the very measured procedure indicated by the Rapporteur would be accepted here.

Voss had before him the Report of the Tokyo Congress [in Englera 14], which he had not attended. Page 151 of that Report indicated that a 60 % majority would be required once more at the next Congress to confirm the principle of registration. Greuter had not been afraid of a second 60 % threshold: “If in six years’ time the system was set up was seen to be functional, and if money had been invested into it, the Section would certainly once more endorse registration.” He therefore assumed the Section was being asked to endorse or not to endorse registration.

Demoulin, having been to all Congresses since that in Leningrad 24 years ago and, knowing all the discussions on effective publication and the registration concept, failed to understand how this could have become such a passionate issue and given rise to so much opposition. At preceding Congresses registration had always been seen as a way out of the problems with locating names in grey literature and having forgotten names that kept cropping up. Registration was a matter of record and had never been envisaged as the kind of censorship that some seemed to fear. Registration was a recording process that would make life easier for those working in taxonomy, ensuring that they would be aware of the names they needed to check. Registration was a different issue from Names in Current Use. One could understand people being afraid of lists set up by people they might not agree with – but registration involved no decision; it only recorded that a name existed, and so helped the taxonomic community. As Faegri had said, there had been a misunderstanding, and another six years were needed to sort out that misunderstanding.

Gams observed that as the registration proposals had been withdrawn and if the Turland motion should pass, the Section would be left with nothing, which would be regrettable. The activities surrounding registration had had many positive aspects. An enormous amount of personal effort and financial resources would have been wasted if the Section went away without any positive action. To have indexes of plant and fungal names such as Index kewensis, the Index of fungi, etc., had always been most beneficial. More indexes were needed, so the funds
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now available for registration should be channelled into the production of indexes. He would also propose two recommendations as to procedures not yet provided for in the Code. [See Rec. 30B and C, above.]

Gereau, as a citizen of a democracy admittedly much younger than that of the Rapporteur-général, but one he fancied as relatively mature, had his own understanding of the democratic process. Reference had been made to consensus. Consensus, meaning 100% agreement of all participants, was not a founding principle of democracy. Democracy, at least in the U.S.A. and in the context of IAPT, worked by an agreed-upon majority under specified rules, coming to a conclusion, and implementing it. A motion had been made from the floor, which was perhaps sweeping but very specific, for the removal of specified Articles and specified wording from the Code. That motion had been seconded. The question had been called. That in turn had been seconded [?]. The Section should now vote on Turland’s motion.

Greuter, upon request from the audience, explained that the motion was to delete from the Code all provisions related to registration that had been introduced in Yokohama. Those who wished to kill the vampire should vote yes. Those who wished to give registration a chance to be reconsidered in six years’ time should vote no. The question had been asked, and if a majority so wished there would be no more discussion before the vote. However, he was not sure that everything had been said that was relevant. He doubted that all those present had taken the trouble to look into the registration database and see for themselves what registration demonstrably could achieve. He feared that specialists in groups that had no such stable permanent indexing system as vascular plants or fungi had not been heard at all.

Zander had been talking to a reporter yesterday who had asked what the meeting was doing. He had replied that it was working on the rules that allowed one to get a good understanding of all the plants of the world. This was done by meeting every six years to deal with those rules. Botanists wanted to get to a final place as quickly as possible and with the fewest possible accidents, i.e., mistakes. As all would realise, he had lied. Many seemed more interested in the ride, fixing their cars or looking into each other’s cars, than in arriving. Those who felt that registration would maximise progress and minimise faults should vote for it, those who saw it as an impediment that would not bring the speed up should vote against. “But by all means, ignore the guy with the loud muffler.”
**Turland's motion** was carried on a card vote (461:251; 64.8% in favour).

**Prop. H** (10:176:6:30) was ruled as rejected.

**Recommendation 32 C**

**Prop. A** (41:59:117:2).

Greuter explained that the heavy "ed.c." vote had a special meaning: that the Recommendation not be deleted, as had been proposed, but salvaged by a factual improvement, as suggested in the Rapporteurs' comments. In fact, a somewhat more elaborate rewording of the Recommendation was needed, which would then read: "When naming a new taxon authors should not adopt a name that has been previously but not validly published for a different taxon". So worded, the Recommendation would make perfect sense.

**Prop. A**, so modified, was accepted.

**Recommendation 32 G** (new)

**Prop. A** (52:161:4:3) had been withdrawn.

**Article 33**

**Prop. A** (81:124:8:2).

Greuter pointed out that the Rapporteurs had commented favourably on this proposal, but had apparently failed to convince those voting, as the mail vote had been negative (58% "no").

Brummitt explained that the *Code* as it now operated was fairly precise, imposing detailed conditions when a nomenclatural act was performed; but it was being applied to names published in the last century, when there was no such guidance and nomenclatural practices were lax and differed from those of today. The *Code* required that reference be made to a basionym when a new combination was made. If one applied this literally, one cut out a number of names that, when published, would have been regarded as new combinations, although there was no actual indication of the basionym. Similar proposals had been discussed at the last two Congresses, but had been rejected because their wording was defective. The new proposal resulted from detailed discussions with Zijlstra. Greuter had agreed that its wording was appropriate to the *Code*. It would apply common sense in a number of significant early situations.
Gandhi favoured the proposal which [as he believed] applied to pre-1953 names only. It could hardly cause any destabilisation of names, only some changes in authorship and bibliographic citation for a small number of names would result from its adoption.

Thulin strongly supported the proposal, which gave clear guidance in an area where guidance was badly needed. The business of so-called “nude combinations” was a moot point in the present Code. Had this provision been in the Code before, it would have saved 40 years of debate over the name Scaevola taccada.

Greuter noted that all comments so far were favourable – but many had voted against the proposal so someone might want to speak against it.

McNeill had problems with the proposal, which would be an addition to Art. 33.2, starting: “A new combination or an avowed substitute published on or after 1 January 1953 ...”. It was not clear whether or not the new clause would apply only to names published after 1952. If it would, it would not be applicable to the example given by Friis; if it would not, clarification of its range of application was needed.

Greuter drew attention to the Rapporteurs’ comments. The proposal clearly addressed pre-1953 situations in the first place, not only post-1952 situations. If the new clause were placed as presently suggested, this would indeed not be obvious: editorial clarification was needed.

McNeill, accepting that the provision would deal with past situations, was still concerned over the rather vague, the bit woolly phrase “in a closely related taxonomic position”. He knew of a specific example where it was really a matter of judgement whether or not the taxon was closely related, where probably no new combination had been intended, but it could be argued that it had, as the named taxon and its potential basionym were in the same genus and even the same section.

W. Anderson was embarrassed that he had not picked up on the problem of placement. He hoped that the proposers would entertain some friendly amendment to solve that problem, because a real problem it was, of the kind one kept in the closet and did not talk about. Many had accepted old combinations that did not meet the Code’s requirements if they were strictly read, and the potential for mischief and destabilisation was tremendous if someone started holding all those old combinations to a strict rule. The proposal would clarify the situation in a way that would confer stability to names carried along in good faith and for good sense.
For Zijlstra it had been self-evident that Prop. A only applied to pre-1953 names. She had not spoken with Brummitt but supposed he would agree. If this was not clear, the proposal might be amended by adding, at the beginning, the phrase: “For names published before 1 January 1953”.

Wiersema noted that most speakers took it for granted that the standard practice, in such cases, had been to treat the names as new combinations, but he was not sure that this was true in all cases. There would be some cases of destabilisation, where a name had not been treated as a new combination and treating it as such would now change priority and result in a change of names. The proposal was advantageous by giving necessary guidance as what to do in such cases, but it did loosen the requirements for validating new combinations beyond the current reading of the Code.

Gandhi agreed with Zijlstra that the provision should be made to apply only to pre-1953 “comb. nov.” and “stat. nov.”. Contrary to Wiersema, he did not believe that any name change would result, only changes of authorship and bibliographic citations, in a very small percentage of cases. Some clarification under Art. 32.5 of “indirect reference” would also be helpful.

Greuter wished to hear from Brummitt as to the period for which the new provision was supposed to apply. From the context it was obvious that it was also to apply to pre-1953 names; but to his mind it should be applicable irrespective of date.

Brummitt replied that after 1952 the requirements were very stringent, so the proposed provision could only apply to pre-1953 names. He had no objection to clarifying this by specifying the date, as it made no difference to the proposal. There were other provisions in the Code covering what came after 1952. One example mentioned to him by Zijlstra might illustrate the sort of problems encountered: a whole book where author citations were not used, but descriptions were given. It contained perhaps 600 new names which one used to count as new combinations. If they were not accepted as such, all would count as names of newly described species and have to be listed anew in standard indices, with possibly disastrous consequences.

D. Ward suggested that adding three words would take care of the problem, “if for such combinations no reference to a basionym is given ...”; that would tie the two situations together [but not in the sense of Brummitt’s intention].
Greuter was confused by Brummitt’s reply. There were, he thought, two situations in which the provision could and should apply: before and after 1953. Before 1953 there was the Kummer example, presumably the one alluded to by Brummitt: a whole fungal book lacking author citations, where well-known species were described and placed in new or different genera. Without the new provision they were technically new species, because the binomials did not previously exist and there was no reference to the intended basionyms. The second situation would arise when, after 1953, an author made a new combination but failed to cite the basionym fully and directly, so it was not validly published as a new combination; however, at the same time the author might provide or cite a Latin description and refer to a type, as currently happened in monographs for any accepted name. In such a case, when the requirements for validly publishing a new combination were not fulfilled while those for to validly publishing a name of a new species were, the intended new combination would be the validly published name of a new species, with priority starting anew. Both situations were relevant. He had therefore automatically assumed that the proposers were aiming at both with one stroke, and that their placing the amendment under a paragraph applying “on or after 1 January 1953” was a fortuitous oversight that could be corrected editorially.

Greuter thanked Brummitt [who had agreed that this interpretation was correct], then turned to the second of McNeill’s objections, still unanswered: the phrase “in a closely related taxonomic position”. Could someone come up with a wording that might be acceptable as a friendly amendment? Perhaps along the lines of, say, “a name which by circumstantial evidence can be shown to apply to the same taxon”?

Brummitt had already mentioned that the same issue had come up at the last two Congresses but had been thrown out because of bad wording. He was fairly certain that the objection to his previous wording, which had caused rejection of the proposal, was use of the phrase “circumstantial evidence”. In reply to McNeill, he did not think an absolutely watertight wording could be found. If the proposal was accepted it would provide proper guidance in the great majority of cases. It might still leave a few cases in doubt, but would seriously reduce their number.

Zijlstra drew attention to the distinction between cases in which there was a description and those where there was none. Comparable proposals at previous Congresses had covered both cases, but Prop. A, as
specified in the last line of the proposal, was restricted to cases with a description, those that would otherwise be published as names of a new taxon. The book Brummitt had referred to [Kummer's *Führer in die Pilzkunde*] was the example she had used at a previous Congress, when the proposed wording still included the phrase “unambiguous evidence”. To her surprise she had found it in the *Tokyo Code* as Art. 32 Ex. 8. At first she had thought the Editorial Committee had made an error, but later she understood their decision that, even with no basionym author mentioned, there was still indirect reference, and she became happy with this solution. The proposal still covered the other cases in which one would otherwise have to conclude that the name was new.

**McNeill** suggested a hopefully helpful amendment that would solve his difficulty and would not, he hoped, create problems he had not foreseen: to add the words “to be intended”, so that the text would read: “... adopted in a new combination which is likely to be intended to apply to the same taxon”. It was the author’s intent that seemed to be important. It might be that at present the accepted taxonomic circumscription was very broad but that the author had thought differently.

**Zander**, having read the proposal ten times, believed he now understood it – but it desperately needed editorial attention and in any case must be split into two sentences. If it was passed, he hoped the Editorial Committee would make the meaning clearer.

**Greuter** promised that the Editorial Committee would do its best and hopefully produce a text that would satisfy everyone. It would also have to make sure that the new provision did not conflict with an earlier decision, taken he believed in Yokohama [in fact in Berlin, indirectly, through rejection of Art. 33 Prop. J], that citation with doubt of an apparent basionym did not make a new combination out of a name. The Committee would try to keep these two issues apart as best it could. The Section would have to place some confidence in the Committee, as it was not possible to formulate a perfect wording during the meeting. However, the proposal’s intent was clear and commendable.

**Veldkamp** found the word “likely” to be very vague. He would prefer, e.g., “obviously” or “clearly”.

**Davidse** still had concern about the retroactive destabilisation the new provision might cause. Perhaps not many names but certainly many author citations would change. The proposal reflected the concern over names now accepted as new combinations that might be names of new
taxa, but the reverse tradition was also to be found. He knew of cases in
the Mexican flora where some names had been accepted as applying to
new taxa which were intended as new combinations.

**Henderson** wondered whether, in the context of an Article as opposed
to a Recommendation, the verb “may” in the fourth line was appropri-
ate. Should not rules rather have verbs like “must”?

**Zijlstra** believed that in the kind of cases mentioned by Davidse, it was
best to consider that the new name was obviously not intended to apply
to the same taxon as the potential basionym. When it would be destabi-
lishing to apply the new provision, it should not be applied.

**Middleton** felt a bit confused. If, for post-1952 names, not all require-
ments had been met to validate a new combination, for example if the
page number or date had been left off, and someone later published the
same name for a new species, did the earlier combination thereby be-
come validly published?

**Greuter** explained that the new provision would work only for names
that were at any rate validly published, so they did exist in that particu-
lar combination and could not be legitimately republished later based
on a different type, which would result in homonymy. The question
was, were such newly published valid names new combinations or
names of new taxa. The basic differences were (1) that names of new
taxa had their own type, which might cause difficulties in rare cases;
and (2) that they would lose the basionym’s priority. The latter aspect
would often cause problems, as the potential basionym might be a Lin-
naean binomial and the intended combination date from the 1990s. If
priority started from 1990 not 1753, most likely some name published
in between those dates would take priority in the required combination.
In such cases the proposed provision would be stabilising.

**McVaugh** answered Henderson’s point on “must” versus “may”. There
was nothing in the rules that made it obligatory for anybody to publish a
new combination. The statement that a combination “must” be made
was wrong, and “may” was correct. It was possible for an author to
make this combination, but it was not obligatory.

**Greuter** predicted that the Editorial Committee would replace “may
be” by “is”.

**Demoulin** explained that the Kummer example referred to earlier was
now covered by the notion of “indirect reference” and arguably had
nothing to do with the present proposal. However, he now felt that the proposal was dangerous and premature in cases like Kummer’s, whose names had been accepted by mycologists for 40 years. The proposed provision might be interpreted as invalidating Kummer’s names.

**Prop. A was accepted.**

**Prop. B (155 : 41 : 19 : 2).**

*Greuter* was pleased of the 71 % “yes” vote in the mail ballot, as this was his proposal. It addressed a conflict, often disturbing and impossible to resolve, between two provisions in the *Code*: the correctability of errors of citation when a new combination was proposed, and the requirement that only the original and not a secondary source be cited. Prop. B and C taken together would resolve the conflict in such a way that (a) one would know what way to go, i.e. whether there was a correctable error or not, and (b) the answer was as fair and as stabilising as possible. He requested *Brummitt’s* opinion on this.

*Brummitt* was happy to support the two proposals. Prior to the Tokyo Congress a Special Committee on Bibliographic Errors had been set up which did a lot of work and had much correspondence – but unfortunately the Secretary of the Committee resigned from botany shortly before the Congress, and no report was published, so the matter was never resolved. Now Greuter had come up with good solutions – perhaps not be the last word, but certainly a great improvement.

*Gandhi* felt that care was needed as to the meaning of the word “omission” in Prop. B. Obviously, at least one part of the basionym citation should be correct, either the basionym itself or the bibliographic reference. If both were incorrect, this did not constitute valid publication. There were however examples where the author of a new combination, perhaps by oversight, had omitted the citation of the basionym epithet, the volume number, or the year of publication. Such omissions resulted in invalid publication, but this was inappropriate in his opinion, because except for one detail all the required information was provided, and it was easy to go back and find the missing detail.

*Greuter* explained that the word “omissions” was not used generally and out of context, but was given a clear meaning by specific reference to Art. 33.2.

*Henderson* proposed that “including” be replaced with “or”: authors were not part of the names of plants. This was accepted as a friendly
amendment by the proposer. [Due to an editorial oversight, the amendment did not find its way into the St Louis Code.]

**Prop. B was accepted.**

**Prop. C (11 : 76 : 23 : 5).**

Greuter admitted that the proposal looked a bit complex; but the situation was complex, and the proposed wording represented the minimum detail needed to achieve clarity.

**Prop. C was accepted.**

**Prop. D (42 : 80 : 6 : 85).**

Reveal had proposed two special provisions to deal with ranks in specific works [see Art. 35 Prop. E]. The present one gave special consideration for works by Engler and Engler & Prantl, in which names that ever since had been considered as subtribal were either formally noted as being names of "sections", or lacked an explicit rank and appeared to be unranked. There were some other instances in the early volumes of these works in which the rank was given but the terminations generally followed by Berlin botanists (which the Rapporteurs had mentioned) were not applied. The proposal would stabilise what had been traditionally understood, but if the Section wished to refer them to the new Special Committee, it was desirable that pending its deliberations the traditional application of names ending in -inae as subtribes be maintained.

Greuter noted the strong (40 %) "sp.c." mail vote. As an apposite Special Committee would be established, it was wise to let it consider this case. A committee encompassed wider experience than any single person might have. Admittedly, especially from a Berlin perspective, Engler's were major publications, but there were others that were also important, and a general overview of the field should better precede specific solutions for a single case. Pending proposals from the Committee, Preamble 10 stated that established usage was to be followed when the consequences of the application of a rule were not clear. Nothing would therefore be lost by waiting another six years before taking a final decision.

**Prop. D was referred to the Special Committee on Suprageneric Names.**

**Prop. E (15 : 98 : 93 : 5).**

Veldkamp knew of many cases where the same combination based on the same type had been made in different places by different authors.
These later combinations were usually referred to as "pro comb. nov." or some similar expression, but its exact nomenclatural status was not defined in the Code. This was not homonymy, as homonyms are defined as having different types. The proposal tried to clarify the situation by an example, using the term "isonym", which had been introduced 30 years before by Nicolson. He was uncertain where the Editorial Committee would want to define this term in the Code and would leave it to their discretion, but he found the Rapporteurs' request for a formal introduction of the term in an apposite provision remarkable.

Greuter, in responding to the latter remark, explained that it was unusual for the Code to use jargon in examples that was not defined in a paragraph or a note. If the Section so wished, the Editorial Committee could perhaps handle this by converting the last sentence of the proposed example, "A later isonym has no nomenclatural status and is to be regarded as a bibliographic error of citation to be corrected", into a Note defining isonyms. The example would then stand on its own, and the term "isonym" would make sense.

Gandhi had come across several instances where people had made an existing combination again, and realising this later, had believed they had made an illegitimate name. The proposal would clarify the situation.

W. Anderson was not keen on the proposal. Referring to the previous introduction of the terms "homotypic" and "heterotypic" into the Code, he felt that what was here called "isonyms" were homotypic homonyms. Such homotypic homonyms were sometimes listed in indices and elsewhere as validly published names. Changing their designation was not really necessary. Defining them as homotypic homonyms would avoid introducing yet another new term into the Code.

Greuter took W. Anderson's comment to show that the Note and example were needed. If someone made a new combination, and someone else unknowingly made the same combination again later, that other person did not publish a new combination – because the combination was not new. Citing the later author as the combiner was just an error, and listing this in an index also was an error, to be corrected in the index. Obviously this should be spelled out clearly, but it involved no change of what was presently in the Code.

Veldkamp added that the misunderstanding was in the homonymy notion. Homonyms were defined to be heterotypic names, but these "isonyms" were homotypic. This was causing confusion to those citing such combinations and needed to be clarified.
Prop. E was referred to the Editorial Committee.

Article 34

Prop. A (6 : 211 : 0 : 1) was ruled as rejected.

Article 35


Greuter suggested that the proposal, which was part of the suprageneric names series, be referred to the new Special Committee on Suprageneric Names, in conformity with the mail vote.

Prop. A was, however, withdrawn.

Reveal presented a new motion instead, which had been published on the World Wide Web in May and of which copies were distributed. [It read: “For suprageneric names published on or after 1 January 1908, the use of one of the terminations specified by Art. 17-19 and Rec. 16A is accepted as an indication of the corresponding rank, unless this (a) would conflict with the explicitly designated rank of the taxon (which takes precedence) or (b) would result in a rank sequence contrary to Art. 5 (in which case Art. 33.5 applies).” Three examples were added.]

In their comments on Prop. A, the Rapporteurs had indicated that they would be happier if the proposed changes were restricted to the period prior to 1906, so as to reduce the risk of retroactively eliminating suprageneric names. Their concern was appropriate. At the Vienna Congress, modern terminations were introduced into the Code. The new provision he had developed with the aid of various correspondents would, if adopted, have the following effect. Suprageneric names proposed prior to 1908 for which the author did not indicate a rank would be considered unranked, but names proposed later without a specified rank would be considered ranked if the author used one of the terminations specified in Art. 17, 18, 19, or Rec. 16 A. A caveat would ensure that names with misplaced ranks would continue to be invalid. The new provision would remedy the failure of some modern authors to specify the ranks of new names after 1 January 1953, as required by Art. 35.1, but the exemption would apply only to suprageneric names. He had come to realise that he himself had not paid much attention to whether or not a person actually specified the rank when publishing a new suprageneric name. Nakai, in a 1943 paper using four different such ranks for a whole series of new names had specified the ranks for some but not for others – yet it was quite clear from the terminations which rank was intended.
Some authors had argued that a name ending in a standard rank-denoting termination of today, but otherwise unranked, was published at the rank denoted by its termination; e.g., that a validly published, otherwise unranked pre-1908 name ending in -aceae could be accepted as the name of a family – even though from the context it might be evident that it was meant to apply below the rank of family. The proposal’s primary aim was to dispose of that idea for pre-1908 names. This was a problem that mainly affected textbooks and floras published in the late 1700s and through to the mid-1800s, particularly in France and Germany. In those early times, the ending -eae was used for both tribe and subtribe. In Germany the term “Gruppe” was used; not as a rank but merely a category for listing names below the rank of family. These names had terminations, sometimes -eae and sometimes other ones. Should one now accept that an otherwise rankless name ending in -eae was published at the rank of tribe, perhaps even if originally placed below the rank of tribe? This proposal would make it possible for unranked names to remain unranked, regardless of their termination. [At Dorr’s request, Reveal’s motion was seconded.]

Funk requested that the decision on Reveal’s motion be postponed till after the lunch break, to permit careful study of the distributed text.

Greuter considered this a reasonable request, but discussion might continue as it was not yet time for lunch. [Laughter.] The Code did not now require the use of rank-designating terms in order to validly publish a suprageneric name. Unranked names published after 1952 were not validly published, but the Code did not prescribe the way in which rank was to be indicated. An author of a suprageneric name who followed the Code by giving that name the specific termination appropriate for its rank, could legitimately claim that the rank was thereby indicated, so the name was validly published. There was no problem until Reveal through his profound explorations of early suprageneric names had found that they often had the standard terminations of today but were either stated to be in a different rank (in which case the stated rank counted), or were in no stated rank. The latter were indeed a problem, because going back now and applying present-day criteria to those early names would result in lots of additional ranked names of tribes, families, etc. This might be destabilising for the period before suprageneric ranks had standardised terminations defined in the Code; but it would be equally destabilising to disallow the option that names published under the current Code could have their rank indicated solely through their termination.
The date from which the proposed provision should operate might be open to debate. The current standardised terminations came into the Code at the Vienna Congress and took effect immediately upon their adoption in 1905. As three years were normally allowed for the Code to be published and become known, the proposed 1908 starting date was reasonable. It could also be argued that the Engler school had triggered standard terminations of suprageneric names by their in-house rule published in 1898 [first, in fact, in Notizbl. Königl. Bot. Gart. Berlin 1: 245-250. Jun 1897], which was mandatory for Prussian civil servants. The date now proposed was reasonable, although 1905 might be equally appropriate. Neither he nor Reveal could tell whether any names would be affected for the better or worse by the choice of date.

G. Yatskievych thought he knew of one name where this proposal would cause a change, and there might be others. In the Tokyo Code there had been a change in the entry for Adiantaceae in App. IIA. The date of valid publication had been brought back to 1840, the authorship changed to Newman, and two post-1840 rejected names had been removed. The name Adiantaceae was now conserved only against Parkeriaceae. Pichi Sermolli had disagreed, as he felt Newman’s names were not of families but either unranked or tribal. As the date for Adiantaceae would again be pushed forward by the new proposal, adoption would mean that the names previously rejected against Adiantaceae but now deleted, Acrostichaceae and Sinopteridaceae, would have to be listed again. The priority of other conserved family names might also be affected.

Greuter happened to be familiar with that case, as was Reveal who, with Hoogland, had found that Newman had actually used the rank of family in 1840. Although they probably failed to convince Pichi Sermolli, they had convinced the Committee for Pteridophyta and himself that Adiantaceae, in Newman’s work, was a family name. This had nothing to do with the termination, nor with the fact that Newman happened to use the termination -aceae in 1840, so the case was irrelevant for the present proposal.

Reveal’s motion was carried first thing after the lunch break.
SIXTH SESSION

Wednesday, 28 July 1999, 14:00-18:00

Article 35 (continued)

Reveal introduced a second motion, to reword Art. 35.2 editorially – which, he said, was merely a suggestion to the Editorial Committee. [The text, which had been distributed, read: “35.2. A new name or combination published before 1 January 1953 without a clear indication of the rank of the corresponding taxon is validly published provided [delete: ‘that’] all other requirements for valid publication are fulfilled; it is, however, inoperative in questions of priority except for homonymy (see Art. 53.5). If it is a new name, it may serve as a basionym or replaced synonym of a name of a taxon in a definite rank [delete: ‘for subsequent combinations or avowed substitutes in definite ranks’].” – There is no record of the motion having been seconded.]

Greuter confirmed that the Editorial Committee would, if asked, take care of the motion and implement it in so far as it improved the wording and did not alter the meaning.

Reveal's second motion was referred to the Editorial Committee.


Greuter explained that Prop. B and Prop. C had the parallel aim of assigning ranks to presently unranked pre-1953 names published with a specified typographical sign instead of the rank-indicating term. Neither was favoured by the mail vote. The Committee for Bryophyta, which had had a thorough look at them, was negative on both (1 : 7). As the proposer was absent, and in view of the mail ballot and of the misgivings of the Committee of which the proposer was a member (which might explain the one positive vote), acceptance could not be recommended at present.

Prop. B and Prop. C were both rejected.

Prop. D (64 : 83 : 51 : 6), belonging to the deferred hybrid package, was eventually withdrawn (and referred to the Special Intercode Committee ICBN/ICNCP).

Greuter advised that, by analogy to the action taken on Art. 33 Prop. D, and unless the proposer strongly disagreed, Prop. E be referred to the Special Committee on Suprageneric Names.

Reveal agreed on the proviso that Preamble 10 did ensure that current usage of the family names addressed by the proposal would continue.

Prop. E was referred to the Special Committee on Suprageneric Names.

Article 36

Prop. A (8: 215: 2: 2), Prop. B (6: 217: 2: 1), and Prop. C. (8: 213: 2: 1) were all ruled as rejected.

Article 37


Greuter asked for comments from the Committee for Fungi and wondered to what extent the actions taken on earlier proposals of that Committee were connected with or affected by this proposal.

Hawksworth said that the mycologists present had discussed the proposal and concluded that it was to be withdrawn, as its sentiments were embodied in what had been approved the day before. Unfortunately, the proposer was not present to do so.

[Gams would later, during the Eighth Session on Thursday morning when he was again present, confirm that the proposal had been discussed by the mycologists in attendance. A revised text, being a shortened version of the published proposal, with its second sentence omitted, had been entrusted to Hawksworth.]

Voss suggested that the proposal, which was worded as a Note, be referred to the Editorial Committee, to be dealt with in agreement with what had been decided the day before [on Art. 8 Prop. C].

Prop. A was referred to the Editorial Committee.


Barrie pointed out the proposal’s main merit: to protect names published after 1957, and mostly before 1990 for which two [or more] specimens of the same gathering were cited as the “holotype”, mostly because the herbarium in which the type was conserved was not specified. Presently the validity of such names was in doubt because of the
ambiguous type statements. By softening the language in the current Art. 37.3 they would be salvaged.

Greuter further explained that the proposal hinged on the new definition of “specimen”, earlier accepted for inclusion in Art. 8, which provided the opportunity to save many names that were presently in limbo. Prior to 1990, when it became mandatory to specify the place of deposit, the holotype was often stated in terms of one gathering by a given collector, with a given number, and from a given locality. Such a gathering often consisted of two or more specimens, even though the original author might have known only one. The change in the rule from “specimen” to “gathering”, which was at the core of the proposal, would save many names that were now generally accepted as validly published.

Nic Lughadha felt that the last sentence should end with “type gathering or illustration”. Otherwise, one would be eliminating illustrations.

Compère agreed that it was necessary to mention illustrations, as during that period many algal names had been published with an illustration as type (a so-called iconotype).

Barrie pointed out that, regarding the mention of illustrations, the proposal just followed the wording of the present provision. Illustrations were covered in the first sentence [where the Code had “element”].

Greuter had meanwhile concluded that adding “illustration” at the end was not necessary and logically inappropriate. The third sentence referred to the second one, which only related to specimens. There was another difficulty, however, which was real. Art. 37.3 addressed situations in which the term “type” was not used explicitly, ruling that inclusion of a single element in the protologue was tantamount to use of the word “type”. If now “specimen” were to be replaced by “gathering”, Art. 37.3 would no longer apply in such cases, if a concrete specimen was cited and not a whole gathering. What was really needed was to add “gathering” to the proposed text, so that Art. 37.3 would read: “Citation of a single specimen, gathering, or illustration is acceptable ...”.

Barrie accepted this suggestion as a friendly amendment.

Thulin had problems with the proposal, as in reality it changed more than one had thought. The intent of Art. 37.3 was to cover cases where there was no explicit type designation, but where only a single element was cited. But the proposal would include cases where there were type statements, addressing a different issue, which worried him. He failed
Art. 37  Nomenclature in Saint Louis

to understand the Rapporteurs’ statement, in the “Synopsis”, that “when strictly applying the current rule one would have to deny the validity of many widely accepted names when it is later found that a single cited gathering consists of two or more duplicates”. The present provision had “a single element”, which was fine; the difficulty would arise with the proposed change. The Rapporteurs had turned the argument upside down. He could not foresee the consequences of the change and so was not prepared to support the proposal.

Barrie explained that two different situations existed. (1) An author had cited as type two different specimens from the same gathering, as illustrated by Ex. 1 of the proposal: Wimmer had cited duplicates in Pretoria and in the Bolus herbarium. (2) The protologue might mention a single herbarium, e.g., “Typus: Pretoria”, but upon verification there were two duplicate specimens in that herbarium. Under the present rules, the name in both cases could be considered invalid. The proposal would soften the provision and made it possible to accept such type designations. Later one might lectotypify the name by one of the duplicate specimens.

Greuter replied to Thulin that “element”, for typification purposes, was a technical term used in Art. 7-8 as a place-holder for the phrase “specimen or illustration”. Nowhere in the Code was it defined to include “gathering”. What was being proposed was to widen “element”, i.e., “specimen or illustration”, to “specimen, gathering, or illustration”, so that, when a gathering was cited rather than a single specimen, the name could nevertheless be deemed valid. Thulin’s first objection was perhaps justified: Barrie’s Committee had tried to reword Art. 37.3 so that it would hit two different flies at one stroke, permitting “type gatherings” and implicit type designations. The Editorial Committee would possibly conclude that the proposed new Art. 37.2 should better be split into two paragraphs, to cover these distinct aspects. The meaning of the proposed amendment, however, was perfectly clear, and the next Code would reflect it appropriately.

N. Taylor would vote against the proposal, being concerned about its implications as illustrated by the first example given. It would result in reversal of the validity assessment of such names by subsequent authors. A holotype could only be in one place.

Greuter replied that most often the herbarium was not specified at all, and specimens with that collector and number might or might not be present in more than one herbarium. Also, under the two-step lectotypi-
Englera 20 – 2000

fication concept introduced by Brummitt [Art. 9 Prop. L], it was possible anyway to designate a lectotype out of a previously designated type.

Farjon agreed with the wording of the proposal as it presently stood, but urged that the final mention of “illustration” [still appearing on the screen consequent to Nic Lughadha’s earlier suggestion] be removed [which it was].

N. Taylor felt that Greuter’s comparison between two-step lectotypification and the case illustrated by Ex. 1 was inappropriate. In the former case, validity of the name was not in question, but the name in Ex. 1, under the present rules, had to be considered as invalid as published because the “holotype” was stated to be in two places.

Barrie retorted that this was really the point of the proposal. Under the current Art. 37.3 authors were required to indicate a holotype, but the revised version would allow the acceptance of names as validly published even when the type designation was faulty, provided that a single gathering was indicated from which a lectotype could later be designated. This would save many names published between 1958 and 1990.

Gandhi pointed out the need to include a reference to Art. 37.5 (and also 37.4) in Art. 37.3.

Phillipson felt that such multi-specimen types could not be regarded as holotypes. A holotype was a single specimen in a particular herbarium, and when a single gathering was represented in two herbaria, it could not be a holotype. Neither could the two specimens be isotypes, as an isotype presupposed a holotype to exist. Perhaps they were syntypes? He also wondered whether it would have bearing on the situation if one sheet were found to be annotated as, e.g., “orig.” by the author.

Barrie replied that herbarium annotations did not have any bearing on what was in the protologue, which was all that mattered. The citation of two duplicate specimens was not a holotype designation, but as it was a citation of a “single gathering”, a lectotype could be designated from among the two specimens.

Phillipson pointed out that there was no term for that kind of type.

Greuter agreed. What was intended was “type for the purposes of Art. 37”. There might be no need for yet another, specific term to cover this.

Silva suggested that, as the proposal had “gathering” rather than “collection”, it should not refer to collectors name, collecting number, and date of collection, but should be rephrased to read “gatherer’s name,”
“gathering number” and “date of gathering”. The term “gathering” was strange to most practising taxonomists.

Greuter suggested that Silva, as a prospective member of the next Editorial Committee, could help resolve this when the Code was edited. The term “gathering” had been approved earlier [with Art. 8 Prop. A].

Wilson suggested that “in two herbaria” be added editorially at the end of the proposed Ex. 1, to make it clear that “Pret.” and “Bol.” were separate herbaria.

Prop. B was accepted.

Prop. C (36 : 160 : 6 : 9) was ruled as rejected.

Article 41


Greuter thought that the proposal, in the mail ballot, had received highest “sp.c.” vote of the set on suprageneric names [but see Prop. B, below]. It should go to the Special Committee on Suprageneric Names.

Reveal welcomed the Section to the strange and mysterious world of how one validated a name above the rank of family, for which there were no specific rules in the Code but only general provisions. The Rapporteurs were correct in commenting that the word “genus”, in clause (b) should be corrected to “family”, and they were also correct in stating that the procedure described in (c) for validating names above the rank of family, by citing the Latin name of the only genus included, was strange. This procedure had first been used by Cronquist in 1981, referred to in the proposed Ex. 0, and had been repeatedly applied since then. He sympathised with the matter going to the new Special Committee, and agreed to deletion of clause (c), if the Section thought it appropriate, but it would be nice to have some directions on how to validate names above the rank of family.

Greuter was confident that the Special Committee, when it set to work, would read these comments in the printed nomenclature proceedings.

Prop. A was referred to the Special Committee on Suprageneric Names.


Greuter pointed out that the mail ballot on Prop. B had resulted in an even higher “sp.c.” vote (71 %) than the one on Prop. A (68 %).

Reveal did not object to its being passed to the new Special Committee.
Prop. B was referred to the Special Committee on Suprageneric Names.

Prop. C (58: 119: 6: 15), belonging to the deferred hybrid package, was eventually withdrawn (and referred to the Special Intercode Committee ICBN/ICNCP).

Article 43

Prop. A (116: 79: 1: 6), belonging to the deferred hybrid package, was eventually withdrawn (and referred to the Special Intercode Committee ICBN/ICNCP).

Article 46


Greuter noted that proposals on author citations, together with those concerning orthography, used to be among the toughest to deal with by a Nomenclature Section. Here was a possible exception, to judge from the positive (55 % “yes”) mail vote.

Trehane had been worried by the growing trend, among editors of journals, serials and other publications, to insist that scientific names be followed by author citations. In many contexts, especially those related to conservation, environmental or economic botany, such citations were superfluous. Editors often felt that their insistence on author citations, which to many were editorial noise, was justified by the present Art. 46. The proposed rewording of that Article would encourage editors not to insist on author citations as a matter of penalty, and would advise botanical authors to restrict the use of such citations to publications of a taxonomic or nomenclatural scope.

Brummitt, while supporting the proposal, brought a detail to the attention of the Editorial Committee: the text referred only to authorship of the name itself; not to parenthetical authorship which should be dealt with in the same way.

Demoulin admitted that sometimes author citations could be dispensed with but was reluctant to weaken the present strong advice. He knew of two laboratory organisms, Aspergillus nidulans (a fungus) and Anacystis nidulans (a blue-green alga), that in biochemical papers were both often referred to as ‘A. nidulans’, and consequent confusion of the two had occurred. This could have been avoided if, in such papers, the authorship of names had been mentioned.
Greuter objected that if a paper was so carelessly edited as to use the same abbreviation in the same paragraph for two different genera, it might also use the wrong author citations. [Laughter.]

Voss heartily supported the proposal for the reasons given. The proposed wording would strengthen the position of authors who objected to an officious editor’s insistence on adding author citations in lists of names in, e.g., ecological papers when they were not needed.

Prop. A was accepted.

Prop. B (66 : 143 : 2 : 3).

Hawksworth explained that the proposal reflected the increasing practice, in mycology, of adding the date of publication to the authorship citation, in formal taxonomic papers. He had done this himself over many years, as it brought the citation closer to an abbreviated bibliographical reference. The practice was especially valuable in locating the relevant publication of a prolific author, and it was easily implemented by taxonomic authors who would have checked the sources of the names.

Zijlstra was against the proposal. Readers who did not know the rules, would believe that the authors plus years referred to the list of references, and as they would not find such publications cited, they would think that the author had prepared a very incomplete bibliography.

Demoulin, while in favour of author citations, felt adding the date was excessive. There were sometimes problems in determining the correct date, so that this requirement would complicate matters for little profit.

Prop. B was rejected.

Prop. C (72 : 134 : 4 : 3) was withdrawn.

Prop. D (120 : 85 : 8 : 1).

Wagner explained that Art. 46.6, introduced into the Code in Yokohama, intended to stabilise the establishment of authorship of plant names by disallowing the use of evidence that was not present in the publication itself. This turned out to be problematic in rare cases of historical works, especially a Catalogue of new and interesting plants collected in upper Louisiana, wherein 17 names were published of which 13 were currently accepted, some for species widespread in North America. No author was listed anywhere in that work, but the authorship was known from external evidence. The proposed additional phrase would take care
of the problem without affecting the intent of Art. 46.6. A search in *Index kewensis* for further potential names of unknown authorship yielded 19 cases, mostly of obscure names, some published on correction slips – a very different situation from the one addressed here.

**W. Anderson** knew of a fair number of similar cases, where authorship was currently attributed on the basis of external evidence and had never been questioned. If it was in question now, there was indeed a problem that the Section should address.

**Greuter** elaborated on the Rapporteurs’ remarks in the “Synopsis”, which might not have been generally understood. (The mail vote in fact did not follow them, disproving the claim that the Rapporteurs’ opinions unduly influenced it.) Art. 46, particularly 46.6, dealt with the authorship of names. The new proposed sentence, beginning “In cases where there is no internal evidence of authorship ...”, meant the authorship of a work – a book or article. When placed in the context of Art. 46.6, however, it would from the context apply to names and not to works. The case of Bentham & Hooker’s *Genera* had been extensively discussed in Yokohama. The conclusion had been that no external evidence could be sought to attribute individual names to either Bentham or Hooker, so under the current Code unascribed names in that work were to be attributed jointly to Bentham & Hooker. The present proposal would, unintentionally, reverse that conclusion. Being in full sympathy with the proposers’ intent, he suggested that their new sentence be taken out of Art. 46.6 and placed after it as a Note, to read “In cases where they is no internal evidence of authorship of a publication ...”. The suggested example would remain appropriate.

**Burdet** wondered if Greuter’s suggestion could be treated as a friendly amendment.

**G. Yatskievych** tentatively agreed, subject to some clarification. The real concern was the authorship of names, and he agreed with the comment that authorship of a publication was a bibliographic matter and nothing to be dealt with in the *Code*. One present example in the *Code* was of Aiton’s *Hortus kewensis*, in which historical scholarship had determined the authorship of individual entries by Solander and Dryander (the details were in Barnhart’s *Dictionary*). Art. 46.6, for good reason, specifically disallowed the use of that information for establishing the authorship of names. The problems arose when, for example, authorship of a work produced in a country under communist rule had been
suppressed and the publication was credited to some anonymous committee or group. The proposal was to allow outside evidence also in these cases of works effectively anonymous as to individual persons, concerning authorship of the included names. The bibliographic citation, however, was not at issue, it might still mention the committee, nursery, or a gardening company, as the case might be. What mattered was to fix the authorship of as many names as possible. There was also the potential danger of someone interpreting the Code in a destabilising way, by claiming that in the absence of a definable author there was no evidence that the author did accept the name [see Art. 34.1(a)].

Greuter thought that G. Yatskievych’s concern, if concern there was, would be covered by adding five words at the end, then to read: “... external evidence may be used to determine authorship of the included new names”. This would spell out that authorship of the names was meant, not of the work, which could remain anonymous for bibliographic purposes. [The proposers agreed.]

Prop. D, so amended, was accepted.

[The following motion, relevant to Art. 46, was dealt with during the Ninth Session on Friday morning.]

Gandhi moved a new proposal [and his motion was seconded], to add a Note and an Example to Art. 46.2, after Ex. 4: “Editorial changes made by the publishing author do not affect the citation of authorship. Example: Cymopterus montanus Nutt. ex Torr. & A. Gray, not ‘Nutt. in Torr. & A. Gray’: Nuttall’s manuscript name was ‘Phellopterus montanus.”

The Note was intended as a clarification. In Art. 46, there were extensive examples of authorship citations. Art. 46 Ex. 4, displayed on the screen, was on a species of Nuttall published in Torrey & Gray, who had used Nuttall’s name and description, so that the authorship clearly was Nuttall’s. Having studied Flora of North America over a period of about 15 years, he had however found numerous cases of unclear authorship. One such example, also displayed on the screen, was of the name “Phellopterus montanus”, submitted to Torrey & Gray by Nuttall in manuscript form, with a full description and other data. Torrey & Gray changed the generic name to Cymopterus, but used everything else: epithet, description, distribution data, all came from Nuttall, and Torrey and Gray ascribed the name to Nuttall. Should the authorship in such a situation be cited as Torrey & Gray or Nuttall? As usual, he had asked several specialists. According to at least two of them, the change...
it generic name was merely editorial, and the authorship was Nuttall in Torrey & Gray, but others had disagreed.

**Brummitt** recalled that when the new rules on "in" and "ex" citations were brought into the Code six years before, there was a precise example mentioned, specifying that in such a case, when Torrey & Gray had changed the genus, Nuttall was not to be cited at all. As Nuttall had never used *Cymopterus montanus*, the name should be cited as *C. montanus* Torrey & A. Gray. The case was not explicitly mentioned in the Code but was covered by Art. 46.3 and Ex. 9. The proposed example, as worded, was contrary to what had been agreed in Yokohama.

**Barrie** agreed with Brummitt, but was uncertain of the relationship of "Phellopterus" to *Cymopterus*. Was the former a genus that Torrey & Gray had sunk into *Cymopterus*, or was it a name that, for some reason, they could not use? Had "Phellopterus" ever been validly published?

**Gandhi** explained that it was a manuscript name of Nuttall's, not validly published, which Torrey and Gray did not use.

**Wiersema** disagreed with Brummitt. Art. 49 Ex. 9 did not apply to Gandhi's case. *Lichen debilis* Smith had not been ascribed to Turner and Borrer who were credited with authorship of a listed synonym. In the present case, *Cymopterus montanus*, the name accepted by Torrey & Gray, had been ascribed to Nuttall. If it were necessary to use information not included in the protologue to ascribe authorship, one would be in trouble. Authorship should be accepted as ascribed.

**Greuter** agreed. Gandhi's example was essential in so far as without it, no one could ever guess what the Note was intended to mean: on its own it did not make sense, so its wording was anyway inappropriate. The example was relevant but its conclusion was wrong. Under the present Code, with the principle – enshrined in Art. 46.6 – that only internal evidence was to be used in determining authorship, the author was Nuttall, to whom both the name and the description were ascribed, even though he had supplied the description under a different manuscript name. One could never be sure that the name change was "editorial", i.e., due to Torrey & Gray, unless there was fully preserved correspondence between Nuttall and Torrey & Gray on the question. It was quite possible that Nuttall had at some stage endorsed the change in generic denomination, and had agreed to the name ascribed to him by Torrey & Gray. The example, with its conclusion altered, could be offered to the Editorial Committee, but the Note was unhelpful.
Marhold found the example useless, as Nuttall had contributed the epithet but what mattered was the whole name.

Demoulin considered the present Art. 46.3 very unsatisfactory, as it led to situations such as of Lichen debilis, quite contrary to common sense. But this could not be helped now. If some were dissatisfied with the current provision and, having tried to apply it, could prove that it did not work and had to be changed again, they would have to make proposals to another Congress. Meanwhile, using the system specified in Art. 46.5 for names in groups with a later nomenclatural starting date might be envisaged. This was only a suggestion, but at present nothing else could be done.

Zijlstra asked whether Torrey & Gray, who clearly had mentioned Phellopterus montanus as Nuttall’s manuscript name in synonymy, had also ascribed Cymopterus montanus to Nuttall. [Gandhi, and others, replied affirmatively.] She agreed that, if so, “in” was appropriate.

Hawksworth pointed out that the case of Lichen debilis was covered exactly by Art. 46.3. Smith did not use quotation marks in his work, but was actually copying from an unpublished text produced about 1809, effectively published much later.

Brummitt hated discussing examples without seeing the printed text. One was told Torrey and Gray did such and such, but upon checking one might find something different. In his experience, Torrey & Gray quoted exactly what they had used from Nuttall, so there was internal evidence. They must have mentioned Phellopterus montanus and attributed it to Nuttall, but had themselves used Cymopterus montanus. The Code in such a situation was not perhaps quite clear, but if one dug into the ramifications of its Articles, one had to conclude that the citation must be Cymopterus montanus Torrey & A. Gray, without mention of Nuttall. Indeed it would be confusing to cite Nuttall, who had never used the name Cymopterus montanus. This was what had been agreed six years before. Going back on it would cause change in many cases. The same situation arose every time when a publishing author attributed an epithet to an earlier author, but under a different genus. He strongly opposed the proposal.

Greuter felt that the Section was being involved with technicalities. He agreed that if matters were as Brummitt thought, the authorship was Torrey & A. Gray, but he also agreed with Zijlstra and Gandhi that if Torrey & Gray had ascribed the adopted name to Nuttall, Nuttall was
the author. The Editorial Committee would take note of the example and check its appropriateness carefully before it was introduced. It would also look once more at the *Lichen debilis* example, to ascertain that it was indeed appropriate. The two examples were not however identical, because for *L. debilis* the ascription was in synonymy only. On this understanding, the motion itself should better be defeated.

Gandhi did not mind leaving the decision to the Editorial Committee, but wished to see the situation clarified by having the example spelled out in the Code. Torrey & Gray had ascribed the adopted name to Nuttall while mentioning that Nuttall had submitted it under *Phellopterus montanus*.

Gandhi's motion was withdrawn.

*Here the record reverts to the normal sequence of events.*

**Recommendation 46A**

**Prop. A (111 : 84 : 15 : 4).**

Greuter explained that the present Rec. 46A provided a standard recipe of how authors’ names were to be abbreviated, which did not in every case reflect sound current practice. For instance, Hornschuch would have to be abbreviated “Hornsc.” not Hornsch., which for those familiar with German was an anathema. The proposal offered a slightly different recipe that, he thought, reflected current practice – unless someone knew of cases in which it would not work properly.

Ochsmann suggested adding a Note mentioning Brummitt & Powell’s 1992 list of abbreviations. That list had been adopted by the Taxonomic Databases Working Group as an official standard. In the interests of databasing, botanists should use one standard abbreviation system not their own individual abbreviations.

Hawksworth pointed out that Rec. 46A Note 1 already mentioned Brummitt & Powell’s list.

Prop. A was accepted.

**Recommendation 46E (new)**

**Prop. A (27 : 122 : 59 : 6).**

Hawksworth saw the proposed Recommendation, which authors could draw to the attention of editors of non-taxonomic works, as a logical consequence of Art. 46 Prop. A, just approved. Apart from the editorial
deletion suggested in the Rapporteurs comments’, the words “and date” would also be deleted, due to the rejection of Art. 46 Prop. B.

Farjon did not seriously object to the proposal, but queried whether any authors or editors of non-taxonomic publications would bother to look into the Code.

Hawksworth agreed that the point was well taken; still, having something to photocopy and pass on to an editor or author could be useful.

Ziobro stated that the Code was referred to, especially in the U.S.A., for the naming of herbal products and herbal supplements. The recommendation could have practical implications in that country.

Demoulin, having opposed Art. 46 Prop. B, could obviously not approve this one, which went even further. Why should, e.g., floristic and phytosociological works not use author citations, when they had traditionally used them? One might not wish to urge their use in all kinds of botanical publications, but to recommend against them in kinds of literature that always had used them was not reasonable.

Gereau had been surprised to hear that editors of publications on herbal products and herbal supplements referred to a botanical Code, but if so: would anyone care to see less precision in the use of botanical names in these otherwise unregulated medical products?

W. Anderson drew attention to the Rapporteurs’ comments on the expression “critically assessed”. Earlier today the Section had given Stuessy a hard time for using undefined vague terms. The same criticism applied here: who was to decide what was critically assessed?

Hawksworth explained that the proposal now before the Section took care of the Rapporteurs’ comments by omitting that phrase. It now read: “Authors and editors of botanical publications are urged to restrict the use of author citations to formal taxonomic and nomenclatural works”. Author citations in non-taxonomic journals generally did not follow the Brummitt & Powell standard, and errors were often considerable, so that they did not add precision but rather confusion.

McVaugh was not entirely clear why the Code should try to dictate editorial policy for non-taxonomic and non-nomenclatural books. This was like telling the editors of the Bible not to use botanical names.

G. Yatskievych had not yet made up his mind and asked for clarification on two points: (1) was a Flora a formal taxonomic and nomenclatural work? and, if so, (2) was something less technical like an excursion...
Flora still such a work? Almost every Flora contained author citations, sometimes erroneous ones that had been put in rather arbitrarily. Would the proposed Recommendation, depending on the answer, muddy the waters further or rather clarify things?

Hawksworth replied that in his personal view a Flora was a taxonomic publication, and people working on a Flora should assess author citations very carefully.

Demoulin thought that whether or not a Flora was a taxonomic work was contentious. However, a checklist was definitely not a taxonomic work, as taxonomy implied classification. Floristics were not taxonomy. Besides, Prop. A had been heavily defeated in the mail ballot.

Greuter guessed that what kept the Section looking at the proposal was that the amended version was less unacceptable than the original one. Addition of an explanatory clause at the end, “such as Floras, checklists, and monographs” might help. Replacing the strong verb “urged” by “advised” would also make the proposal more palatable. The basic intent was to provide a sound argument to authors or, conversely, to editors: when one or the other wanted at all cost to put in author citations where they did not belong, he or she could then be told that this was not recommended by the Code. However, as a Rapporteur he had always been reluctant to introduce new Recommendations into the Code.

Veldkamp, as a non native English speaker, was puzzled by the construction of the sentence. If he understood it correctly, the intent was to say: “... are urged to use author citations in formal taxonomic and nomenclatural works only”. [Hawksworth agreed.]

Toolin had had the same misconception and misunderstanding as Veldkamp. To make the meaning clear, the text had to be reworded. If the idea was that author citations were only to be used in formal taxonomic and nomenclatural works and not elsewhere, that should be stated.

Voss supported the Rapporteur’s suggestion to use a less urgent verb such as “advised”, and also wished to eliminate “restrict”, writing instead: “to avoid ... except when necessary for nomenclatural or taxonomic clarity” and omitting reference to the nature of the work.

Marhold disliked the proposal, but should it be approved, he urged that the word “formal” be deleted. What was a “formal taxonomic work”? Or, conversely, an “informal taxonomic work”? It meant nothing to him.
Jansonius suggested the following wording for the proposed Recommendation, as a possible friendly amendment: “The use of author citations should be pertinent.” [Laughter.]

Landrum observed that the proposal urged only authors and editors of botanical publications. Were not other authors also being urged?

Greuter noted that Voss’s and Landrum’s suggestions had been accepted by the author of the proposal as friendly amendments, as had that of Jansonius. [The revised text, as displayed on the screen, read: “Authors and editors are advised to avoid the use of author citations for botanical names except in taxonomic and nomenclatural works, such as floras, checklists and monographs.”]

W. Anderson was reminded, by this discussion, of a carpenter who kept trying to even the table legs by cutting off a little more, so that in the end there was not much left. So little was now left that the proposal should be voted down; it was not worth the time spent on it. [Applause.]

Farjon also urged that the proposal be voted down. True, it was merely a Recommendation, but there were numerous popular books about cacti, roses, lilies, orchids, etc., that used botanical names with author citations, and if they took that advice seriously (which he doubted they would), many of their names would become more ambiguous as to their real meaning than they were now, with the author citations appended.

Wiersema shared Farjon’s concern. In addition, there were many legal documents in which authorship of names was specified and where that level of precision was necessary.

Prop. A was rejected.

**Article 49**


Greuter – glad that the Section had got through Art. 46 and its Recommendations so quickly – suggested that this proposal, which pertained to suprageneric names, be referred to the new Special Committee, in agreement with the mail vote.

Reveal urged immediate consideration of the proposal. Its subject had been discussed extensively in *Taxon* over a number of years. The proposed way of dealing with parenthetical author citations for suprageneric names corresponded to the “Nicolson model”, which had been widely followed. Parenthetical authors were used only when the name
had been established or validated by citation of a “basionym” – using that term broadly – as in the proposed Ex. 8 and 9. In Ex. 10, Candolle had cited a basionym but also provided a Latin description; so merely Candolle would be cited but no parenthetical author, as had traditionally been done. Presently, both ways of citing names above the rank of genus were found in the *Code*. It would be useful to establish a single, ascertainable policy of citing parenthetical authorship of such names.

**Greuter** concurred that a single policy would be desirable, but the one that was being proposed was not necessarily the best and certainly not the only possible one to be followed. The question should better be examined by a whole group of people. He had misgivings with a rule under which an author who had qualified, e.g., a new subfamily as a “stat. nov.”, based on a tribal name, had to be cited alone, just because he happened to have provided a Latin diagnosis. If the proposal was referred to it, the new Special Committee might perhaps decide, in despair, to dispense with all parenthetical author citations at supragen-eric ranks. This would please him greatly.

**Brummitt** (and, he said, lots of people all over the place) wanted a decision. This matter had been argued at successive Congresses. The Section could not keep referring it to Special Committees. He supported the proposal.

**Voss** had no complaints about the proposal itself, but he objected to the proposed Ex. 10 for exactly the reason the Rapporteur had given. He saw no reason to omit the parenthetical author simply because it was the same author that would appear after the parentheses. In both cases the basionym author should be cited, too.

**Reveal** had no preference for which way the Section would go; all he wanted was an answer.

**Greuter** observed that the proposed example could not be changed as long as it was mandated by the rule. The proposal would be either adopted or, if rejected, referred to the Special Committee.

**Prop. A** was rejected on a card vote (368 : 259; 59.12 % in favour) and thereby referred to the Special Committee on Suprageneric Names.

**Recommendation 50E**

**Prop. A** (208 : 3 : 4 : 0) was accepted.
Nomenclature in Saint Louis


Hawksworth was concerned that the proposal might be seen to effectively change what was currently a Recommendation into a formal rule. In fact it provided welcome additional guidance on a very special kind of mycological author citation, appropriate in formal works. Sanctioning was still being confused with valid publication, even in major works that had recently been published by the two pre-eminent botanic gardens in the British Isles. Mycologists should be encouraged to use such formal author citations when they had checked and understood them, but any upgrading of the recommendation was inappropriate.

Greuter drew attention to the Rapporteurs' comments. Deletion of the words "if it is desirable" from the Recommendation as presently worded might perhaps be controversial. The added second sentence indeed appeared to be an improvement.

Demoulin noted that Prop. B had been heavily supported by the Committee for Fungi and by the mail vote. It would reword the present Recommendation. There was no question of turning it into a rule.

Prop. B was accepted.

Article 52


Greuter pointed out that the proposal would remedy an apparently unintentional omission in Art. 52. Inclusion, in a newly named taxon, of the previously conserved type of a name that should have been adopted should obviously lead to illegitimacy. However, at present this was not spelled out in the rules.

Prop. A was accepted.

Prop. B (13 : 138 : 55 : 3) was withdrawn.

Article 53


Greuter explained that Art. 53.5, the paragraph that dealt with homonymy at the ranks of subdivisions of genera, did not provide that confusingly similar names were to be treated as homonyms. This was awkward, as such a provision existed for generic, specific and infraspecific names, and there was no logic in exempting names in the ranks between
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genus and species. Almost certainly this was an accidental omission. The proposal would bring the homonymy rule for names of subdivisions of genera into line with those for names at other ranks.

Perry suggested an addition. As it was sometimes difficult to decide whether two names were sufficiently alike to cause a problem, there should be the possibility of obtaining a binding decision from the appropriate Committee, as specified in Art. 53.4 for names at other ranks.

Greuter promised that this point would be taken care of editorially.

Prop. A was accepted.

Prop. B (41 : 159 : 6 : 1) and Prop. C (38 : 163 : 5 : 1) were ruled as rejected.

[The following motion, relevant to Art. 53, was dealt with during the Ninth Session on Friday morning.]

Gandhi moved a new proposal [and his motion was seconded], to add a Note to Art. 53.6, after Ex. 17: “The renamed homonym remains legitimate and has priority over the nomen novum at the same rank if a transfer to another genus is made.”

As stated in Art. 53 Ex. 17, Linnaeus used the name Mimosa cinerea for two different species in 1753. In 1759, he changed one to M. cineraria. Under to Art. 53.6, the retained name had priority over the replaced one. His question was: what was the status of the renamed homonym after 1759? He had discussed the problem with five specialists in nomenclature. All agreed that both names were legitimate in 1753. According to one, it remained legitimate in 1759; a second thought that it became illegitimate in 1759; the other three were uncertain. The proposed Note would make it clear that the renamed homonym remained legitimate and took priority over the corresponding nomen novum of the same rank if a transfer to another genus was later made.

Greuter had probably been among Gandhi’s uncertain experts, as the question was complex. The later of two homonyms was illegitimate. Of two homonyms that were published on the same date, neither was later, so at least for the time being neither was illegitimate. If an author, applying Art. 53.6, renamed one of taxa, the other’s name was treated as having priority. Had the Section approved to make the distinction between precedence and priority [Gen. Prop. C], there would not now be a problem: the second name would take precedence but priority would remain unchanged, i.e., the date would remain the same for both. As the
Code read now, it could be interpreted to mean that, by an author’s act of maintaining one name and changing the other, the date had in a way been shifted and that, by that act, the replaced name had become a later homonym. This was a point upon which the Code was, and would continue to be, unclear as long as it used the wrong term “priority” rather than “precedence”. Technically the proposed text was not just a Note, because what it made explicit was not yet in the Code. The proposal was one of two possible, logically equivalent solutions. The other was to declare the renamed name illegitimate. It was uncertain which answer was more stabilising. He did not know whether the second Mimosa cinerea had been transferred to another genus and, if so, whether the epithet cinerea or cineraria had been retained. The case was probably rare, because there were few simultaneously published homonyms, and even for those, the question of whether or not to use the basionym would arise only if there was a generic shift. But it was, in principle, a valid case.

Gandhi knew of a concrete example [of which he cited the details later on; see Trock & Barkley in Sida 18: 385-387, 635-636. 1998-1999]. The case had come to his attention when reviewing an article for publication. He again explained the two alternative solutions to the case: (1) his proposal, which was in line with the tenet “once legitimate, always legitimate” [the converse of Art. 6.4]; and (2) the view that the subsequent choice, by establishing the relative priority of the names, made the “later” homonym illegitimate.

Greuter went on to explain that another controversial issue was relevant. The Code said that rules, unless otherwise stated, were retroactive, but nowhere did it declare that nomenclatural acts had retroactive effect. On the contrary, in concrete cases, such as lectotype designations, it specified the reverse. Art. 53.6 referred to a nomenclatural act. Therefore, there was logic in following Gandhi’s proposed solution. Since concrete cases must be exceedingly few, stability of names was under either possible solution was not so important.Acceptance of the motion could be recommended, on the understanding that the Editorial Committee would look at its appropriate status.

Gams failed to understand why the proposal was necessary, as the problem was one of simple priority. If a homonym was “activated” by transfer into another genus, its priority counted only from the transfer date. A later author could choose either epithet when coining a new combination, as the replacement name was older than the original homonym, was it not?
Greuter responded by explaining the situation once more. There were two homonymous, simultaneously published species names, so there was not and earlier and a later homonym. One species was then renamed with a nomen novum with its own priority. The renamed species was later transferred to another genus. The question was: had one to use the epithet of the simultaneous homonym or that of the nomen novum that had replaced it? If the homonym by being renamed became illegitimate, then one must use the epithet of the nomen novum. If the renamed homonym remained legitimate, then its epithet must be used. The Code gave no clear answer.

Knox wondered whether this might create a problem. If the renamed homonym remained legitimate and its epithet had to be used upon transfer of the species to a different genus, and if the retained homonym were subsequently transferred to the same genus, would this not require a change of epithet of the retained homonym?

Greuter confirmed this, but it was unlikely that the retained homonym should be transferred later than the renamed one, and anyway, the number of cases falling under that rule would be exceedingly low.

Wiersema agreed that the number of cases would be very low, but expressed preference for the alternative solution, to declare the renamed homonym illegitimate and transfer the epithet of the nomen novum. Both epithets would then be preserved, even if the two species were later transferred to a same genus. Under the motion, both might be lost.

Greuter asked Gandhi if, in the example he had in mind, stability would be better served by declaring the replaced homonym legitimate or illegitimate.

Gandhi preferred his proposed solution, but would not mind the contrary one. In his example, the authors had eventually considered the renamed homonym illegitimate and used the epithet of the nomen novum for transfer. [In actual fact, as that epithet was no longer available for transfer, they had created a third one.]

Greuter was hesitant. In the only case known, stability would be better served by Wiersema’s preferred solution. So the score was 1:0, but with many abstentions. The Section should first vote on the principle of the change, as a clarification was needed at any rate, then proceed to choose between the two options by a vote requiring a 50% majority – just in case there should be a card vote. [Laughter.] Gandhi’s option...
was on the board; the reverse one would read: “The renamed homonym becomes illegitimate and has no priority over the nomen novum, ...”.

Compère agreed with the necessity for change, but preferred the second option. If an author were careless enough to publish the same name twice for two different species, in the same work, even if it was Linnaeus, he did not deserve to have both names considered as legitimate. [Applause.]

Greuter felt that one should not be too hard on poor old Linnaeus; he had no computer at his disposal. [Laughter.]

The principle of a change was accepted, and discussion turned to the competing options.

Nic Lughadha agreed that the situation was very infrequent (she was aware of cases in the Myrtaceae, but did not have the details at hand). One should not abandon the principle that a name was either legitimate or illegitimate when published to deal with an exceptional situation. She favoured Gandhi’s original proposal. With it, one could look at a name at publication and decide whether it was legitimate or illegitimate, irrespective of later acts.

Briggs knew of a similar case, in Restoniaceae, where Robert Brown was the careless author, and thought the second option would produce greater stability.

Zijlstra suggested a change of wording for the second option: “The renamed homonym should be treated as illegitimate”. This would overcome Nic Lughadha’s objection.

Greuter objected that the verb “should” was inappropriate in a rule. It would turn the provision into a recommendation.

Zijlstra corrected herself to “must be treated”. She preferred the second option. If a species known by a nomen novum was transferred to another genus, it was be easiest to continue using the same epithet.

Greuter promised that the Editorial Committee would consider the suggested change of wording.

Gilbert’s concern with the first option was: when there was a new combination, how would one know on which of the two original homonyms it was based? One could not tell from the author citation; supplementary information was needed. With the second option, one of the new combinations would always be based on the nomen novum, which in turn traced back to the illegitimate name.
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Greuter agreed that this was so for future transfers, but most cases were, presumably, transfers of the past. Past authors could not have known the decisions of the present Section meeting, they might have taken up the epithet of the to-be illegitimate name. For the past, Gilbert’s objection applied to both options.

Gandhi added that the author of the transfer would typically cite the page number for the basionym, which would eliminate ambiguity.

Pipoly knew of a case in Myrsinaceae where the same epithets were used for two different species in the same genus, one by Candolle, the other by Martius. They were published in the same month and as far as one could determine, for practical purposes, simultaneously. So the same problem could exist also with different authors. He favoured Gandhi’s approach.

Greuter concluded that each option had its merits and shortcomings. For the sake of stability and maintenance of epithets upon transfer, the second approach was presumably preferable. He agreed with Nic Lughadha and Gandhi that the Code’s own intrinsic logic, which one should not undermine without cogent reasons, spoke for the first approach. He would refrain from giving advice, the Section should choose, predictably by a card vote. The first or “yes” vote would be in favour of Gandhi’s option, the second (“no”), in favour of the alternative. Upon a question by Henderson, he added that the situation covered by Art. 58 Ex. 2 was different, as it referred to a later not a simultaneous homonym.

Gandhi’s motion was carried on a card vote, in preference to the alternative option (359 : 354; 50.4 % for Gandhi).

[Here the record reverts to the normal sequence of events.]

Article 54

Prop. A (44 : 169 : 1 : 5) was ruled as rejected.

Recommendation 54A (new)


Hawksworth was gratified by the strong mail vote support for the proposal, which would find sympathy with those who wished to reduce the possibility of botanical generic names being confused with those of other groups of organisms.

Prop. A was accepted.
Article 55


Greuter pointed out that both proposals had been made redundant by the earlier adoption of Art. 22 Prop. A and Art. 27 Prop. A, covering the same ground. They could be referred to the Editorial Committee, which would be free to make use of any of their wording, if appropriate.

Prop. A and Prop. B were both referred to the Editorial Committee.

Article 56


Greuter referred to the proposal’s Yokohama history. A proposal with exactly the same intent had been defeated six years before, having the drawback of being so worded that only a small number of definite binomials would have been covered by the rule. As they were all names of European apomicts or near-apomicts, this had been seen as a parochial approach to the problem, but the idea of a similar rule presenting an open option, as was now being proposed, was encouraged. The proposer was a specialist of Rubus in the first place and also knew other “rosaceous nightmares”. There was nothing wrong with the proposal, which numerous specialists working in such groups would certainly welcome.

Brummitt indicated his support of the proposal.

Demoulin felt that the proposal would be welcomed not only by the specialists but above all by the non-specialists. It was mainly those who were not Rubus or Taraxacum specialists who would keep speaking of Rubus fruticosus, Taraxacum officinale, etc., in an aggregate sense.

Voss added his own support and agreed with Demoulin’s point. The rule was not limited to apomicts and would enable non-specialists to continue to use aggregate names like Polygonum aviculare in works that were not designed to split hairs.

Reveal was concerned about the expression “for a long time” as he did not know how to define a long time, and suggested their deletion.

Greuter was all for precision, but since this would involve a Committee decision, it was appropriate to give the Committees guidance as to the kind of cases in which the rule was to apply, yet leave them some latitude in judgement.
McVaugh was worried about certain species names that had long been in use for aggregates of numerous species. How many species did make an aggregate? Was the proposed rule going to prevent the use of the oldest included binomial to designate the aggregate? Or would such a name then become rejected for the species? The proposal seemed to prohibit simultaneous use of a name for the aggregate and for one of the included species.

Brummitt felt that the competent Committee, which would decide on each case, deserved to be trusted. Those objecting to a specific proposal would be free to submit their views to the Committee.

Gereau could find no definition or mention of either the aggregate or segregate species level in the Code, yet a rule was being proposed to deal with taxa at yet another, undefined level of rejection. He felt a high level of personal rejection towards the proposal.

Pedley faced the same dilemma as McVaugh. There had been a recent trend to split some groups of so-called Acacia in Australia. The A. holosericea aggregate included perhaps five taxa. How would A. holosericea in the narrow sense have to be named if the binomial was used to designate the aggregate? Who was to decide? Should he submit the case to a Committee when another botanist might feel that A. holosericea was not an aggregate at all? This was a dangerous provision.

Vincent felt that the provision would be dangerous by doing away with the principle of priority. He had split a number of former species into smaller ones, retaining the original name for the species that included the type, and saw no reason to disallow use of the name in the narrow sense.

Greuter pointed out an apparent misunderstanding. As before, botanists would be entitled to do just what Vincent had described. They would not normally propose any name to be included in App. IV, nor would anyone else. The proposal was for an enabling clause, permitting such action when it was needed. Rubus fruticosus was one of the oldest binomials in its genus, and people who did not want to go into detail would lump what others considered as a few hundred species under that designation. The name R. fruticosus could probably be typified and applied to one of the microspecies, but no one in his or her wits would want to use the name at that level. Taraxacum officinale was a similar case. The present procedure was to propose these names for rejection, so they would be listed in App. IV. But then, a special rule was needed to permit retention of the rejected name in the aggregate sense, which
under the current Code was forbidden. Botanists dealing with such groups felt a strong need for the proposed rule.

McNeill echoed Greuter’s remarks. The procedures of taxonomic review, such as splitting a species recognised in the past into a number of separate ones, were adequately covered by the Code and were not in question. The proposal was designed for extreme situations in which the only meaning in which a particular name had historically been applied was for a complex of nearly always agamic species – sometimes not just hundreds but thousands of microspecies. Polygonum aviculare was probably not a case in which the proposed rule could be invoked, but the examples given by Greuter certainly were, and a number of similar cases existed.

G. Yatskievych sensed trouble with the proposed rule. A few years ago Weber had published a paper in Taxon on the typification of some names of Taraxacum complexes, pointing out that the type of T. officinale did not belong to the taxon people had thought it did. He had written to Weber to enquire about the name to be used for the North American introductions, now the type had been reassigned. As he understood the proposal, if T. officinale could only be used in the aggregate sense and was rejected at the segregate species level, then those in North America who enjoyed a small subset of the aggregate over a very large range had no name by which to designate their particular taxon.

McNeill explained that just the reverse was the case. Without the proposal no overall name would be left for the introductions once Taraxacum officinale had been rejected. Prior to rejection, the name could only be used for a taxon including its type, but that microspecies might not even be present in North America. If the proposed rule was applied, the name would remain available for those who wished to recognise the species at the aggregate level.

G. Yatskievych thought that for some of the complexes under discussion as the worst offenders, botanists had already laboriously selected types and identified them to the microspecies entity, and in that case, the names could not be rejected.

McNeill retorted that of course the Committee could reject typified names. Once a name had become widely and correctly used for a particular microspecies, the provisions of the proposed article, “names ... used only in this broad sense and not for segregate species”, would no longer apply and the Committee would judge accordingly.
Keil saw no real need for such a provision. The current nomenclatural repertoire of terminology enabled to handle the situation. The expressions “sensu lato”, “sensu latissimo”, “sensu stricto”, etc., could be used to describe the broad or restricted application of names.

Farjon suggested insertion of “(e.g., apomicts)” into the proposed text, to clarify what kinds of taxa were meant.

Greuter disliked introducing reproductive biology into the Code.

Brummitt urged the Section to get its perspective right on this. Six years earlier, after a discussion of an hour and a quarter, a similar proposal had been defeated because it was considered too specific. It listed four apomictic groups which had been carefully selected with his support by the late D. H. Kent, and it was not intended to extend the list beyond those four groups: *Taraxacum officinale*, *Rubus fruticosus*, *Euphrasia officinalis*, and *Alchemilla vulgaris*. The matter had been brought to a head when someone had typified the name *E. officinalis*, which had been widely used for a group of apomictic species, and published subspecific combinations under it for the former microspecies; this had thrown the eyebrights crowd into apoplexy. There had never been any suggestion of using the proposed provision widely for other situations. Now the proposal seemed to be in danger of being defeated because it was too broadly worded. In this way there would never be progress; yet there were real problems in a few groups that needed to be addressed, which this proposal did.

W. Anderson found it extraordinary to receive the same advice from Greuter, McNeill, Brummitt, and Voss. [Laughter.] They had convinced him that the solution proposed was needed and safe. He would vote for it.

Henrickson suggested that examples be provided, including apomictic groups, to guide the process. This would provide the needed clarification.

Bhattacharyya considered the proposal to be dangerous. Herbarium taxonomists often used the aggregate species concept loosely, but without proper cytological study and breeding experiments. There were many herbaceous aquatic and marsh plants, such as *Lemma*, *Utricularia*, and *Wolffia*, that were not known properly and where some botanists might want to use aggregate species without proper study, when the segregates were in fact good species. This could be misleading for field workers who knew the life histories of their plants.
Demoulin felt that W. Anderson’s comment did not leave much to add, but had two technical points to make. In response to Keil: if *Taraxacum officinale* were allowed to be used in the restricted sense, this would make necessary constant specification of the meaning, “sensu lato” or “sensu stricto” – a very peculiar nomenclatural situation. As to Henrickson’s suggestion, no concrete examples of names falling under the proposed rule yet existed, but one might devise an example with likely candidates such as *Taraxacum officinale* and *Rubus fruticosus*.

Wiersema supported the proposal of which he recognised the need. On a point that was perhaps editorial, he requested that there be some sort of recommended abbreviation to be used after such names, to indicate their special status: “nom. rej.” would not do, as the names were only rejected in one sense and one would need to know that they were still being used in the aggregate sense.

Greuter accepted the desirability of such a standard of citation and predicted that the point would be raised within the Committee for Spermatophyta when the first such cases would be debated. The proper solution could hardly be found by the present Section. It should better result from a dialogue between the proposers and the Committee.

Phillipson pointed out that plant taxonomists had long recognised the difficulty of applying one set of rules when facing many different biological situations. Hybrid taxa were a parallel case, and there was a special set of rules to deal with their names. Something similar was needed here. Using conservation and rejection seemed a funny way to deal with the situation at hand.

Moore had understood the proposal to mean that names were to be conserved in an aggregate sense, i.e., effectively, their application would not be determined based on their type, which would clash with the “one name, one type” principle.

Zijlstra favoured the proposal. Vegetation scientists wanted to get rid of names whose meaning was unclear without the specifications “sensu lato” or “sensu stricto”, but she had started wondering whether such names really belonged in App. IV. They were not in fact “nomina utique rejicienda”. Would it be possible to treat them as conserved and rejected couples under App. III?

Greuter snapped that those who favoured a proposal should better not talk it to death. The placement of names falling under the new provision...
was a practical problem, and a solvable one, but there was no need to worry about it at this stage because no case yet existed. The Editorial Committee might bother in six years’ time whether such entries should be placed in App. IV, as proposed, and if so, how the header of App. IV might be adapted. They would probably end up under a separate subheading in App. IV. There was little merit in multiplying the Appendices.

Henderson suggested that the word “certain” might be removed. This was an Article and it referred to species names generally. He also wondered whether, if this passed, one would end up with having to conserve names such as *Fabaceae* in an “aggregate” or restricted sense.

Veldkamp suggested that an aggregate species should not have a type specimen but a type species. This would solve Zijlstra’s problem.

Greuter had noted Henderson’s suggestion, which the Editorial Committee would have present when doing its work – if the proposal was adopted. Perhaps the Section might now wish to vote?

**Prop. A** was rejected on a card vote (403 : 283; 58.72 % in favour).

**Article 58**


Greuter explained that the whole of the present Art. 58 had caused headaches to many. Historically it was a remnant from the pre-type-method period that had been adapted to type language, which had resulted in a patchwork. Strictly speaking Art. 58 was altogether superfluous, as there was nothing in it that was not covered by other rules in the *Code*. However, many had appreciated its guidance, especially for didactic purposes, as its message was not spelled out coherently elsewhere in the *Code*. In view of its present complexity and patchy nature, he had tried to come up with an improved wording, which had found good support in the mail ballot. He hoped the rewording covered everything that was now in the Article (but not the possible addition as by Prop. B). It would make the article much more easy to read and understand – and also less dangerous, as some of its clauses had in the past been taken to carry a meaning they did not: they were merely of an explanatory nature, not supposed to enforce anything new.

Veldkamp entirely agreed with the proposal, except perhaps for minor changes, but felt that a reference to Art. 55.1 would be useful. The rule
that “a name may be legitimate even if its epithet was originally placed under an illegitimate generic name” was often overlooked.

**Greuter** added that, as Veldkamp of course realised, the two provisions covered quite different situations. Art. 55.1 referred to legitimate names whose epithets were placed under illegitimate generic names: they could not be used as such but were perfectly legitimate and had priority for transfer. In cases covered by Art. 58, the whole name was illegitimate, and the issue was what might happen to its epithet. The suggested cross reference might be useful but also confusing. This should be considered by the Editorial Committee.

**Prop. A was accepted.**

**Prop. B (23 : 95 : 88 : 0).**

**Greuter** explained that the proposed addition would cover a special case, not now mentioned explicitly: that generic names can serve as epithets in names of subdivisions of genera, and that epithets in names of subdivisions of genera (provided they have the appropriate form, i.e., are substantives in the nominative singular) can serve as generic names. If the Section felt that the matter was sufficiently important, the proposed text, suitably adapted, could be added to the reworded Article, perhaps as a second paragraph. Personally he felt this was rather an overkill, but the statement was factually correct.

**Prop. B was rejected.**

**Recommendation 58A**

**Prop. A (146 : 57 : 5 : 1).**

**Greuter** felt embarrassed at having produced what one might call an “isoproposal”: the same proposal had been published twice, his coming later (being the journal editor, he should have known). Both proposers had independently come to the same conclusion: Rec. 58A was inappropriate as it stood, and was not salvageable. The mail vote agreed.

**Prop. A was accepted.**

**Prop. B (6 : 191 : 11 : 1) was ruled as rejected.**

**Article 59**

**Prop. A (128 : 27 : 23 : 12).**

**Greuter** noted that the proposal was a mere rewording and a welcome clarification. The mail vote was very favourable, and so was the vote of
the Committee for Fungi (10 : 1). This clear recommendation by the specialists concerned should be followed.

Hawksworth agreed that the proposed simplification was very valuable.

Prop. A was accepted.


Hawksworth explained that the proposal related to Art. 59 Ex. 2, introduced in Yokohama to try to discourage people from abusing Art. 59.5 to create names that were not really needed. The proposal, which all those who disliked the proliferation of unnecessary names would welcome, was to spell this out in the main text. The Committee for Fungi supported the proposal (8 : 2 : 1).

Greuter reiterated the Rapporteurs’ question, in the “Synopsis”, whether this was a to be rule, in which case the verb “should” was inappropriate, or a Recommendation, when “should” was fine but the proposed placement was wrong. The predominant “ed.c.” vote (47 %) in the mail ballot favoured treatment as a Recommendation, to follow Art. 59. [Gams, as Secretary of the Committee for Fungi, indicated approval of that course.]

Demoulin agreed that in substance this was a Recommendation, but hesitated over its appropriate place. It directly followed from Art. 59.5. If separated, it would need an explicit statement to clarify that intent.

Hawksworth suggested that the Editorial Committee might consider a transfer of Art. 59 Ex. 2 to the new Recommendation.

Greuter [upon an unrecorded question from the audience] explained that “established” as used in the proposal did not have the sense of “validly published”. He then [following a dispute on editorial points between Demoulin and another Section member, off the tape record] indicated that the Editorial Committee would decide, and probably Demoulin would win as he was a member of that Committee: such was life. [Laughter.] The proposal should be accepted on the understanding that it would result in a Recommendation.

Prop. B was accepted on that understanding.

Article 59bis (new)

Prop. A (43 : 72 : 8 : 61) had been referred to the ad hoc Committee on Fossil Plant Proposals. It was subsequently withdrawn in favour of a new proposal (see under Art. 11).
Greuter introduced the issue of orthography, which was a great moment. To judge from the time spent on it, that issue had been one of the most important aspects considered at past Section meetings. Here as elsewhere – perhaps here more than elsewhere – whatever was decided should have the obvious backing of a large majority. When discussing and voting on these proposals, the Section should bear in mind that all orthographical rules it might approve would mean nothing if the botanical public did not follow suit. It was useless to burden the Code with changes that were not well supported by a large public within and outside the session room. It was better to live with the present Code if one had nothing better to offer.

Demoulin pointed out that, in the mail ballot, the highest positive vote for any of the proposals on Art. 60 was 25%; there were usually about 50% “no” votes, and also about 25% “sp.c.” votes. This meant that a small minority was in favour, a majority was against, and many did not care and would like to see the roundabout going on and on. If one accepted the principle of not considering proposals that had not received substantial support in the mail vote, i.e., if one would modify the agreed procedure and add the “sp. c.” votes and abstentions to the “no” votes, all of these proposals would be ruled as rejected. By going through all the proposals individually, a lot of time would be lost that, if saved, the Section might use to do important work. He moved that the Section reject all orthography proposals and ask the General Committee to devise a way to resolve the problems later – but not by again appointing a useless orthography committee! [The motion was seconded.]

Greuter pointed out that it was unusual and inappropriate to reject proposals en bloc. He suggested Demoulin’s motion to be equivalent to calling for the question on each individual proposal. If this was approved there would be no discussion, and the Section members would have to raise their hand in rapid sequence an indefinite number of times.

Demoulin’s motion, thus redefined, was carried.

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Art. 60


Brummitt paid tribute to the enormously hard work Zijlstra had done on all these proposals in the Committee on Orthography. It must have been very disappointing for her to see them all put away so quickly. She had been confronted with insuperable problems in trying to have her Committee agree on anything. He had been incorporated at a late moment into the Committee. When she was spending long hours writing reports, he was out in the field, and every time he came back to his hotel in the evening there was another fax urging him to vote that there would be a quorum and she could put some Committee decision on record. She had achieved an enormous task with great difficulties. The Section should thank her for trying to get something through. [Applause.]

[The following point was dealt with during the Seventh Session on Thursday morning.]

Zijlstra moved to delete the word “generic” from Art. 60 Note 1. [There is no record of her motion having been seconded.] The present text, “Art. 14.11 provides for the conservation of an altered spelling of a generic name”, dated from a time when only names of economically important species could be conserved.

Greuter acknowledged sloppy work by former Editorial Committees, for which he took part of the blame. He recommended adoption of the motion. Demoulin had made a note in his copy of the Code, that the word “generic” was indeed superfluous. Furthermore, the beginning of Art. 60.1 should be modified to read “Unless conserved, the original spelling …”.

Greuter thought this a good suggestion for the Editorial Committee, which Demoulin, hopefully still a member, would want to bring up. [He obviously failed to do so, as the change is not in the St Louis Code.]
**Zijlstra’s motion** was carried.

*Here the record reverts to the normal sequence of events.*

**Jansonius** asked Burdet whether he expected to establish another Special Committee on Spelling and Orthography; or did the way in which such a Committee’s work had just been rewarded put a caboche on any future such effort?

**Burdet** took it that yesterday the Section had expressed the clear wish not to have another such Committee. The question was not open, but could of course be reopened.

**Jansonius** felt that a better result could be achieved, though perhaps not by the past Committee in which there had been many difficulties. A different Committee might come to other results. He would not press the point.

*The following comment, relevant to Art. 60, was made during the Ninth Session on Friday morning.*

**Zijlstra** asked to be given five minutes to comment on matters of orthography. She would not move any change, but wished to poll the Section on its preferences among different approaches to orthography problems, much as Trehane had done for hybrids, because many were dissatisfied with the lack of progress in solving these problems. She defined two idealistic approaches at the extremes and a medium, pragmatic approach, with intermediate stages, all shown on the screen:

1. Everything that could be proven wrong was to be corrected.
2. Intermediate between 1 and 3.
3. The rules were to specify that: (a) generally accepted corrections of the past should stand, (b) almost no new corrections would be necessary, and (c) as few as possible cases would require conservation action.
4. Intermediate between 3 and 5.
5. What had been published would stand.

One problem of the Special Committee for Orthography had been lack of common ground. Most members who responded to the first circular held a position close to 4 or 5, while those who did not respond preferred 1 or 2. Her own position was close to 3. If, in the future, there were to be a new Committee on Orthography, or perhaps preferably an informal working group, it would have to be more homogeneous so as to have a chance to reach common ground. She asked that the Section
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members be polled as to their preferences with respect to her five approaches, the results to serve as a guidance to those seeking a solution. Burdet polled the section, which was about equally divided among the five approaches. “If you can make anything out of this – I wish you luck!” [Laughter.]

[Here the record reverts to the normal sequence of events.]

Recommendation 60B

Prop. A (21: 68: 121: 1) was referred to the Editorial Committee.

Recommendation 60C


Greuter noted the positive mail vote of 55% on the proposal, which came from the Committee on Orthography. It would widen the range of examples in the present Rec. 60C.1(a) by adding one case that was perfectly in line with what the Recommendation presently said.

McNeill suggested that this proposal and the following one, being both editorial, be referred to the Editorial Committee.

Prop. A was referred to the Editorial Committee.


Greuter agreed with McNeill that the proposal was of a similar nature as the previous one, adding a further case to the same Recommendation. Although it was hardly more than editorial it should better be approved.

Prop. B was accepted.


Greuter noted the special meaning of the “ed.c.” vote, which had been quite substantial: that [rather than adding the proposed text as an exception under Rec. 60C.1(b), as proposed] corresponding examples be added in the apposite place under Rec. 60C.2, which dealt with personal names with an established latinised form. Most female Christian forenames, and the proposal dealt only with those, had a well-established latinised form, as had the corresponding saints. The aforementioned procedure, which had been suggested by the Rapporteurs in the “Synopsis”, would best take care of the concern of the proposer.

Brummitt pointed out that most proposals on Rec. 60C, this one in particular, were not merely editorial. Prop. C involved a change of
current practice, and although he might support the proposed way of dealing with it, this should not be left to the Editorial Committee.

Greuter responded that the proposal was largely supporting established practice but would also get around some awkward consequences of the present Recommendation. Stearn would not have proposed anything that was contrary to current practice. If Brummitt agreed with the suggestion but wanted to make this a voted example this could be done, but it was hardly necessary.

Brummitt was interested to hear the views of those who opposed Prop. C, as he though many would.

W. Anderson asked whether this was the place where what is called a Recommendation is actually obligatory?

Greuter explained that Rec. 60C.1 was, but not Rec. 60C.2. The latter was not referred to in the provision on correctable errors [Art. 60.11].

W. Anderson pointed out that, as these would only be recommendations, those who disagreed did not have to follow them.

Voss had voted against the proposal in the mail ballot, as it would create a conflict with Rec. 60C.1(b), which provided for the inserting of -ii after a personal name ending in a consonant.

Greuter explained that this was exactly Stearn’s point. Botanical tradition regarding female Christian forenames deviated from what Rec. 60C.1(b) appeared to mandate through the backdoor of Art. 60.11. Tradition was almost uniform in this respect, with but few exceptions. Stearn had checked the numbers to show that stability, or rather tradition, were better served by his proposal than by applying Rec. 60.1(b). But as the Rapporteurs had pointed out, it was not mandatory to deal with those Christian names under Art. 60C.1, because they mostly had a well established latinised form and so could be dealt with under Art. 60C.2. The latter was not a rule through the back-door. When treating Christian names there, as by the Rapporteurs’ suggestion, botanists were perfectly entitled to form epithets in the traditional way.

Pedley made three comments: (1) traditional Latin forms did not exist for all feminine names, for example Muriel; (2), if the Recommendation need not be followed, a plant name could have two acceptable spellings, e.g., Acacia mabelae and A. mabeliae; and (3), at various times during the sessions, speakers had noted the general lack of knowledge of Latin
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in the botanical community, would one now end up with one rule for men and one for women? This would be ridiculous.

Greuter, on point (1), noted that in a case like Muriel, there obviously was a Latin botanical tradition [as demonstrated by the traditional use of the epithet murielae], and anyway, if the Section defined that there was, so there was. [Laughter.] Questionable cases could be listed as examples to make this clear. As to (3), Pedley was evidently in favour of unisex.

Jansonius reiterated the point that a Recommendation referred to in Art. 60.11 was thereby made mandatory, although he had never understood why. The meaning of an -i or -ii added to the name of a person to be honoured, was to reflect a distinction between family and given names. The first i, in -ii, indicated a patronymic stem augmentation. “Jule”, known to all as Caesar; was his given name, but his children became the Julius clan, so in the genitive the -i was doubled and became -ii. First names were never provided with an extra -i, so if a first name was latinised, -us (or -a for a female) was added; which in the genitive became -i (or -ae). This was as Stearn had said. The problem was that few people had at least a basic understanding of Latin. [Having requested and obtained a show of hands] he was gratified at the number among those present who had. Yet, lately in publications he had noticed how little the Latin behind our nomenclature was understood. The Code should try to keep as pure as possible its rules based on Latin.

Greuter reiterated that, whereas Rec. 60C.1 was made mandatory by the back-door clause of Art. 60.11, Rec. 60C.2 was not. As Pedley had correctly pointed out, there was one major difference between the Rapporteurs’ suggestion and Stearn’s proposal. When such Christian names were treated by definition under Art. 60C.2, then a spelling such as murielae was not correctable if originally so published, neither was murielae correctable. In traditional usage murielae won by lengths, but if treated under Rec. 60C.1 of the present Code was to be corrected to murielae – which no one did. Under Stearn’s proposed amendment, murielae would be enforced, and murielae would have to be corrected to murielae. He usually supported standardisation, and would normally have recommended Stearn’s proposal as submitted, but the proposed selection of female Christian epithets worried him. It looked as though it came from an English dictionary of Christian names. There were female forenames, including non-Christian ones, in many other languages, for which the proposal’s results would be unusual if not
disruptive. There were, e.g., Spanish names ending in -n, such as Concepción, for which no epithet ending in -nae had ever been published, as they were treated as latinised under Art. 60C.2 and given the genitive ending -nis. The liberal solution of treating all female Christian forenames under Art. 60C.2 was preferable, even though it did not lead to complete standardisation.

Brummitt felt that treating such cases under Rec. 60C.2 completely took the teeth out of the proposal. Greuter erred if he felt that everyone was united over the spelling Arundinaria murielae, the case that had triggered the proposal, as it was hotly argued amongst bamboo specialists who wanted guidance on whether they should put the additional -i-in or take it out. Stearn’s proposal intended to clarify this.

Filgueiras, who had taught many courses of botanical Latin, pointed out that what was being discussed was simple, basic, grammar. If a feminine name was considered as belonging to the first declension, the genitive had to end in -ae, and if a masculine name was considered as belonging to the second declension, the genitive singular ended in -i. He did not want to support either option, but just give the basic facts.

Demoulin noted that the Section had just been given a practical demonstration of how difficult it was to work with several people on an orthography issue. A way out would be to add a Note under Rec. 60C.2: “Traditional given names, even non-classical, are considered to have well established latinisations”.

Greuter liked Demoulin’s suggestion, which he [and others] seconded as a motion. Some examples taken from Stearn’s list could then be added, which would incidentally remove the present male bias from Art. 60C.2, where [having overlooked Beatrix] he saw only males.

Voss definitely opposed the motion because, while it was optional to follow Rec. 60C.2, the result would contradict what was now mandatory under Rec. 60C.1(b). Besides, how was one to know what the well-established latinised form was to be when it did not exist?

Demoulin, as the inventor of Rec. 60C.2, reminded Voss that it provided for exceptions to Rec. 60C.1 (turned into a rule by Art. 60.11). An epithet placed under Rec. 60C.2 was no longer covered by Rec. 60C.1. What Stearn had really wanted was to avoid the absurd standardisation of this kind of name under Rec. 60C.1(b). The alternative was to change Rec. 60C.1(b), which is what Stearn had proposed and
would have been fine if he had not overlooked the problem that the Rapporteurs had pointed out, with names like Concepción. In view of that difficulty, it was preferable to place the matter under Rec. 60C.2, which effectively sheltered these epithets from the noxious effects of the mandatory Rec. 60C.1.

Veldkamp, noting that the proposal referred only to female names, wondered whether the word “feminine” should be added in the amendment.

Greuter declared that masculine names were already covered in principle, by way of examples, by the present Rec. 60C.2.

Wiersema added that, with the proposed amendment, existing epithets (as in _Arundinaria murielae_) would not be left in limbo: for them, as Greuter had explained, the original spelling would have to be accepted. Rec. 60C.2 would provide guidance on what spelling to adopt in forming new epithets.

Zijlstra disliked the idea of dealing with this matter under Rec. 60C.2. This would result in a situation where two different spellings were to be used side by side, one ending in _-ae_, formed in agreement with Rec. 60C.2, which was not however obligatory; the other in _-iae_. This would be contrary to Stearn’s intent.

Nicolson was uncomfortable with the intended distinction between given names (no augmentation) and other names (augmented). In various language groups, for example in China and India, each had their own naming traditions, and to sort out which name was a given name was not easy – look at Kancheepuram N. Gandhi. He had come to the conclusion that it might be better to add the augmentation to all names without caring whether or not they were given names. He was against Stearn’s proposal and also opposed the amendment.

Gandhi confirmed Nicolson’s remark that the situation was extremely complex for Indian personal names, as patterns varied widely within the country. Especially in the south, people used their given names for all official purposes. In telephone directories or degree certificates the given name was placed at the end of the name. There was the additional complication of caste names, which could come last, the surname being placed first, and the given name omitted.

Palacios-Rios, being of a Spanish mother tongue, agreed with the use of latinised feminine given names, correctly declined, in the formation
of epithets. She preferred a placement under Rec. 60C.2, which would keep the spelling optional.

Demoulin, in the wording of his motion, had deliberately used the term “traditional” to avoid the problems with names in other than western European languages. He now realised that there were traditional given names also in other parts of the world. Perhaps it was more appropriate to refer to “Given names traditionally used in botanical Latin...”

Greuter wondered whether it was appropriate to narrow down the provision. The proposal gave latitude for suitable traditions to develop on a case-by-case basis. If Indian ladies developed a preference for being honoured by an -ae termination, so be it; if they preferred -iae, no problem. The Code could not foresee what was most palatable to the many nations around the world. Placing given names or forenames or their equivalent under Rec. 60C.2 in a general way would give full latitude for traditions to develop that were in line with euphony or taste in each of the various language groups. Once these traditions had developed, one could then perhaps fix them by standardisation. At present the uncertainties were so many that one could only lose by treating the matter under the mandatory Rec. 60C.1.

Brummitt wished to revert to the basic proposal. If the woman’s name was Muriel, Rec. 60C.1(b) required that the epithet be murieliae. If it had been published as murielae, it had to be corrected to murieliae. Steam’s proposal would effect the opposite: the spelling would have to be murielae even if originally published as murieliae. This would be a change to established practice. While not siding strongly one way or the other, the felt that a decision was needed. He had no answer to the question of Asian names, although it might be possible to find some wording to the effect that the provision applied only to names derived from established European languages. Stearn’s proposal was to have the wording in Rec. 60C.1 so that it would be mandatory. Putting it in Rec. 60C.2 would make the provision completely useless and would only confuse everybody, as this was a Recommendation that did not have to be followed.

Burdet pointed out that Demoulin’s motion was being considered. Discussion would return to the original Prop. C if it was defeated.

Greuter contradicted Brummitt. Demoulin’s motion was neither useless nor confusing. Presently the epithets in question were governed by Rec. 60C.1 and must be standardised in a sense opposite to the one which Stearn favoured for good reasons. As the time was not yet ready
for full standardisation in that sense, the motion would be beneficial as it would avoid the obligation to standardise *murielae* or *edithae* into *murieliae* and *edithiae*.

**W. Anderson** noted that the Section was spending a lot of time considering an addition to the *Code* that would mainly deal with two epithets. This seemed to be the opposite of evening the table legs by shortening them, it was adding a little bit to each leg for dealing with this and that situation. This had not been well thought out, one could not foresee the consequences, and one should not change the *Code* in such circumstances. He would vote against both the amendment and the original proposal.

**Greuter** observed that more than two examples were concerned. The full list, published in Stearn’s proposal, included about 30 names.

**Demoulin’s motion** was defeated.

**Veldkamp** pointed to a conflict between the wording of Stearn’s proposal and the example of Beatrix in Rec. 60C.2. Beatrix, although it ended in a consonant, resulted in *beatricis*, not *beatrixae*.

**Filgueiras** supported Brummitt’s idea, but even if the proposal were adopted it would not preclude the termination *-iae*. For example, the name Olivia resulted in the genitive epithet *oliviae*, because the *i* was not part of the termination but belonged to the stem. With regard to Beatrix, this, like Conceptio, was a third declension noun; the genitive epithets were *beatricis* and *conceptionis*.

**Kolterman** wanted to be reassured, before a vote was taken, that the example cited in the Rapporteurs’ comments, Concepcion, and other names such as Milagros, Carnación and Flor, used for both for men and women, were covered by Rec. 60C.2 as having classical Latin forms.

**Greuter** replied that, at least for Milagros, this was certainly not so.

**Silva** observed that this was a good example of the conflict between the stability of rules and the stability of names. The stem augmentation question had been debated in nomenclature since at least fifty years, ever since he had been in the business. The rule had remained; yet few of the botanists who liked the stability of rules had followed these rules. He himself had followed them, and in nomenclatural notes to a 1250-pages catalogue published in 1996 he had brought all such commemorative names into conformity with the *Code*. To change after all these years just for one exception would make the whole field an object of ridicule.

**Prop. C** was rejected.
**SEVENTH SESSION**

**Thursday, 29 July 1999, 9:00-13:00**

**Recommendation 60C (continued)**

**Prop. D (35: 77: 47: 52).**

**Greuter** introduced the proposal. A Russian author sought clarification on the derivation of epithets from female Russian surnames. There were two options: one was linguistically correct in the proposer’s opinion, the other was presently mandated by the Code. Usage was 2 to 1 in favour of applying the present rule. The proposer wished the Section to consider switching to non-prevalent usage, as it was grammatically more appropriate. It would however be awkward to change a rule to make it conflict with usage, in a spirit of correctness that few botanists from outside Russia would understand. Also, if grammar-based special rules were introduced for Russian names, proposals from other language domains with completely different grammars and traditions were inevitable, each seeking to have its own exceptions written into the Code – perhaps with exceptions to the exception for, say, Dutch or Indian botanists when they became established in the English language domain. The Rapporteurs had therefore suggested that it might be more appropriate to leave the Code as it was and add an example showing how the present rule worked when applied to female Russian names. It would however be good to know how Russian botanists present in the room did feel.

**Egorova** thought the proposal to be unnecessary. The origin of Russian female surnames was not important for the formation of epithets. Most botanists at the Komarov Institute thought that epithets derived from Russian surnames should be formed in accordance with the current rule.

**Greuter** welcomed this clarification, which was important as it came from the country concerned. If the proposal was rejected, the Editorial Committee would feel free to add a suitable example.

**Demoulin,** the day before, had proposed to act on proposals relating to Art. 60 without discussion. He had meant to include the proposals on Recommendations. The Chair had thought of it otherwise. This was fine – but the long discussions on Prop. C – by Stearn, the most respected expert in orthography matters – had not resulted in any agreement. The Section had tried to fix the single flaw in the proposal, on third de-
clension names, and had failed. This was an excellent demonstration of the near impossibility of discussing orthography constructively in a group, whether it be the Nomenclature Section of a Congress or a Committee. He had been accused of laziness because he had not participated in the work of the last Special Committee on Orthography. But he was not lazy, he disliked losing his time and see others losing their time. This is why he opposed renewing the past experience and setting up once more a Committee on Orthography. On a matter like Lectotypification, a Special Committee might come up with consensus solutions, and the Section, even if not fully understanding all implications, would follow. In a Committee on Orthography, the usual result of a vote was: four in favour, three against, one abstaining; and the Section, unsure of the meaning of the proposal, would vote “no”. His earlier calling for the question did not result from hostility toward the Committee or its Secretary, whose hard work he fully appreciated, and he apologised if it had been so understood. No more time should be spent on orthography now. For the next Congress, any proposal should either limit itself to the general principles or deal with technical points, but not mix the two aspects.

Greuter noted that only few proposals on orthography recommendations were left, including one supported by a positive mail vote. It was appropriate to proceed in the normal way but be concise, in consideration of those who were by now overfed with orthography. For the present proposal, the understanding was that if it was defeated the Editorial Committee would be empowered to add an example of how the Code worked.

Prop. D was rejected.


Greuter observed that the proposal had received a 55 % positive mail vote, and would effect an editorial improvement.

Prop. F was accepted.

Recommendation 60E


Greuter explained that the prevalent “ed.c.” vote (53 %) on the proposal supported the Rapporteurs’ suggestion that the Recommendation, rather than being deleted, should salvaged by substituting the words “correct” or “usual” for “original”. If the proposer would accept the
Rapporteurs’ advice as a **friendly amendment** [he did], the Section could vote on the proposal as modified.

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

**Recommendation 60G**

**Prop. A** (57: 7: 130: 17) was referred to the **Editorial Committee**.

**Recommendation 60H**


**Greuter** noted the mildly favourable mail vote. The Committee for Fungi might want to give an opinion. [Reply by **Gams**: the Committee had no objection, not having considered the matter.] At present, Rec. 60H was one of the “back-door rules”, disguised as a Recommendation, and one part of the proposal was to transfer it to Art. 60. Two other cases of “back-door rules” on orthography would remain [Art. 60.8 and 60.8], but on this minor point it might be tidier to rectify the situation. That change was basically editorial, but would be so apparent that the Editorial Committee would not feel empowered to effect it without the Section’s consent. The second part of the proposal would expand the provision, to cover fungi that were parasitic on or symbiotic with animals. Naming fungi after their animal host or commensal was traditional in mycology, and it seemed logical that the rule should apply in such cases as well. Several prominent mycologists in the room were nodding.

**Demoulin** reported on the last Committee for Fungi meeting. Gams, the only one to comment, had felt that the proposal was an improvement. Nobody had objected, so the Committee’s approval could be implied.

**Hawksworth’s** reason for wanting the Committee’s advice was that he had no feel for how many epithets in mycology were derived from animal names, nor for the correct spelling of the animal hosts’ names. The provision was straightforward when the associated organism was a plant, for which mycologists knew the correct spelling or could easily check it. With, e.g., insect groups the case might be less clear. The expanded rule might perhaps destabilise current names.

**Greuter** noted that none of the Committee members present felt it did. Also, it had been rightly pointed out that hyperparasitic fungi were not presently covered because – as Hawksworth would appreciate – their substrate was not a host plant but a host fungus.
Voss, who favoured the proposal, could not help but point out that it created disharmony with the zoological Code. Animals that fed on plants were not obligated to be named in accordance with the correct spelling of that plant. For a moth that fed on *Galium* the epithet *galli* had been published, and zoologists were obligated to follow that original spelling. He supported this disharmony. [Laughter.]

Prop. A was accepted.

**Recommendation 60I**


Greuter noted that the proposal, to delete a superfluous word, was hardly more than editorial and should be accepted.

Prop. A was accepted.

**Article 62**

Greuter announced two proposals from the floor on gender of generic names. Both aimed at stabilising the gender of one particular word element, as had been done for other such elements under Art. 62.2(a) to (c).

Compère introduced a motion on behalf of the Permanent Committee for Algae, which had voted 12: 2 in favour of it [so that no second was required]. In the second sentence of Art. 62.2(c), after "-anthos (or -anthus", the words "-phykos (or -phycus)" were to be inserted. The Greek word *phykos* (seaweed) was neuter, but had been generally treated as masculine in compound generic names. In about two thirds of the cases such names were treated as masculine, and only in one third, as neuter. Adding -phykos to the exceptions for -anthos and -chilos, currently in the Code, would stabilise the nomenclature of more than 50 algal generic names, especially of macroalgae.

Greuter recommended acceptance. He was unaware of any usage of -phycos or -phycus outside the phycological domain, but even if there was, it was pretty certain also to be masculine. This was one of the rare cases where a Greek noun ending in -os was neuter. Transliteration into Latin gave -us, which still was neuter. But as soon as the *ph* was changed to an *f*, making it the word fully Latin, it became masculine *fucus*. No one had ever considered *Fucus* to be neuter.

Compère's motion was carried.

Greuter called for the second motion, to define -glochin as a feminine.
McNeill wondered if it was appropriate to introduce the proposal at this time because it had not been reviewed by the Committee for Spermatophyta. The motion would define as feminine the termination -glochin, which had been treated alternately as feminine or neuter, with feminine usage prevailing. The present Code could be interpreted as favouring feminine, but was not explicit, so both usages persisted. The feminine status of -glochin would be made clear by including it in Art. 62.2(b). [The motion, to add “-glochin” to the examples listed in the first sentence of Art. 62.2(b), was seconded.]

Brummitt replied positively to the question whether the motion was appropriate at this point. He had received some correspondence from McNeill on the subject. If the motion were accepted, this would save two conservation proposals.

Voss found it misleading to refer to -glochin as a termination. According to his Greek lexicon, the word triglochin was a word in its own right, meaning three-pronged anchor, to which the fruit of Triglochin palustre resembled. He supported retaining neuter gender for it.

Greuter noted that, even though McNeill had referred to a “termination”, the wording in the Code was: “Compounds ending in -achne, ...”.

Demoulin supported the Code’s present wording; chlamys and daphne were also words standing on their own.

Greuter explained that -glochin would be inserted in its alphabetical place, after -daphne and before -mecon. Nothing else would be changed.

McNeill’s motion was carried.

**Prop. A (69: 137: 3: 6).**

Hawksworth the proposal’s background was the set-up, jointly by the International Union of Biological Sciences and the International Union of Microbiological Societies, of an International Committee on Bionomenclature. An ongoing interface of botanical nomenclature with that body, to look at its proposals at an early stage and perhaps prevent repetition of the acrimonious debates that had occurred recently, would be useful. There would certainly be proposals arising from the ICB meeting at the IUBS General Assembly, the following year, and from the bacteriologists at their international congress in a month. He did not plan to be a member of the proposed Permanent Committee.
W. Anderson said that for the past few years a small group of people had been going around the country, telling botanists, bacteriologists, and zoologists in turn that the other groups were about to jump on the BioCode train, so they should better jump on not to be left behind. Approval of this proposal would surely be presented and taken as an implicit endorsement of the BioCode by botanists and their readiness to abandon their autonomy. Voting down the proposal was the botanists’ opportunity to get rid of the BioCode for the foreseeable future and to communicate to other biologists that they did not want to lose their autonomy. Everyone who opposed the BioCode should vote against the proposal.

Hawksworth felt that W. Anderson’s statement was totally misleading. It showed lack of familiarity with the Committee’s remit.

Chaloner was much in favour of the proposal and did not think it held the threats that some saw in it. He was sorry to see how emotive an issue harmonisation, like other themes repeatedly discussed these days, had become. He did not know why the concept was seen as so threatening for botanists. For anyone teaching in biology and trying to sell botany to others, it was absurd that botanists should be so stuck in the mud that they were not even prepared to use the same language as, e.g., zoologists. At a time when so much was bringing life sciences together, botanists must not turn their backs on their colleagues working in other areas of biology. They should be seen as prepared, at least, to discuss with others the mutual nomenclatural problems.

Stuessy saw no harm in keeping an eye on harmonisation. To totally withdraw would not be a productive way to handle the issue. There was, however, a problem with establishing a new Permanent Committee, considering the mandates and responsibilities of those that now existed under the Code. They were the General Committee, the Editorial Committee, and a number of taxon-based committees. Establishing a Permanent Committee on Harmonisation did not feel right, because it would indicate total commitment of botanists to the concept of harmonisation. He moved, as an amendment to the proposal, that a Special Committee on Harmonisation be established. This would allow botanists to continue to work on the issue and amass facts, and would be an appropriate vehicle to offer proposals if desired.

Hawksworth was happy to accept Stuessy’s motion as a friendly amendment. Upon request for clarification from the audience, he explained that special committees were established by a particular
Congress to work between two Congresses, while permanent committees, enumerated in the *Code*, were ongoing and had their membership updated at each Congress. Without renewal, the mandate of the existing Special Committee on Harmonisation was coming to an end.

Greuter strongly supported the amended proposal, which provided a good solution. The standing of permanent committees differed from that of special committees authorised by a Nomenclature Section, then set up by the General Committee, to report to the next Congress. The members of permanent committees were appointed by the Section and Congress on a proposal from the Nominating Committee, but the latter would be in trouble if it had to come up with a list of members on such short notice. As Stuessy had aptly explained, the least botanists could ask for was to be kept informed of what was going on in the world at large, which would be the main remit of the proposed Special Committee.

Zander held that the only reason to change rules or to make new rules should be to advance science, the science of botany not nomenclature. This consideration should guide those voting. The present campaign of fear, uncertainty, and threat was inappropriate.

Greuter pointed out that the original proposal was no longer on the floor. The proposal for a Special Committee would not add any text to the *Code*. It would authorise the General Committee to establish a Special Committee on Harmonisation, to report to the following Congress.

Filgueiras requested an explanation, in simple terms, of the advantages for botanists of a *Code* harmonised with the other branches of biology.

Greuter feared this would lead the Section astray from the point. In view of the clearly expressed wish of the Section, a new Special Committee on Harmonisation would not have in its remit the study of ways towards a harmonised *Code*. Obviously, any committee had latitude to consider what it might wish, but this would be outside its formal remit.

Demoulin could understand that vascular plant taxonomists, probably a majority of those present, were not concerned with possible conflicts with other *Codes*, but people working with fungi and algae had permanent problems with organisms also treated under some other *Code*. They needed the proposed Committee, not for the fun of harmonisation (or perhaps better relationship) of *Codes*, but to look into their problems.

Farjon agreed with Demoulin. If the remit of the Special Committee was not to look into the possibilities of Harmonisation – written with a
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capital “H” – then one ought to consider whether to use the word “harmonisation” in its name, or a word that better reflected its remit.

Borhidi supported the proposal. Botanists had to face future trends in systematics and taxonomy, which included new integrative fields like coevolution and plant-animal relations, so they had to familiarise themselves with zoological nomenclature and nomenclatural terminology.

Watson was dissatisfied with the loose remit of the committee. Unless it were defined tightly, he could not support the proposal.

P. Stevens suggested the Committee’s name to read “Special Committee for Liaison with other Codes”, to describe what it was supposed to do.

Hawksworth accepted the suggestion as a friendly amendment. The main difficulty in defining the remit too tightly was the need for a flexible body that could respond to discussions as they might arise during the next six years.

Prop. A was accepted as amended.

[The following motion, relevant to Div. III, was dealt with during the Ninth Session on Friday morning.]

West introduced a motion on behalf of the Nominating Committee. During the past week the Committee had found itself unclear on the procedures and structure of Division III of the Code. They wished to propose that a Special Committee to review Division III be set up. [West’s motion, coming from a Committee, did not need being seconded.]

Greuter always welcomed special committees appointed to look into specific problem areas. West might perhaps specify whether the problems concerned the whole of Div. III or specific parts of it, so that the mandate of the Special Committee would become clearer.

West explained that the questions did not concern the section on Permanent Committees, but mainly the institutional voting procedures. However, the entire Division might need an overall updating. It was not clear to the Committee how some of the procedures had been established, which had become rather mixed and intertwined.

Greuter suggested that it might be useful to add “especially Div. III.4” to the motion, as that was the portion covering institutional votes. This would not strictly limit the mandate.

Davidse explained that he and other new members of the Nominating Committee had found that the procedures by which the members of the
various committees were elected were not spelt out as clearly as might be desirable, so Div. III.2 should also be reviewed. Div. III.1 was not in question, but looking at Div. III.3 might be useful. Div. III.4 was of primary concern, and the Committee felt strongly that the issue of institutional votes needed review. When reading the Proceedings of past Congresses he had come to realise that the matter was quite complex. Appointing a Special Committee seemed the most reasonable approach.

Those who had attended the IAPT meeting on the previous day knew of the proposal to form a Committee to review the constitution of IAPT. If the proposed Special Committee were approved, it might be appropriate to have some overlap in the membership of these two Committees. Perhaps the same person might chair the two groups. As everyone recognised, IAPT was involved intimately with the Nomenclature Section by publishing the Code and the Committee reports. Their close relationship and the way in which they interact was another area worth investigating. Divisions III.2-4 were the ones the Committee had in mind.

Greuter, having obtained a clearer explanation from the proposers, did not think that a formal amendment of the proposal was necessary. The Section could now understand the issue. As there had been concern about one or more of the areas, they should certainly be looked into. He volunteered some advice to the to-be Committee: that they might come up with proposals that were solid and not too sweeping, proposals that would be acceptable to a 60 % majority at the next Congress, because otherwise their work would be in vain. Division III was a solid, tradition-fraught instrument, but not all procedures that had been established over the years were spelled out in it, as would be useful in the interest of greater transparency. However, major changes in Division III might be utterly destabilising, as they might trigger changes in the concrete rules elsewhere in the Code. He warned the Committee to be very conservative in its views; to seek carefully for a clear wording that conformed to traditional practice, as that practice had worked successfully all along the Congresses, to the equally shared satisfaction and dissatisfaction of all concerned. This was just advice from one individual person, nothing more. He urged that the motion be approved: this would fulfil a need and the wish of many present – and many not present, too.

West's motion was carried.

[Here the record reverts to the normal sequence of events.]
Appendix I

Prop. A (0: 140: 8: 0) was ruled as rejected.


Greuter, as the author of this proposal which was scarcely more than editorial, was surprised at the majority of “no” votes (51%) in the mail ballot. The proposal would become irrelevant if the hybrid Appendix were to be deleted, which he understood would not be the case. Art. H.12.1 presently stated that an infraspecific epithet could be placed subordinate to the name of a nothotaxon of infraspecific rank. Nomenclaturally, there was however no placing of infraspecific epithets under higher-ranking infraspecific names. Art. 24.1 was clear: infraspecific names consisted of the species name and the infraspecific epithet, plus a rank designator in-between. The expression “genus A species B subspecies C variety D” was a classification and not a name. The name was “genus A species B variety D”. Art. H.12.1 was simply incorrect, perhaps a left-over from times when the Code employed sloppy language in this respect, and the proposal would remedy this.

Prop. B was accepted.

[At this point discussion of the proposals concerning fossil plant names (see under Art. 3 and 11) and on the hybrid package (see under Pre. 8) took place. The end of the hybrid deliberations follows.]

Trehane introduced a new proposal from the floor, to delete Art. H.3.3, “The epithet in the name of a nothospecies is termed a collective epithet” and Art. H.3 Note 2, “The term ‘collective epithet’ is used in the International Code of nomenclature for cultivated plants-1980, to include also epithets in modern language.” Neither did any longer apply. The current cultivated plant Code did no longer use the phrase “collective epithet”. In fact, it had been a misnomer, as all names and epithets were, by definition, collective.

Greuter thought that the proposal sounded sensible and was basically editorial, since the current text was wrong. should be referred to the Editorial Committee [Trehane agreed], which would verify that the contradiction was real and make sure that nothing wrong was deleted, while deleting what was wrong.

The new proposal was seconded and referred to the Editorial Committee.
EIGHTH SESSION
Thursday, 29 July 1999, 14:00-15:00

[All business conducted during the Eighth Session was relevant to provisions of the Code dealt with earlier. The proceedings of the corresponding debates can be found under Art. 30 Prop. D (Keil’s motion), Rec. 30B and 30C (conclusion), Art. 21 (Greuter’s motion), Art. 8 (Silva’s motion), and Art. 14 (Reveal’s motions).]

NINTH SESSION
Friday, 30 July 1999, 9:00-12:00

[All business conducted before the coffee break during the Ninth Session was relevant to provisions of the Code dealt with earlier. The proceedings of the corresponding debates can be found under Div. III (West’s motion), Art. 53B and 46C (Gandhi’s motions), Art. 60 (Zijlstra’s comment), and Art. 15 (Stuessy’s motion).]

Appendix IIIA

Greuter, before introducing a specific question on App. IIIA, explained the process by which the Code was going to be published. Members of the Editorial Committee present would, at the end of the session, set a date for their meeting, which the last two times had been in Berlin at the beginning of the next subsequent year. The Committee would have at its disposal the rough transcripts of the taped sessions’ debates, so that it could verify what had been said. All who had moved new proposals or amendments from the floor should hand in to Barrie the exact wording, so that it was certain to be available to the Committee. The outcome of the present meeting, the Section votes, would appear in the November issue of *Taxon*, along with a new version of the mail vote results, with the handout’s misprints corrected. Anyone who knew of a good example to illustrated a new or modified rule, or even an old rule, was welcome to send it either to himself, McNeill, or Barrie – but should please do so quickly. A first draft of the body of the *St Louis Code*, incorporating the Section’s decisions and any new examples, would be distributed to the Committee within a month. Any suggestions received later than the turn of the year would likely come too late for being considered. As the *Code*
covered all kinds of botanical organisms, examples from cryptogamic groups, notoriously underrepresented, were particularly welcome. A final draft would result from the Committee’s plenary meeting, a week that would be at least as concentrated as the one the Section had just lived through; after which, work on the appendices would start. Thanks to Reveal’s kindly motion, the Committee would derive an extra kick from dealing with family names. To be serious: he was sorry that the Section had decided against maintaining the clean current version of App. IIB which, along with all other appendices, had gone through several painstakingly detailed revisions over the past two or three editions.

Here now came the question he wanted to ask. It was editorial, so there was no decision to be taken, but the Editorial Committee would be guided by the Section’s expressed preference. Ever since Vienna when conserved names were first introduced, the Code had followed a peculiar, traditional sequence for conserved generic names of Spermatophyta, now in App. IIIA(E): they were not arranged alphabetically, as in all other groups, including fungi and the main algal divisions, but followed the system of Dalla Torre & Harms. (This was perhaps a bad place to object to that sequence, as St Louis still used it for arranging their herbarium.) The longer one kept using that system, the more it had become outdated, as reflected by the numerous entries in which a small letter was appended to the Dalla Torre & Harms number, indicating a genus name not adopted by these authors; and also by a large number of now obsolete family placements. The Editorial Committees of past Codes had considered placing all names in alphabetical order, which would also ease indexing and simplify the index, but had not wanted to implement such a change without knowing whether it might cause bad feelings. The question put to the Section was: would it be very awkward if, in the next edition of the Code, App. IIIA, Spermatophyta, were to appear in alphabetical rather than Dalla Torre & Harms order?

McVaugh was all for alphabetical order, but would find it awkward if it were broken down to genera. He now had to use the index when looking up a conserved name. But being usually interested in which names in a particular family were conserved, he would prefer an alphabetic arrangement of families to an arrangement of all genera in a single alphabetical series.

Greuter, thanking McVaugh for his input, first wanted the Section’s opinion on Dalla Torre & Harms per se. If it was negative, he would then ask whether the alphabetic sequence of genera should be done
within the Spermatophyta as a whole, within Gymnosperms and Angiosperms, or within individual families. Was there any kind of emotional affinity with the Dalla Torre and Harms system in the audience?

Frodin pointed out that if one were to alphabetise family names, one would have to decide which system of families to use.

Bhattacharyya thought alphabetical sequence would be helpful, as Dalla Torre & Harms’s Index was absent from many libraries. Herbaria such as Copenhagen were now arranged alphabetically, as were many important reference books, e.g., Willis’s Dictionary.

Sousa noted that his institution’s herbarium [MEXU] was arranged by Dalla Torre & Harms, and people liked it because it was convenient for identifying plants; related taxa being close to each other. But for the purpose of locating a name in a book, alphabetical sequence was fine.

Greuter requested a show of hands on the first question: who was in favour of retaining the Dalla Torre & Harms sequence in App. IIIA: Spermatophyta? Who?? [No hands were raised.] He had rarely seen the Section so unanimous! [Laughter; as someone asked what the question had been, the poll was repeated: again unanimity!] Rather than asking whether a break-down by families was preferred, he requested suggestions as to which system should be used to define the to-be alphabetised families. [No specific system was recommended.] Was there support for alphabetising by families? [There was not.] Was there support for separating the gymnosperms and the angiosperms? [There was.] Should the paraphyletic dicots be separated from the monophyletic monocots? [There was some support, but a majority against.] The Section had given to the Editorial Committee exactly the guidance it needed.

Pipoly suggested that the Editorial Committee post all submitted examples on the IAPT Web site as soon as possible, giving people the chance to look them over and, perhaps, provide a better example.

Report of the Nominating Committee

Nicolson presented the report of the Nominating Committee, copies of which had been distributed. There had been a good deal of negotiations and revisions. He wanted to thank the members of the Committee individually, because they had worked very hard: William Chaloner; Patricia Dávila; Gerrit Davidse, who had done a yeoman’s job in getting copies made and distributed; Knut Faegri, who had offered his many years of expertise and experience; Walter Gams, who was full of
very constructive ideas; Ching-I Peng who had made valuable input, as had Gideon Smith; and Judy West, who had stepped up and taken the ball many times when it was about to be dropped.

The report itself was presented as a slate. Assuming it would be accepted, one correction was needed. John McNeill, as Rapporteur-général, would no longer be a member-at-large of the General Committee, but an ex officio member with a special office, as was mandated in the Code. There were eight Permanent Committees mentioned in the Code. Nominations for committee membership had been submitted by each of them, through their chair or secretary, and had been honoured and respected by the Nominating Committee, because of these groups’ expertise and their knowledge of the experts who were best to serve. He felt comfortable with the slate the Nominating Committee was putting before the Section for its approval, with the aforementioned correction.

Dorr asked for a clarification. On the list for General Committee, under Secretary, there was Barrie’s name in brackets. What did this mean?

Nicolson replied that he felt the time was approaching for him to step down as Secretary, and he would do so after a transition period of about a year, enough to get the new General Committee functioning. He had suggested to the Nominating Committee that as the Section’s Recorder Barrie had not done his job too badly, had not made too many mistakes, and maybe he should be sentenced to that office. [Laughter.] So his name had been added in square brackets, as a provision for the future. This had been his personal suggestion, which the Nominating Committee had accepted. They had decided to leave Barrie’s name in, because if it were missing except on the Editorial Committee, one might think they did not like him.

Greuter suggested that, since Barrie would not hold the office for another year, he be listed for the time being under the members-at-large. Otherwise he would have no standing on the General Committee, which would be absurd. [Nicolson accepted this suggestion.]

Wagner requested a clarification. With McNeill being upgraded, had another member-at-large to be added? Was a specific number needed?

Nicolson thought not. In view of the procedural 60 % majority rule, a total number divisible by five was desirable, but not mandatory; a Committee of 25 (now 26) was already quite large.
Reveal would gladly step aside in favour of Barrie, if need be. His membership could be reconsidered upon Nicolson’s retirement, if appropriate. Nicolson accepted his offer. The handout was essentially a first draft. If Reveal stepped down and Barrie was included, this would appear in the published version.

Stuessy, maybe missing something, thought that if McNeill went up as Rapporteur-général and Barrie was added in his place, the count was still 25 and Reveal could stay on.

Nicolson explained that at the moment, Barrie was sitting in square brackets. He was to be added as a numbered Committee member.

Funk asked that this be left to the General Committee to decide. There was general agreement on the people listed; this was not really an issue. Jorgensen wondered whether McNeill was living in the U.K. or Canada. On the Editorial Committee he was listed as from the U.K.

McNeill explained that he should be listed under Canada for the time being. He was officially in Toronto until at least November. He intended to relocate to the Royal Botanic Gardens, Edinburgh, in the near future, and at the time of the next Congress expected to be based there.

The Report of the Nominating Committee was approved unanimously. [Having been published separately (in Taxon 48: 775-776. 1999), it is not reprinted here.]

Reports of the Permanent Nomenclature Committees

Committee for Algae

Compère had been unaware before coming that he was supposed to deliver a report to the assembly, but nevertheless presented the following Report. The Committee, established in Yokohama, consisted of 15 members: the Chairman, P. C. Silva; G. Furnari; Linda Irvine; Jerzy Komárek; H. Lange-Berthalot; E. C. de Oliviera Filho; P. Parkinson; D. J. Patterson; A. K. A. S. Prasad; W. F. Prud’homme van Reine; A. Sournia, K. L. Vinogradova, W. J. Woelkerling, T. Yoshida, and P. Compère, Secretary. Three members had resigned from the Committee: L. Irvine, P. Parkinson, and A. Sournia. They had been replaced by Visitación Conforti, J. Larsen, and J. Bolton. During the term between the two Congresses there had been several communications between the members each year, circulating proposals to conserve or reject and opinions, remarks and votes by the members. Six reports were prepared,
five of them published in *Taxon* [the last, No. 5, in Taxon 48: 129-132. 1999]. Report No. 6 was sent to *Taxon* in May and was in the press [see *Taxon* 48: 811-814. 1999]. It recommended seven conservation proposals concerning two family names, two genus names and three species names. These proposals had been ratified by the General Committee at its meeting on the previous day; it also included opinions on several of proposals that had been discussed by the Nomenclature Section, including the two proposals submitted the day before from the floor on behalf of the Committee, one by himself, to add *-phycus* to the exceptions in Art. 62.2(c), and the other by Silva, concerning “iconotype”, which had been withdrawn. Among the names recommended for conservation by the Committee and just ratified by the General Committee were two names of important, widely cultivated microalgae, *Chlorella*, which was threatened by *Zoochlorella*, and *Scenedesmus quadricauda*.

The Report of the Committee for Algae was approved.

**Committee for Fungi**

Gams read the Report of the Committee for Fungi. The membership nominated in Yokohama included Uwe Braun, Halle, Germany; Brian Coppins, Edinburgh, U.K.; Vincent Demoulin, Liège, Belgium; Walter Gams, Baarn, Netherlands (Secretary); Paul Kirk, Egham, U.K.; Lennart Holm, Uppsala, Sweden; Per-Magnus Jørgensen, Bergen, Norway; Tom Kuyper, Wageningen, Netherlands; Erast Parmasto, Tartu, Estonia (Chairman); Guy Redeuilh, Maule, France; Scott Redhead, Ottawa, Canada; Gerry Samuels, Beltsville, U.S.A.; Harrie Sipman, Berlin, Germany; John Walker, Baulkham Hills, Australia; and Wen-Ying Zhuan, Beijing, China. At the end of 1998 Redhead had abruptly terminated his membership, and at the end of their present mandate, Braun, Coppins, Holm, Kuyper, and Walker had decided to step down. Parmasto had expressed his wish to give up chairmanship. The remaining members were prepared to continue. By a ballot dated 15 May 1999, the following six from among eight candidates were nominated for Committee membership: Leland Crane, Urbana IL, U.S.A.; Pier Luigi Nimis, Trieste, Italy; Lorelei Norvell, Portland OR, U.S.A.; Pavel Lízioň, Bratislava, Slovakia; Svengunnar Ryman, Uppsala, Sweden; and Trond Schumacher, Oslo, Norway. With these additions the Committee would again consist of 15 members. Each year, three or four mailings had been distributed, with comments and discussion. In the last three years, they had been sent by e-mail. Five reports on the Committee’s conclusions had been
published in *Taxon* in 1994, 1995, 1996 and 1998. The sixth, most recent one, No. 8, dealing with 8 proposals, had been submitted for publication a month before [see *Taxon* 48: 807-810. 1999], and its conclusions had been ratified on the previous day by the General Committee. Two of its items were of major importance, conservation of the family name *Helotiaceae*, proposed by Richard Korf and accepted unanimously, and of *Fusarium sambucinum* against *F. roseum*, which concerned many mycologists. Committee members had submitted five proposals to amend the *Code*, which had been discussed previously: one had been withdrawn, the others were approved with some modification.

The Report of the Committee for Fungi was approved.

**Committee for Bryophyta**

Zijlstra read the Report of the Committee for Bryophyta. During the past six years the Committee had considered 22 proposals to conserve a generic name, two of which were still under discussion. Of the 20 others, 15 were recommended for approval, albeit in some cases with major changes. In two of the five cases in which the Committee did not recommend a proposal, it was concluded that no conservation was necessary to maintain, a certain spelling in one case, a certain type in the other. In one case, the Committee expressed the opinion that two similar generic names were likely to be confused. Two proposals to conserve a species name and six proposals to reject a species name had been recommended; one case was still under discussion. No proposals on family names had been submitted. Since the last Congress, the Committee had published Report No. 4; No. 5 would appear in the August issue of *Taxon*, and the text of No. 6 had been made available to the General Committee. Both [see *Taxon* 48: 563-565, 815-816. 1999] had been approved by the General Committee on the previous day. The Committee had started, in 1993, with 12 members, mainly from Europe, the U.S.A., and Japan. The Australian member had resigned and had been replaced by another Australian bryologist. Recently, a European member had resigned.

The Report of the Committee for Bryophyta was approved.

**Committee for Fossil Plants**

J. Skog presented the Report of the Committee for Fossil Plants. The Committee had been quite active during the past year, commenting and voting on various proposals to amend the *Code*. A two-page handout with Committee relatively conservative opinions on relevant proposals had
been included in everyone’s folder. Another page, with the Committee’s advice on conservation proposals, had unfortunately not been included. [The full report had been submitted to and approved by the General Committee the day before; see Taxon 48: 817-819. 1999.] There had been only 3 conservation proposals on names of fossil plants, during past 6 years, to conserve the names Cyclopteris Brongn., Neuropteris (Brongn.) Sternb., and Rhaetogonyaulacaceae G. Norris, all of which had been recommended. The Committee had started with 15 members but within the last year had lost two, who were not replaced immediately. Several other members had indicated their desire not to continue on the Committee. All had now been replaced, so the Committee had again 15 members. The Report of the Committee for Fossil Plants was approved.

Committee for Pteridophyta

Zimmer read the Report of the Committee for Pteridophyta. The following 9 Committee members had been appointed by the Nomenclature Section of the Tokyo Congress, for the period 1993 to 1999: R. J. Chinnock from Australia, M. Kato (Japan), B. Øllgaard (Denmark), B. S. Parris (New Zealand), A. R. Smith (U.S.A.), M. Tindale (Australia), and R. Tryon (U.S.A.). During the Committee’s term of office, a single change in membership had taken place: Tryon had resigned in 1994 and was replaced by M. Palacios Rios (Mexico). Five circulars had been mailed, and one report had been published [in Taxon 48: 133-134. 1999], which had been approved by the General Committee. There had been only eight proposals falling into the Committee’s purview: 3 to conserve a name, 4 to reject a name, and 1 to authorise another Committee to have a pteridophyte name rejected against a later homonym. The Committee had voted on seven proposals, recommending six and considering one to be superfluous. Action on the eighth had been postponed, awaiting clarification, by the present Congress, of the provisions on lectotype designation. Thanks were due to all Committee members for their help, as successful Committee work depended on the skills and responsiveness of its members. The Report of the Committee for Pteridophyta was approved.

Committee for Spermatophyta

Brummitt read the Report of the Committee for Spermatophyta. Since the Tokyo Congress, the Committee had processed several hundred conservation or rejection proposals, plus some requests for a recommendation on homonymy. Several reports had been published in Taxon and
so passed to the General Committee for ratification. All proposals published up to the May 1999 *Taxon* were either already reported on, or had been voted on and were on their way to a formal report (c. 50 cases) [see *Taxon* 49: 261-278. 2000], or were under discussion and a vote would be taken shortly. As usual, a statistical breakdown of the Committee's recommendations since the last Congress would be submitted to *Taxon* [not published by mid-2000]. At this Congress the Committee's Chairman, former Secretary of the Subcommittee for Family Names and longest standing member, Günther Buchheim, had stood down after 30 years of service. John McNeill had been elevated to peerage by this meeting and had felt obliged to resign from the Committee. Thanks went to them both for their long service and valuable contributions. This left only two vacancies for new members, adding to the fact that at the Tokyo Congress, nobody at all resigned. As the Committee had felt the need for an infusion of new blood, it had requested to be increased from 12 to 15 members, so that at least 10 votes would henceforth be required to attain the necessary two-thirds majority [in fact, 9 votes: the Committee majority required being 60%]. The Nominating Committee had thus appointed five new members: two from N. America, Gerrit Davidse and K. N. Gandhi; one from S. America (for the first time in the Committee's history), Tarciso Filgueiras; one from S. Africa (the second African to serve on the Committee), Piet Vorster; and a second member from Australia (the second ever lady member, joining Egorova, also present), Gillian Perry. It was irrelevant but very nice that the Committee now had two lady members. It also had a new chairman, Ib Friis. The new Committee had met and resolved to speed up the processing of proposals. The delay between publication of a proposal and publication of the corresponding Committee report in *Taxon* was not as long as might seem: the result of voting was known long before appearance of the published report, and the authors were usually informed of the fate of their proposals as soon as the vote had been finalised, although occasionally he had failed to write such letters quickly enough. One must note that the Committee received frequent submissions from outside, commenting on proposals, and all such submissions were circulated to all Committee members, as part of the democratic process. The Committee would do its best to continue to process proposals as quickly as possible. Thanks were due to all for their continued support and always cordial, sometimes amusing correspondence, which he greatly enjoyed.

The Report from the Committee for Spermatophyta was approved.
Greuter [holding up a copy of the Tokyo Code] followed the tradition he had initiated in Yokohama by stating that this was the Report of the Editorial Committee: the “purple Code”, which had been approved as the basis of the Section’s debates at the beginning of the sessions. He took the opportunity to thank all who had contributed to making it happen: the Editorial Committee and all those from outside who had submitted suggestions, especially examples, or had assisted with the updating of the Appendices, mostly but not always as members of the Editorial Committee. He acknowledged Dick Brummitt in particular who, not a Committee member, had supplied valuable information and feedback on editorial changes to existing entries; Piers Trehane, a member, who had done a major job in completely restructuring the subject index; and, last but not least, Brigitte Zimmer, who had done all of the layout work and produced camera-ready copy, making it possible for the Code to appear promptly. He hoped to have her help once more in producing the Saint Louis Code, so that it could again, as had almost become a tradition, be published within one year after the Congress.

The Report of the Editorial Committee was approved.

Nicolson started his Report by mentioning the General Committee Members elected in 1993 at the Tokyo Congress. All had done their duties: the Chair, Werner Greuter, who had held a number of offices entitling him to Committee officership; the Vice-Chair, Ed Voss, being in charge of vice; all Permanent Committee Secretaries who had just delivered their own reports (Dick Brummitt for Spermatophyta; Brigitte Zimmer for Pteridophyta; Pierre Compère for the Algae; Gea Zijlstra for Bryophyta; Al Traverse, elected in Yokohama but later succeeded by Judy Skog, for Fossil Plants; and Walter Gams for the Fungi); as well as John McNeill, Secretary of the Editorial Committee. The General Committee had three officers, seven ex-officio members, and ten members-at-large (not counting Ana Anton from Argentina who did not respond to correspondence and had been considered as having resigned): Hervé Burdet, William Chaloner, David Hawksworth, Paul Hiepko, Pat Holmgren, Zennoske Iwatsuki, Juri Menitzki, Toni Orchard, Rodolfo Pichi-Sermolli, and Gideon Smith. These twenty members had survived their work load without complaint: six circulars, single-spaced and written in very small type. In future, electronic communication would have to be
used more extensively, there had been too much paper moving around. Three reports had been published, but not all business had been completed. He had therefore offered to stay on one more year as Secretary, to try to fix all loose ends.

Greuter added that, as had been mentioned in some of the previous Reports, the General Committee on the day before had met and ratified five not yet published Special Committee reports submitted to it as proof or in manuscript. They would be published in the August and November issues of *Taxon*. Ratification of these five reports was to be included in the General Committee Report, so as to permit the names in question to appear in the next *Code* as approved by the Section and by the Congress, not with the provisional status of "asterisked names".

The Report of the General Committee was approved, on the understanding that it included ratification of the five yet unpublished Permanent Committee reports.

**Other business**

Burdet, as the Section's last item of business, introduced a motion from the chair. It might seem a technicality, but without that last motion whatever the Section had decided and achieved would be vain. The text of the motion, also displayed on the screen, read: "The Section instructs the Rapporteur-général to present a resolution to the Resolutions Committee of the XVI International Botanical Congress, to the effect that the Congress should approve the decisions taken by the Nomenclature Section".

Burdet's motion was carried.

Burdet, speaking on behalf of the Section, thanked all those persons from Saint Louis who had assisted so effectively the Section's operations [applause]: Peter Hoch, the persons at the registration desk, not to forget A.J. Senior who took care of the tape recording. The Section had much appreciated the services of those who had variously assisted the Recorder by carrying around the microphones and collecting the votes, among them Patricia Taylor Hawksworth and Marilyn McNeill. [Applause.] It had appreciated the coffee and tea so generously dispensed. He expressed special thanks to the Rapporteurs, Werner Greuter, Rapporteur-général, and David Hawksworth, Vice-Rapporteur, to the Recorder, Fred Barrie, and his assistants, for all the work they had done. Theirs had been a splendid job, considering the bulk of the material that had to be brought under control. Dan Nicolson, the Nomenclature
Editor of *Taxon*, and the Editors of *Taxon*, Brigitte Zimmer and Werner Greuter, were to be thanked for preparing the material for publication. His personal thanks went to all 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. [Applause.] He was sure that all had understood that if at times he had been pushing a little, it had been for a good cause. The meeting had benefited greatly from the presence of senior members. Not all could be named, but two deserved a special mention: Rogers McVaugh and Knut Faegri. McVaugh had attended most Nomenclature Sessions since Paris in 1954, and Faegri, all Section Meetings since Stockholm in 1950. [Applause.] It was wonderful to have had them present; even more wonderful was their invaluable active contribution to the Section’s work. He hoped all here present would be in the same good shape at subsequent Congresses.

Barrie, too, had several people to acknowledge. The first was Neil Harriman, about to retire from the University of Wisconsin in Oshkosh, but showing a real aptitude for a new career as an overhead projectionist. [Laughter.] He also thanked Gordon McPherson and Barbara Kittrell for their help with the registration desk; Norbert Brown, A.J Senior’s assistant, who had helped keep the electronics working so well, and the graduate students who had assisted Marilyn and Patricia in moving the microphones, passing the ballot boxes and collecting votes: Deby Arifiani, Luzmila Arroyo, Jason Bradford, Cristina Casado, André Chanderbal, John Gaskin, Shing-Fan Huang, Antoni Jardin, Lucia Lohmann, Simon Malcolmber, Allison Miller, Zacharia Mogombo, Giancarlo Oliveira, Michelle Price, Thomas Prinzie, Jason Rauscher, Sylvain Razafimandimison, and Tony Westerhaus. [Applause.]

Kabuye had kept quiet throughout the meeting, waiting for an “other business” item. She was glad to learn that the new Secretary and Treasurer of IAPT intended to consider grants to support developing countries. She noted that there had been only two mail votes from Africa, indicating very low IAPT membership there. Several African botanists would have been interested in being present at the sessions, to learn more about nomenclature through discussion and debate, but they could not afford to come on their own. Being retired, she had had to use her own savings to assist, which had not been easy. Assistance to taxonomic institutions and interested individuals to attend such meetings would be most welcome. She had already raised at the Berlin Congress the matter of institutional membership in IAPT. Since *Taxon* was an
important publication for nomenclature, she would recommend subsi-
dising copies to many institutions in developing countries. [Applause.]

Chaloner wished to record, on behalf of the Section, his sincere thanks
to the Chairman. Burdet had ensured that all others were thanked. He
had been apologetic about his pushing through the Section’s business;
but this had been the very best bit of an excellent job, well done.
[Lengthy applause.]

Burdet declared the Nomenclature Sessions of the XVI International
Botanical Congress in Saint Louis 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.

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Appendix B
Institutional votes

A revised list of institutional votes, comprising 489 institutions totalling 857 votes, was drawn up by the Bureau of Nomenclature and approved by the General Committee, in accordance with Div. III of the Code, to replace the list approved for the XV International Botanical Congress (Englera 14: 259-265. 1994). Upon registration, the Bureau of Nomenclature granted one vote to each of 9 further institutions upon their written request presented by a member of their staff. These institutions were added to the approved list as given below, their entry being preceded by a double asterisk (**), so that the total is now 498 institutions and 866 votes. A simple asterisk (*) heads the entries of all other institutions that were represented at the nomenclature sessions in St Louis.

Institutions are identified by the name of the city in which each is located, followed by the standard herbarium code used in Index herbariorum (http://www.nybg.org/bsci/ih/), when extant, or else by the institution's name. The number of votes allotted to each institution is given in the right-hand column.

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Ashkhabad (ASH)  1
Asunción (FCQ)  1
Athens, Greece (ATHU)  2
Athens, USA (GA)  1
Auckland (AK)  1
Auckland (AKU)  1
Auckland (PDD)  1
Austin (TEX)  4
Baarn (CBS)  4
Baghdad (BAG)  1
Baku (BAK)  1
Bangi (UKMB)  1
Bangkok (BFK)  2
Barcelona (BC)  2
Barcelona (BCC)  1
Barcelona (BCF)  2
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| Bern (BERN)                  | 1 * Campinas (UEC)         |
| Birmingham (BIRM)            | 1 * Canberra (CANB)        |
| * Blacksburg (VPI)           | 1 * Canberra (CBG)         |
| * Bloomington (IND)          | 1 * Cape Town (BOL)        |
| * Bogor (BO)                 | 4 * Cape Town (NBG)        |
| Bogotá (COL)                 | 3 * Caracas (VEN)          |
| * Bologna (BOLO)             | 1 * Carbondale (SIU)       |
| Bombay (BLAT)                | 1 * Cardiff (NMW)          |
| Bonn (BONN)                  | 1 * Castelar (BAB)         |
| * Boulder (COLO)             | 1 * Catania (CAT)          |
| ** Brasilia (IBGE)           | 1 * Cayenne (CAY)          |
| Brasília (UB)                | 1 * Champaign (ILLS)       |
| * Bratislava (BRA)           | 1 * Changsha (HNU)         |
| * Bratislava (SAV)           | 1 * Chapel Hill (NCU)      |
| * Bratislava (SLO)           | 2 * Chengdu (CDBI)         |
| * Brno (BRNM)                | 1 * Chengdu (SZ)           |
| * Brno (BRNU)                | 1 * Chicago (F)            |
| * Brooklyn (BKL)             | 1 * Chongqing (HWA)        |
| Bruxelles (BRVU)             | 1 * Chongqing (SM)         |
| București (BUCA + BUCM)      | 2 * Christchurch (CANU)    |
| * Budapest (BP)              | 5 * Christchurch (CHR)     |
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** Mérida (CICY) 1  Norman (OKL) 1
* Mesa de Cavacas (PORT) 1  * Novosibirsk (NS) 1
* Mexico City (ENCB) 3
* Mexico City (FCME) 1  * Oslo (O) 5
* Mexico City (MEXU) 4
** Mexico City (UAMIZ) 1  Ottawa (CAN, CANA, CANL, CANM) 5
* Miami (FTG) 1
Milwaukee (MIL) 1
Minsk (MSK) 1
Mississippi State (IBE) 1
Missoula (MONTU) 1
* Montecillos (CHAPA) 1
Monterey (UNL) 1
Montevideo (MVM) 1
Montpellier (MPU) 2
Montreal (MT + MTJB) 2
Montreal (MTMG) 1
* Morehead City (IMS) 1
* Morgantown (WVA) 1
Moscow, Russia (MHA) 2
* Moscow, Russia (MW) 2
Moscow, USA (ID) 1
* München (M) 5
* München (MSB) 1
Murcia (MUB) 1
* Nairobi (EA) 2
Nairobi (NAI) 1
Nanjing (N) 1
Nanjing (NAS) 2
Nanjing (NF) 1
* Napoli (NAP) 1
Neuchâtel (NEU) 1
New Delhi (HClO) 1
1
Institutions

New Haven (YU) 1
New Orleans (NO) 1
* New York (NY) 7
* Nichinan (NICH) 2
Norman (OKL) 1
* Novosibirsk (NS) 1
* Oslo (O) 5
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### Nomenclature in Saint Louis

<p>| * Praha (PR + PRM) | 2 * San Jose (CR) | 1 |
| * Praha (PRC) | 3 Sandakan (SAN) | 1 |
| * Pretoria (PRE) | 4 Santa Barbara (SBBG) | 1 |
| Pretoria (PREM) | 2 Santiago (SGO) | 1 |
| * Pretoria (PRU) | 1 * Santo Domingo (INB) | 1 |
| Provo (BRY) | 1 * Santo Domingo (JBSD) | 1 |
| Pullman (WS) | 1 * Sao Leopoldo (PACA) | 1 |
| Pullman (WSP) | 1 * Sao Paulo (SP) | 4 |
| Pune (AHMA + AMH) | 1 * Sao Paulo (SPF) | 2 |
| Pune (BSI) | 1 Sapporo (SAP) | 1 |
| Quebec (QFA) | 1 Sarajevo (SARA) | 1 |
| * Quito (QCA) | 1 Saskatoon (SASK) | 1 |
| * Quito (QCNE) | 1 * Sassari (SS) | 1 |
| Rabat (RAB) | 1 Seattle (WTU) | 2 |
| Raleigh (NCSC) | 1 Sendai (TUS) | 1 |
| * Reading (RNG) | 2 Seoul (SNU) | 2 |
| Recife (URM) | 1 Sevilla (SEV) | 2 |
| * Regensburg (REG) | 2 Shanghai (HSNU) | 1 |
| Reykjavik (ICEL) | 1 Shanghai (SHM) | 1 |
| Riga (RIG) | 1 Shenyang (IFP) | 2 |
| * Rio de Janeiro (HB) | 1 Shillong (ASSAM) | 1 |
| * Rio de Janeiro (R) | 2 * Singapore (ASSAM) | 2 |
| * Rio de Janeiro (RB) | 5 * Sofia (SOM) | 3 |
| Riverside (UCR) | 1 ** Spring Arbor (S. A. College) | 1 |
| Rockville (ATCC) | 1 * Stellenbosch (STE) | 1 |
| * Roma (RO) | 1 Stevens Point (UWSP) | 1 |
| * Rydalmer (DAR) | 1 * Stockholm (S+SPA+SPL) | 6 |
| * S Miguel Tucumán (LIL) | 3 Stockholm (SBT) | 1 |
| * Saint Andrews (STA) | 1 Stockholm (SUNIV) | 2 |
| Saint Augustine (TRIN) | 1 * Storrs (CONN) | 1 |
| * Saint Louis (MO) | 7 * Stuttgart (STU) | 2 |
| * Saint Paul (MIN) | 2 Suva (SUVA) | 1 |
| Salaspils (LATV) | 1 Sverdlovsk (SVER) | 1 |
| Salt Lake City (UT) | 1 * Sydney (NSW) | 6 |
| * Saltillo (ANSM) | 1 Taipei (NTUF) | 1 |
| San Diego (SD) | 1 ** Taipei (ROC) | 1 |
| * San Francisco (CAS, DS) | 4 * Taipei (TAI) | 1 |
| San Francisco (SFSU) | 1 * Taipei (TAIF) | 1 |
| * San Isidro (SI) | 3 * Tallahassee (FSU) | 1 |</p>
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