Australian Systematic Botany Society NEWSLETTER

No. 67 JUNE 1991

Dodonaea heteromorpha West

Price: $5.00

Registered by Australia Post
Publication No. NBH 8068

ISSN 1034 -1218

AUSTRALIAN SYSTEMATIC BOTANY SOCIETY INCORPORATED

Office Bearers

President

Dr J.G. West
Australian National Herbarium
GPO Box 1600
CANBERRA ACT 2601
Tel (06) 246 5113
Fax (06) 246 5000

Vice President

Dr G.P. Guymer
Queensland Herbarium
Meiers Road
INDOOROOPILLY QLD 4068
Tel (07) 377 9320
Fax (07) 870 3276

Secretary

Dr B.J. Conn
National Herbarium of NSW
Mrs Macquaries Road
SYDNEY NSW 2000
Tel (02) 231 8131
Fax (02) 251 4403

Treasurer

Dr D.B. Foreman
National Herbarium of Victoria
Birdwood Avenue
SOUTH YARRA VIC 3141
Tel (03) 650 9424
Fax (03) 650 5917

Councillors

Dr J.A. Chappill
Department of Botany
University of Western Australia
NEDLANDS WA 6009
Tel (09) 380 2212
Fax (09) 380 1001

Dr J.M. Powell
National Herbarium of NSW
Mrs Macquaries Road
SYDNEY NSW 2000
Tel (02) 231 8139
Fax (02) 251 4403

Affiliated Society

Papua New Guinea Botanical Society

Australian Botanical Liaison Officer

Dr G.J. Leach
Royal Botanic Gardens Kew
Richmond, Surrey. TW9 3AB.
ENGLAND.
Tel 44-81-940-1171
Fax 44-81-948-1197

EDITORIAL

The last issue of the *Newsletter* made it out in March, but only just. Since that was our first attempt at being editors it was a pretty hectic process, as we had to learn everything as we went along. We got some useful background information on the process from Barbara Barnsley, but no hints on how to make it all work. We were, however, determined to get it done without ringing her and Mike Crisp in the middle of the night, pleading for guidance.

Unfortunately, a few glitches appeared in the finished product (particularly towards the end), which we're sure many of you noticed. Some of these appear to be due to human error, while others were caused (deliberately, we're convinced) by the computers. Some of the latter we still don't understand (such as why odd-numbered page numbers sometimes aren't printed out, and why some blocks of text sometimes don't print either), in spite of hours of sweating and swearing. Hopefully, these will be ironed out in the future. There's nothing we can do about the human errors.

Nevertheless, the production of the *Newsletter* is now such a high-tech process that maintaining both the quantity and the quality is not really a one-person job anymore. It's lucky there are two of us.

As many of you will realize, Barbara is usually more polite than David. Therefore, she is mainly responsible for liaising with the authors, book reviewers, council members, chapter conveners, and anyone else we can think of who should be contributing to the *Newsletter*. She also deals with the illustrators, book publishers, printers, and Australia Post. All of this keeps her pretty occupied.

David is mainly responsible for getting the various contributions onto the computer, and for doing the initial page layout. The text is entered (or read, if the author has been kind enough to send their contribution on an MS-DOS diskette) as an ASCII file using Wordperfect 4.2 on an Apricot PC (on permanent loan from a kind friend, and therefore ensconced in David's study at home).

After they've been checked, the ASCII files are transferred to a Macintosh diskette using the Copy II PC Deluxe Option Board in a Deltacom AT. The files are then made good using Microsoft Word 3.01 on a Macintosh Plus (and this is where text submitted on a Macintosh diskette enters the process).

The page layout is done using Ready,Set,Go! 4.0, also on the Mac Plus. Illustrations and logos are captured and edited using the Applescan system attached to another Mac Plus; and graphs are produced using Fig.p 5.0 on the Deltacom AT.

The final page layout and proof-reading is a joint effort by the both of us, so you can blame both of us for the finished product. All of the decisions regarding policy matters, the suitability of slanderous articles and incriminating photos, and the choice of filler text for the odd left-over spaces, are also joint decisions. We haven't had any disagreements over these matters yet.

The editors' job also involves putting each individual copy of the *Newsletter* into its envelope, sticking on the address label, bundling all of them into postcode groups for bulk mailing, and lugging the whole lot down to the post office. We weren't told about this in the original job description. So, this is done by the both of us, and anyone else silly enough to be standing in the vicinity on the appointed day. This process does, however, require people with a university degree.

Then, of course, we start all over again, the next day.

The concept that editorial work in the modern era takes more than one part-time person seems to be more and more common. For example, the editor of the *Bulletin of the Ecological Society of Australia*, Jill Landsberg, notes (in the editorial of issue 21:1) that while there is nominally only one *Bulletin* editor, it requires three other people to help do the job:- one to do the typing, one to help with the desk-top publishing, and one to liaise with the printer.

In this issue of the *Newsletter* we have tried to keep Dr Morrison's contributions under control. In the last issue, as well as contributing one of the longest articles ever published, he wrote most of the editorial as well as the description of his personal history, and he also made guest appearances in Barbara Wiecek's personal history, Louisa Murray's book review, Greg Leach's ABLO report, and the Sydney Chapter report. This time he only gets part of the editorial. Instead, Barry Conn and Paul Hattersley offered to write this issue.

David Morrison
Barbara Wiecek

ARTICLES

"In honorem Georgii Caley" — George Caley's collections and descriptions of orchids in the colony of New South Wales 1800-1810

Joan B. Webb
Faculty of Science
Kuring-gai Campus
University of Technology, Sydney

Peter R. Lister
Faculty of Horticulture
Hawkesbury Campus
University of Western Sydney

Abstract

In this paper, Caley's orchid descriptions are examined and compared with descriptions and diagnoses of botanical colleagues of his day and subsequent botanists working on Australian orchids. Using several orchid species, Caley's botanical accuracy and competency are demonstrated to be of a standard worthy of greater recognition than has been given him to date.

Introduction

George Caley, botanical collector in New South Wales from 1800-1810, did not receive the acclaim accorded to botanists such as Robert Brown and Alan Cunningham, although his collections included close to 2,000 species, taken from his collecting area of the County of Cumberland and the Blue Mountains. Today, his collection is housed mainly in the herbarium of The Natural History Museum, South Kensington, London, but many specimens may be found in Geneva, Vienna and Florence, with a few at Kew, Edinburgh, Sydney and Melbourne.

However, Caley's contribution, a potential never recognized, could have come from his detailed and accurate descriptions. Problems of finance, isolation and personality prevented him from publishing his work, even though his botanical expertise indicated that he was more than just a collector. This paper, on only one aspect of his extensive work, is presented as an indication that a valuable nineteenth century resource has been overlooked.

Caley the Botanist

Apart from his enduring efforts as a collector, Caley spent what must have been all of his remaining time recording details of his journeys and collections, describing each plant and those characters that he felt delineated closely related species.

Caley wrote both descriptions and diagnoses, not just observations or diary entries. His descriptions included habit, habitat, morphological variation and any anatomical differences [1].

During his ten-year stay in the colony, Caley collected hundreds of orchid specimens from many genera, including *Acianthus* R.Br., *Adenochilus* J.D. Hook., *Bulbophyllum* Thouars, *Caladenia* R.Br., *Caleana* R.Br., *Corybas* Salisb., *Cryptostylis* R.Br., *Dendrobium* Sw., *Pterostylis* R.Br., *Rimacola* Rupp, and *Thelymitra* J. & G. Forster, some of which were later designated as type specimens by Robert Brown in 1810 [2].

Brown arrived in the colony on the 10th May 1802, and by the 18th June was in the field botanizing with Caley. A little known fact is that Caley, Bauer and Brown spent considerable time botanizing together. Between Brown's arrival and his departure in May 1805, Caley and he were in the field together on at least four occasions:- in Sydney (June 1802), to the Cowpastures (October 1803), Parramatta - Prospect (September 1804) and between Port Jackson and the Hunter River (October 1804) [3].

These excursions enabled Caley to show Brown the best locations for new botanical material in areas that Caley new well. Undoubtedly, Caley benefited in learning of new taxonomic work from Brown, and also of how Brown might arrange the work he was preparing on the Australian flora. The time they spent together is reflected not only in Caley's beautifully written descriptions, but also in Brown's use of Caley's specimens and nomenclature.

In September 1803 Caley named his "Druid's

Cap uniflora" (now *Pterostylis nutans* R.Br.) with a description of a plant that he had first collected in April 1803 (see Webb and Lister, *A.S.B.S. Newsletter* **66**: 16-19). Brown latinized "Druid's Cap" to "Druocephalum" [4], and used this as a generic name for "Druocephalum acuminatum", "Druocephalum ophioglossum" & "Druocephalum parviflorum" until he created, and later published, the name *Pterostylis* for that genus.

It is difficult to say when Brown started using the name *Pterostylis*. It is probable that he spent time working on orchids while living on the 'Ocean' moored at Risdon Cove in Tasmania during February and March 1804 [5], as this was an ideal time to work on his collections and to write to his colleagues [6].

However, in June and November 1804 Caley used Brown's generic names *Pterostylis* and *Caladenia* on his field tags, indicating that Brown may have created these names prior to June 1804. Brown left for Tasmania in November 1803 [7] aboard the 'Lady Nelson'. If he had created these generic names before leaving, then Caley probably would have been using them also; but Caley was still labelling his specimens and describing them as "Druid's Caps" at this time. Brown did not return to Sydney until August 1804, and did not meet with Caley until September. Brown must therefore have written to Caley from Risdon Cove regarding these nomenclatural changes, for Caley to have known about them and used them prior to Brown's return.

Brown used some of Caley's (and Bauer's) collections for his work on the Australian flora. He thought highly of Caley as a botanist, honouring him in the naming of the orchid genus *Caleana* and also in the text of his *Prodromus*:- "in honorem Georgii Caley", describing him as "a skillful and accurate botanist" [8]. Despite this, Caley has received little botanical acknowledgement.

Closer examination of Caley's work demonstrates his botanical competency, and also the errors made by subsequent botanists from George Bentham (*Flora Australiensis, Vol. VI*, 1873) right through to Mark Clements (*Australian Orchid Research, Vol. 1*, 1989) in giving others, mainly Brown, credit for Caley's efforts.

We have chosen four well-known species of *Pterostylis* for detailed discussion, followed by further discussion of various other taxa of particular interest.

Note:- In reference to Caley's field tags, "S" represents Sydney and "P" represents Parramatta [9].

Pterostylis acuminata R. Br., Prodr. 326 (1810)

Caley's name: Druid's Cap Dryandri

Type: ex BM, is a Brown collection;
R.Brown, Iter Australiense, 1802-5.
No. 5523 Druocephalum acuminatum:
neighbourhood of Sydney
1805 March-May
in arenos [arenos = Latin "sand"]

also a field tag of Brown's;
Druocephalum acuminatum
1803 Sept
Port Jackson

Caley's field tags

Druid's Cap.S.May 1804
certainly Dryandri

Sydney May 1804
acuminata

S.May 1804
(?) Bulb 3
dryandri
acuminata

Druid's Cap like uniflora
N.rocks May 1804
dryandri Bulb 4 (?)

Discussion

Brown has clearly latinized Caley's name of "Druid's Cap" to "Druocephalum" and named this taxon "Druocephalum acuminatum" in September 1803.

Caley must have created the name "Druid's Cap", as Brown would not have used the vernacular in this manner. None of the specimens examined have "Druid's Cap" in Brown's handwriting. Furthermore, if Brown had created the name "Druocephalum", then Caley would have been using that name in preference to "Druid's Cap". Therefore, Brown had latinized a Caley name that he saw as a fitting title for such a plant.

Brown, in fact, used four generic names for *Pterostylis*. He used "Arethusa", "Arethusoides" and "Druocephalum" as manuscript names, and finally decided on *Pterostylis* as the most appropriate. For example, in September 1803, according to Caley [10], "Mr Brown informs me it is *Arethusa tetrapetala*" in reference to *P. nutans* R. Br.

Brown's type specimen of *Pterostylis ophioglossa* (then "Druocephalum ophioglossum") is also labelled by Brown as "Arethusoides tetrapetala v. pygmaea" [4].

Caley called *P. acuminata* "Druid's Cap Dryandri" in honour of Jonas Dryander, former pupil of Linnaeus (and librarian and curator to Banks), but also used the Brown name "acuminata" in May 1804.

Brown collected the type in "March - May 1805". His collection of September 1803 must have been vegetative (the specimen cannot be clearly identified with a field tag on the herbarium sheet), as this species flowers early in the year, usually March to June on the coast. Brown states that he collected at "Port Jackson". All of Caley's collections, as was his description, were made in May 1804, while Brown was in Tasmania.

Pterostylis nutans R. Br., Prodr. 327 (1810)

Caley's name: Druid's Cap Dicksoni

Type: according to Clements (1989) is;

"'Port Jackson; North Rocks', Jul. 1804, *R. Brown* s.n."

& a syntype he lists as "'Port Jackson, half way on the Sydney road from Parramatta', Jul. 1804, *R. Brown* s.n." [11]

also at BM is a Brown label viz;

8 Pterostylis nutans prodr. 1. p327
North Rocks Mr. G. Caley

on the reverse is written in Brown's hand;

Druocephalum cernum
P Jackson Oct.1803

Caley's field tags

Druid's Cap-Dicksoni
N-rocks July 1804

Dicksoni Half way on the Sydney road from Parramatta
July 1804

Druids Cap Dicksoni Half way on the Sydney road from Parramatta
July 1804

Dicksoni North rocks

P Sept 1803
~~*uniflora*~~ *Dicksoni*
N.Brush
June 1804

N.Brush
May 1805

Discussion

Clements lists the type and syntype as being collected by Brown. This is not possible, as Brown was in Tasmania at this time. However, the collection locations listed by Clements are direct quotes of Caley's field tags for his collections of July 1804.

Caley gives a description of this species as "Druid's Cap uniflora" dated "Sept. 1803", below which is the remark referred to earlier:- "Mr. Brown informs me it is *Arethusa tetrapetala*". Caley gives another two descriptions dated "June 22nd 1804" and "July 18th 1804", after which he states:- "This is the same as I called uniflora in a former description, and in which erase - nectary upper lip small, upright - and insert Tail curling upwards. The specimen that I sent you by that name, I am doubtful whether it being the same as from what the description was taken, as at that time I only knew one species, and it not unlikely, but what I may have confounded with it." [12]

Caley named this species in honour of James Dickson (1738-1822), cryptogamist, nurseryman and seedsman.

Pterostylis longifolia R. Br., Prodr. 327 (1810)

Caley's name: Druid's Cap Solandri

Type: is a Brown label, but a Caley collection;

14 Pterostylis longifolia prodr. 327
North Brush June 1804
Mr. G. Caley

[a single specimen designated as "(a)=Lectotype" by Clements]

another Brown label reads;

14. Pterostylis longifolia prodr.
1.p.327.
Port Jackson
1804 Mr. G. Caley

[this is designated as a syntype specimen]

Syntype: both Caley and Brown labels.

Only one of Caley's field tags quoted by Clements (1989) as "'Near Paldtor? farm', May 1805, *G. Caley* s.n."

Lectotype and syntype specimens designated as such by M.A. Clements on 11.4.83

Caley's field tags

Near Patello's farm
May 1805

~~*Near Patt.*~~
Near Patello's farm
May 1805

Pterostylis
longifolia
a very scarce plant

[the above six specimens labelled as "(b)&(c) Syntype Specimens" by Clements]

Druid's Cap Solandri
N Brush of June 1804

[this label accompanies the four specimens with the Brown label mentioned above and designated as a syntype. Also on this sheet is a Sieber specimen No. 160.]

Discussion

The type specimen was collected by Caley while Brown was in Tasmania (November 1803 - August 1804). In fact, Brown never saw a living plant of *P. longifolia*, as he states in his *Prodromus* [13].

Bentham [14] lists only Brown and Sieber as collectors of this species in N.S.W. However, all of the specimens collected from 1800-10 were collected by Caley, except one collected by Sieber (No. 160).

By correlating field tags, it is found that Caley and Bauer were collecting together in June 1804. This is supported by Brown's *Prodromus*, where (on page 327) he attributes the collection of *P. longifolia* to Caley and Bauer:- "D. Caley et Bauer v.s."

Caley's tags also refer to "Patello's farm". William Patullo had this property as early as 1796 [15], which was sited somewhere near present-day Epping.

One of Caley's specimens is labelled "Pterostylis longifolia a very scarce plant", and appears to have been collected in May 1805. This indicates that Brown had already established the genus; and according to a letter from Brown to Banks on 21st February 1805, this appears to be the case:- "I have finished the first eight classes, which, exclusive of Gramineae and Cyperoideae, contain upwards of 1000 species." [16]

Brown placed the Orchidaceae in Linnaeus' Class 1 "Monandria". These plants are those that exhibit "one stamen, or male part, in each flower, which also contains the female parts" [17]. Despite basing his *Prodromus* on Jussieu's arrangement, Brown divides his "Orchideae" into five subtaxa called "Sections I - V., Monandrae" [18].

Brown did not "arrange" his collection until after his return to England in November 1805. He spent the next four months examining his specimens, and two days of each week arranging specimens with Dryander in the Linnaean System [19]. In a letter to Banks dated the 6th August 1803, Brown states:- "In arranging the collection I at first follow'd Jussieu's Ord's Naturalis; but I soon found the plants of doubtful affinity so numerous that I judg'd it better to use the Linnaean method." [20]

Mature flowering plants of *P. longifolia* bear cauline leaves, whereas immature or vegetative plants bear rosetted (radical) foliage. These radical leaves usually develop as lateral growths following flowering. *P. obtusa* R. Br. and *P. grandiflora* R.Br. also exhibit this habit. Caley made reference to this in his description of June 10th 1804; but as Brown had not seen live plants, he made an error in his *Prodromus* when describing *P. longifolia* in stating "caule folioso foliis radicaulibus nullis" [13] [stem leafy, radical leaves absent]. Bentham, working from Brown's material, repeats this error in stating "without any radical rosette, the lower leaves reduced to short sheathing scales" [14].

Caley named this species in honour of Daniel Solander, a pupil of Linnaeus.

Pterostylis grandiflora R. Br., Prodr. 327 (1810)

Caley's name: Druid's Cap Banksii

Type: ex BM, is a Bauer collection;
R. Brown, Iter Australiense, 1802-5.
No. 5530
12 Pterostylis grandi-
flora prodr.327
Port Jackson
Mr Bauer

Syntypes: as designated by Clements (1989) are Caley collections for June 1804 and May 1805 as listed below.

Caley's field tags

Druid's Cap Banksii
North Brush P. June 1804

Banksii
N Brush
June
~~*April*~~ *1804*

Banksii
N.Brush
May 1805

N Brush Banksii
May 1805

Discussion

As mentioned above, Caley and Bauer were collecting together in June 1804. Brown, in his *Prodromus* and on his herbarium label, states that Bauer collected the type and that the collections of this species were made by "D. Bauer et Caley (v.s.)". Brown never saw this species, as he was in Tasmania until August 1804 and left the colony on the 24th May 1805 [3], before *P. grandiflora* flowers.

It should also be noted that when Brown refers to Port Jackson he is referring to "the neighbourhood of the colony of Port Jackson, including the banks of the estuary named Hunter's River or Coal River", not only that area in the vicinity of Sydney as we know it [21].

Brown's label attributing Bauer with the collection is undated, but the collection would have been made in June 1804, as not only does Caley's description bear this date, but a letter dated the 12th December 1804 from Brown to Banks states:- "Mr. Bauer did not accompany me to Van Diemen's Land, which, upon the whole, perhaps is not to be regretted, as he would, doubtless, find ample employment here, and during last winter was, I learn, uncommonly fortunate in the detection of new species of orchideae" [22].

This clearly refers to the collection being made in the winter of 1804. The only collections made in 1805 were in May and also in the "North Brush" (near Parramatta). It is also interesting to note that of the specimens collected between 1800 and 1810, none of those examined at BM have tags other than those of Caley's. There are no tags in either Brown's or Bauer's handwriting. A Caley specimen at Geneva (Herbarium Delessert) has a tag in Caley's hand, as "Pterostylis grandiflora a scarce plant" [undated].

Caley named this species in honour of Sir Joseph Banks, his employer and naturalist aboard Cook's first voyage.

Glossodia major R. Br. and
Glossodia minor R. Br., Prodr. 326 (1810)

Caley's names: Specala major and Specala minor

Type: for *G. major* according to Clements (1989) is;
"'Port Jackson', *R. Brown* s.n."

and for *G. minor* is;
"'Port Jackson', Sep.-Oct., 1803, *R. Brown* s.n."
the syntype for *G. minor* is;
"'Port Jackson', Sep., 1804, *R. Brown* s.n."

Discussion

On August 30th and 31st 1801, Caley wrote descriptions of two orchid species he had collected, naming them "Specala minor" and "Specala major" respectively. This was some eight months prior to Brown's arrival in the colony, and in the case of *G. minor* two and three years prior to Brown's collections of this species [12].

In this instance, Brown has used Caley's specific epithets yet created his own generic name of *Glossodia.*

Adenochilus nortonii Fitzg., Austr. Orch. 1, 2 (1876)

Caley's name: Breweria aestivalis

Discussion

Caley wrote a description of this species in November 1804 from 11 plants that he collected on his Mt Banks journey during that month. As this was subsequent to Brown's completion of his work on orchids (May/June 1804), Caley also had written a field tag using the Brown generic name of *Caladenia* as follows:-

Near Station rock
M.B.Journey
Nov 1804

Caladenia aestivalis
a very scarce plant
In the mountains

A Brown label reads;

Caladenia aestivalis R.B.
Near Station Rock Nov 18th Caley
Mount Banks Journey

Here again we see Brown's use of a Caley name. Unfortunately, Brown makes no mention of this taxon in his *Prodromus*.

The genus *Adenochilus* J.D.Hook. was not created until 1853, when Joseph Dalton Hooker published the name based on the New Zealand species *Adenochilus gracilis* J.D.Hook. This was nearly 50 years after Caley's collection and description. It was to be another 23 years before Robert David Fitzgerald published the name *Adenochilus nortonii*, based on material collected near Mt Victoria by J. Norton in November of that year [4].

So, Caley's detailed work on this species went unnoticed and unrecognized, not only for those 72 years, but until today. One J. Norton is immortalized in the naming of the plant in recognition of its "discovery", and Caley is virtually unknown.

Caley's Descriptions

In all of his descriptions examined by us, Caley maintained a format and content acceptable even today when describing a new taxon.

The acceptable procedure for such taxa is to provide a short, concise diagnosis in Latin, followed by an English equivalent. This will usually contain those specific characters that separate this taxon from closely related and established taxa. Following this, is a more detailed description of the plant, encompassing more general features such as growth habit, habitat, flowering or fruiting phenology, perfume, symbionts, pollinators, and collection details.

In the case of Brown's *Prodromus*, we have little more than pages and pages of Latin diagnoses. In some cases, the information provided lacks sufficient detail to be certain of the determination of the specimen concerned. George Bentham's *Flora Australiensis* is a great improvement, as he provides dimensions of floral segments and whole plants, collection details, observations on variation within a taxon and related taxa, as well as dichotomous keys. H.M.R. Rupp's *The Orchids of New South Wales*, for so long considered to be the major work on orchids within that state, is very similar to Bentham's format.

Caley's descriptions are clear, precise and detailed, and his format is consistent. This usually starts with the detailed description of the flower, in the order:- perianth, androecium, then gynoecium including fruit. Following this, he would describe the plant's gross morphology from tuberoids and roots, then leaves and stems to inflorescences, growth habit, habitat, and such minor details as the presence or absence of a scent.

On many occasions, he follows the main description with some short diagnostic comment on one or more characters that separate or demonstrate an affinity with another taxon. Caley lists this as a "Spec. char.", or makes a comment such as:- "and this last remark is this only one that gives a specifical distinction" [12].

The detail exhibited in Caley's descriptions demonstrates great observational skills, and a clear, systematic approach to his botanizing.

Prior to obtaining copies of Caley's specimens held in London, we examined some of Caley's descriptions in the Mitchell Library, Sydney. It was possible to identify species from his descriptions. Two examples are his descriptions of "Druid's Cap Baueri" (August 1804) and "Breweria vernalis" (August 1803), these being *Pterostylis bicolor* M.Clements & D.Jones and *Caladenia caerulea* R.Br. var. *caerulea*, respectively.

Caley's knowledge of various taxa and the "natural variation" within each species was apparently very broad; and over several years of examining, collecting and dissecting various taxa he could make comments as follows for his "Druid's Cap Baueri". On a field tag for September 24 1804 is written:-

This has not the edges
of the lip so regular as the
2 first specimens
P Sept. 1804 24th
Specimens 8

In 1987, Clements and Jones [11] separated this taxon from the closely related *P. mutica* R.Br.

Caley's Terminology

Although Caley's botanical terminology is not identical to that used by Brown, or that in use today, it was very consistent. Table 1 provides a comparison between Caley's terminology and present day terminology for equivalent floral parts.

As an example of Caley's observational and descriptive skills, Caley's description of "Druid's Cap Dryandri" is included here. Caley wrote the description from material that he collected in May 1804.

Table 1. Terminology used by George Caley for descriptions of orchids collected between 1800 and 1810, compared with current terminology

Caley's Terminology	Current Terminology
Cal.	Floral bract
Bloss.	Flower/Perianth
uppermost or upper petal	Dorsal sepal
lateral petals or inner petals	Petals
lower petal	Lateral sepals (fused)
Nectary or lower lip	Labellum
pedicle	Claw
tail or upper lip	Labellum appendage
Anthers or Stam.	Pollinia
2 cells	Clinandrium
anther appendage	Anther
Style or Pist.	Column
appendages	Column wings
protuberance or prominence	Stigmatic surface
Bulb	Root-stem tuberoid
Root leaves	Rosette or Radical leaves
Sheath	Bract, bracts of scape
Germen	Ovary
Reflected	Deflexed
Score	groove

Druid's Cap Dryandri [*Pterostylis acuminata*]
Cal. Sheath egg spear shaped, erect
Bloss. Petals 4, uppermost hunched at the base, vaulted, ending in a spearshaped point, with a prominent green line on the back part forming a keel, & with 4 small lines of the same color on the distended part; the sides & upper part entirely of a pale green but with a membranaceous white on the lower part between the green lines
Lateral petals crescent shaped, narrower towards the base, ending in a spear shaped point, nearly as long & frequently as long as the upper petal, with a large blunt tooth near the base on the upper edge, of a pale green color, but white in the middle of the lower half, but with 2 or 3 faint green lines, tooth white These lateral petals do not taper so gradual to a point as the upper one does - Lower petal of a pale green, but with faintish white lines Segments awl shaped, erect, the edges rolled inwards - Nectary on a pedicle, spear shaped, the point bent downwards, of a deep green color, with a prominent broadish line in the middle, but on the underside this line is a furrow, of a whitish green color on each side; tail green, rather tapering, bearded at the end with a few white thick like short hairs Anthers yellow Style green, united 1/4 of its length to the upper petal Appendages white, blunt at the lower end, sharp at the upper, slightly hairy. In the middle of the style on the forepart are 2 oblong prominent shining lines or protuberances - Root consisting of white thread like fibres Bulbs 2, roundish, varying in size, the younger one the smallest, of a whiter color Root leaves 5 in number in all that I counted, egg shaped, with 2 faint parallel lines on each side of the midrib, of a dullish green on the underside, when magnified showed minute transparent dots; underneath of a whitish color, but with similar dots as above, they end in a

sharp point, & are sometimes undulated. Stalk cylindrical, mostly with a sheath below the middle similar to the calyx, but sometimes it is wanting Sheath of the calyx generally a great way below the calyx Germen straight, smooth

May 1804

Conclusion

It appears that there are several reasons for Caley lacking the recognition that he deserves. A major contributing factor lies in the subsequent botanists, such as Bentham, overlooking his work. Even today, workers in this field continue this trend.

Bentham lists only ten orchid species being collected by Caley. He fails to mention *Pterostylis longifolia*, of which Caley collected the types. Brown attributes Caley with the collection of only four orchid taxa in his *Prodromus*, although he does honour him in the naming of the genus *Caleana*.

Brown certainly felt that Caley was a competent botanist, as did J.H. Maiden earlier this century. Caley's downfall lies in his failure to publish, believing he lacked the skills, qualifications, position and academic standing required for such a task. Furthermore, he lacked the money to produce a work on the Australian flora. [24]

Caley was complete in his approach, making collections that exhibited the level of variation within each taxon. His descriptions, although sometimes a little exhaustive, are systematic, detailed and accurate. He is certainly deserving of the title "Botanist", rather than being regarded as a mere collector.

Brown hoped that Caley would publish his work, speaking of him as:- "a most assiduous and accurate botanist, who, under the patronage of Sir Joseph Banks, has for upwards of eight years been engaged in examining the plants of New South Wales, and whose numerous discoveries will, it is hoped, be soon given to the public, either by himself, or in such a manner as to obtain for him that reputation among botanists to which he is well entitled." [23]

Acknowledgements

We gratefully acknowledge the assistance of Sylvia Gould (The Natural History Museum, London [BM]) and Dr Don Foreman (National Herbarium of Victoria [MEL]) for chasing and forwarding specimens and labels.

Notes

[1] Information from Caley descriptions held in the Mitchell Library, Sydney, manuscript MS FM4/2568, and also from small hard-covered notebook, BMNHB, London.
[2] Brown, R. (1810) *Prodromus Florae Novae Hollandiae et Insulae Van Diemen.*
[3] Mabberley, D.J. (1985) *Jupiter Botanicus. Robert Brown of the British Museum.* London.
[4] Information from Caley's field tags on herbarium specimens held in The Natural History Museum, London [BM].
[5] *Historical Records of New South Wales, Vol. V*, pp. 283, 343.
[6] *HRNSW, Vol. V*, p. 343, Brown to Paterson.
[7] *HRNSW, Vol. V*, p. 509, Brown to Banks (12.xii.1804).
[8] Brown, R. *Prodr.* p. 329.
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[10] Mitchell Library, MS FM4/2568 Plant List 1803-04, p. 91.
[11] Clements, M.A. (1989) *Australian Orchid Research, Vol. 1.*
[12] Mitchell Library, MS FM4/2568, Caley Journals 1802-07, 1804 list.
[13] Brown, R. *Prodr.* p. 327.
[14] Bentham, G. (1873) *Flora Australiensis, Vol. VI*, p. 364.
[15] Map by Bryan Thomas (1978), "Sydney 1796. The First Land Grants, Sydney to Parramatta", based on the Charles Grimes map, "The Plan of the Settlements of New South Wales".
[16] *HRNSW, Vol. V*, p. 558, Brown to Banks (21.ii.1805).
[17] Dickinson, A. (1967) *Carl Linnaeus 'Pioneer of Modern Botany'.*
[18] Brown, R. *Prodr.* pp. 312, 314, 320, 330.
[19] *HRNSW, Vol. VI*, p. 268.
[20] *HRNSW, Vol. V*, p. 182.
[21] Brown, R. *Prodr.* p. xxxiv.
[22] *HRNSW, Vol. V*, p. 510.
[23] Brown, R. (1867) *Miscellaneous Botanical Works, Vol. II*, p. 191. Ray Society.
[24] Webb, J.B. (1990) *George Caley, Nineteenth Century Naturalist.* Unpublished thesis.

If I could remember the names of all these particles I'd be a botanist. — Enrico Fermi

Stability of the scientific names of plants

A report on the meeting "Improving the Stability of Names: Needs and Options" held at the Royal Botanic Gardens Kew, 20-22 February 1991

Paul Hattersley
Flora of Australia
Australian Biological Resources Study

Introduction

"We must never forget that Systematists have two very different ends to meet:- 1. To provide a ready nomenclature without which the science cannot advance and which we change as little as possible – and further use every means to avoid even a necessary change – so important is it for all to get up the nomenclature [produce a synonymy], and so bulky and complicated is this literature. 2. To arrange the members of the Vegetable Kingdom scientifically, which is only done for the sake of scientific followers. Now we repeatedly find that to express our views scientifically we must break up the whole nomenclature, and rather than do this excessively, we confine ourselves to stating our views without acting on them." J.D. Hooker in a letter to C. Darwin in 1858 (in L. Huxley, *Life and Letters of Sir Joseph Dalton Hooker, Vol. 2* p. 454. New York: D. Appleton [1918]. Source: P.F. Stevens, in P. Baas *et al.* (eds) *The Plant Diversity of Malesia* pp. 387-410 [1990]).

I was lucky enough to attend the recent meeting "Improving the Stability of Names: Needs and Options" held at the Royal Botanic Gardens Kew, in February this year, and organized under the auspices of IUBS, in association with IAPT and the Systematics Association. To have stable names is, for the most part, incompatible with taxonomic revision using current naming procedures, and this has not changed since the time of Hooker (though the attitude to nomenclature may have!). The papers presented at Kew will be published as a number in the IAPT *Regnum Vegetabile*, perhaps as early as September this year, so I write this article in haste to serve as something of a 'private eye' preview, to air the issues, and to make comments and observations of my own.

Sessions at the Kew meeting were entitled "User needs for stability in names", "Zoological perspectives", "Approaches to stability in names", "Lists of names in current use", "Registration of names/publications", and sessions for offered papers and on general considerations. This article tries to cover these areas, also giving, I hope, a flavour of the debate at Kew on these issues.

Purpose of the meeting

Following the 1987 IBC in Berlin, the General Committee for Plant Nomenclature launched five Special Committees in May 1988 (Nicolson, *Taxon* **37:** 442-444 [1988]). One of these is the Special Committee for Registration of Plant Names, comprising Subcommittee A on the Registration of Names and Subcommittee B on the Registration of Publications – Roger Hnatiuk (Australian National Botanic Gardens) is a member of Subcommittee A and the Secretary of Subcommittee B. Later in 1988, at its XXIII General Assembly workshop in Canberra in October, IUBS invited the General Committee for Plant Nomenclature to appoint another Special Committee, on Lists of Names in Current Use, and this sixth Special Committee was established in November 1989. It comprises Subcommittee A, with a mandate "to prepare lists of names in current use", and Subcommittee B with a mandate "to consider proposals to be submitted to the Nomenclature Section at the Tokyo congress" (see Nicolson, *Taxon* **38:** 662-663 [1989]). Judy West (Australian National Herbarium) is a member of Subcommittee B. All of these subcommittees are to report to the XVth IBC in Tokyo in 1993.

The "Names" conference was organized as an open international forum in which to discuss these topics (regarded as controversial by many), to report on the activities of the subcommittees, to debate their proposals, and to provide feedback to them for "closed" subcommittee meetings held the day after the general meeting. The subcommittees were to develop firm proposals (published in *Taxon* later this year — August probably) to be submitted at the Tokyo Congress in 1993. There was an open invitation to interested persons to observe the "closed" meetings, so they were not secret!

The "Names" meeting, according to Frank Bisby (University of Southampton, U.K.) was really addressing one main issue, that of "getting rid of the problem" of the priority rules of the ICBN (Articles 11–12). This concerns *extant* names (those already published), and the major solution considered was the establishment of "lists of names in current use" (NCU lists). The second issue, that of registration of names and publications, attracted less attention and discussion than the NCU lists. Registration is concerned with *future* names (those not yet published), including their effective and valid publication. Note that, in the past, the term "registration" was being applied by some (e.g. Greuter, *Taxon* **35**: 816-819 [1986]) to prospective lists of both extant and future names, while by others (e.g. Hnatiuk & Chapman, *Taxon* **35**: 823 [1986]) it was intended for only future names from the outset, with an 'Index' proposed for extant names. Currently, its use seems to be confined to future names. An NCU list would, in effect, be a 'register' or 'index', but such terms may carry connotations of bureaucracy, censorship and cost, though none of these are intended.

The matters of whether and how NCU lists should be "protected" were not discussed in as detailed and structured a way as, in retrospect, they might have been, considering the intricacies of the proposals submitted by Werner Greuter (Museum Berlin-Dahlem, Germany). Greuter's proposals are sufficiently comprehensive to warrant reproduction here (Appendix 1). I do not know how much these proposals were amended at the "closed" meetings, but clearly they should be watched carefully. Greuter drafted these proposals for Subcommittee B. Note especially below, the reference to them made by Bill Anderson (University of Michigan Herbarium, U.S.A.), who provided during the meeting the most eloquent critique of these proposals. NCU lists, if protected, would effectively introduce new starting dates for nomenclature. In itself this seems a desirable goal to many. The question is how best to do it.

David Hawksworth (International Mycological Institute, U.K.) considers that we are at a "watershed with regard to nomenclature"; and a point emphasized by Bisby was that taxonomy is failing to provide an effective information retrieval system for other biologists, and for other people in general. Taxonomists "have not got their act together" (Bisby). A recurring theme over the 3 days was that if taxonomists do not provide a workable stable nomenclatural framework on which biological information can hang, and soon, then professional taxonomists will be, indeed to some extent already are being, circumvented. Clive Stace (University of Leicester, U.K.) (and see *Nature* **350**: 466 [1991) was therefore of the opinion that taxonomists should get straight to the point, and produce a world list of plant names that cannot be pre-dated — he said that perhaps his talk should be entitled "Haven't you all got something better to do?!" Anderson, on the other hand, believed that other scientists needed taxonomists more than taxonomists needed the patronage of other scientists — if users want to produce their own lists and codes, let them! They can then have the joy of rediscovering all the problems with language, homonymy, synonymy, splitting, etc.

What are the problems for users and taxonomists?

The problems of keeping up to date with name changes were outlined by most of the speakers of the first day. These included a horticulturalist, a seed technologist, a mycologist, a teacher, a palaeobotanist, a conservationist, and a geneticist; apparently there were no satisfied customers among them. Brandenburg (Centre for Variety Research & Seed Technology, Netherlands) pointed out that the hybrid nomenclature appendix of ICBN "is fortunately disregarded by most practicing agriculturalists and horticulturalists, because it is useless". Chauvet (Bureau des Resources Génétique, Paris) really needed names for genomes, not plants. Norman Hughes (Cambridge University, U.K.) claimed that nomenclature governed taxonomy in palaeobotany, and that "ICBN-driven taxonomy is entirely inadequate for fossils". Gunn (United States National Seed Herbarium; paper read by Dan Nicolson) emphasized that, as typification had not been very rigorous in the past, basing names of taxa on prior names was not always a good idea. Information scientists (Hine, University of York, U.K.) *have* to be aware of the problems of the users, but taxonomists do not. Taxonomy is the cornerstone of biological databases and vital to on-line retrieval services, said Hine, yet the "conversion of the sound principles of taxonomy into practice" for users, was "a big headache". In order to be responsible to users, and at the same time to maintain an adequate degree of stability in names, C.A.B. International and Biosis, for example, often make their own internal decisions about name changes in their databases.

The following day, Patterson (University of Bristol, U.K.) highlighted the problems in naming an estimated 120,000 protist taxa, with homonymy being the biggest problem. Definition of protists,

their taxonomic affinities with the rest of the living world, their classification and delimitation are all changing rapidly. Patterson called for a Protistan Code. At present either the ICBN or ICZN can be used, or both. He wants a non-partisan solution — a system that has "non-coherent" nomenclature.

Stace was sure that stability was essential. What do people want from taxonomists? — (1) good predictive classifications, (2) floristic works, and (3) stable nomenclature. The problem, he said, is "people"; in the 1950s, nomenclature was more stable — now "quite a lot of taxonomists like changing names". He was sure that many changes were ill-thought out, and were made without regard for users. Barnett (University of East Anglia, U.K.) cited a good example the next day:- 1984 was a "bad year" for *Hansenula* (a yeast genus), he proclaimed. An author published 30 species names that year, and that *same* year the *same* author also abolished the genus! Barnett also lamented the exclusion of living types for fungi; there is now no justification for this, as immutability was achievable with improved freezing techniques.

A range of requests/suggestions/demands were propounded by these users. They included:-

- Lists of NCU, with types
- Links between NCU lists and global species information systems
- A central body to settle disputes over names
- An uncoupling of nomenclature from taxonomic theory
- Uniform taxonomic databases, where name should not matter
- An "intelligent interface" in electronic data systems, for users who do not appreciate taxonomic complexities.

Taxonomists, too, said Greuter, want "to avoid the burden of checking types, synonymy, validity, etc.". David Ride (Australian National University) referred to the example of a book on family group names in birds, where 334 taxon names were eventually deemed "secure". About 2,000 separate publications (pre-1849) had to be consulted to search exhaustively for prior names and to assess validity, etc.; this took 4 years of what Ride referred to as "futile labour". The point was made by Fosberg (Smithsonian Institution, U.S.A.), however, that the present Code had evolved to do precisely what everyone was seeking — to achieve stability. Name changes due to the discovery (or "excavation", as Mabberley expresses it) of prior names will diminish. Thus, it is the Code that needs to be stabilized, not names.

The extent of name instability

Listening to unhappy "users" is not an objective way of measuring the magnitude of the problem. On the face of it, the problem would seem to be immense, as there are an estimated 1.5 million extant taxa of vascular plants, bryophytes, algae, fungi and fossils (see Hawksworth & Greuter, *Taxon* **38:** 142-148 [1989]). I was particularly struck, though, by the lack of quantitative data presented at the meeting by the proponents of NCU lists, on name changes made due to application of the priority rules. Fosberg submitted that less than 5% of the 200,000 spermatophyte species had their names changed for nomenclatural reasons since the introduction of the Code in 1867. Double that percentage had names changed for taxonomic reasons, leaving 85% with no name change at all. David Mabberley cited a revision of the Meliaceae, where no name changes were due to application of the priority rules. His view was that the argument should be turned on its head. Because there will be name changes until synthetic works are completed, the answer to the stability issue should be to promote monographic work. Stace questioned Mabberley's reference to revision of a tropical plant family, as the fact that few name changes are encountered there is due to the relatively meagre amount of taxonomic effort devoted to tropical groups; for the European flora (and I think here he was referring to the *Flora Europaea* project), 10% of species had their names changed for reasons of priority. Dick Brummitt (Royal Botanic Gardens Kew, U.K.) made the surprising point that less than 0.005% of generic names in the *Index Nominum Genericorum* changed per annum for nomenclatural reasons.

I was therefore left in doubt as to just how much improved stability could be anticipated by providing NCU lists, certainly for families and genera. Here we are talking about less than 800 family and about 14,000 generic names for vascular plants, and a total 'plant' generic list of about 55,000 names (vascular plants, bryophytes, algae and fungi; but excluding fossils). Name changes will occur anyway — for taxonomic reasons. The extent of name change as a result of taxonomic revision is, by contrast, apparently phenomenal. Lock (Kew Index, Royal Botanic Gardens Kew) reported that of 5,500 names listed in the 1989 *Index*, 30% were new combinations. I wonder, therefore, whether this relegates the problem of instability caused by application of the priority rules to insignificance. No-one is suggesting for a moment, of course, that taxonomic revision ceases — and taxonomists seem to agree on that! But it

was pointed out on a few occasions that the binomial is inherently unstable; and the suggestion of replacing it with a biocode was resurrected (David Heppell, National Museums of Scotland), as was introduction of some sort of parallel 'tagging' system. One of the proponents of the latter (de Smet, Belgium) suggested adoption of Esperanto for this purpose!

I daresay notions of biocodes and/or parallel naming are unpopular with some, but it seems to me that if one is serious about stabilizing the names ("tags") of taxa, then these options need to be considered — the binomial needs to be replaced or used in parallel. There were frequently calls for "uncoupling nomenclature from taxonomic revision" (Bisby, Heppell, James, Barnett). How this could be done was not the main theme of the meeting, nor did it emerge there as a more pressing issue — but I think it is. If it can be done, name stability becomes a non-issue. Of course there will always be additional names/tags needed when taxa are split or newly described.

Solutions to the "prior names problem"

Solutions to the 'prior names problem' that were submitted over the three days can be summarized as follows:-

1. Do nothing to the Code, i.e. stabilize the ICBN and get on with definitive revisions.

2. Extend the application of the existing conservation and rejection rules, e.g. remove the restriction of the 'conservation of specific names' rule to species of major economic importance. It seems to me that to conserve a binomial is not a good idea; the binomial is a name that clearly indicates taxonomic position, as it is in part composed of the genus name. Surely the conservation of a binomial restricts taxonomic revision, unless a *de facto* parallel naming system is to evolve, and one which taxonomists had not planned.

3. Get rid of, or replace the binomial, e.g. by introducing parallel naming/tagging or biocodes. Note that biocodes solve the problem of instability due to resurrection of prior 'tags' only if part of the biocode is immutable and unique. This part would have to be typified, as with any other name or tag. There would not, though, be any scientific/taxonomic information in this stable part of the biocode, and so disagreement in its assignment to a taxon when constructing a biocode would never be contentious. It could be a randomly assigned number, for example. The remainder (most) of the biocode would include a checker and other identifiers, some of which would indicate taxonomic position and so probably be unstable because of taxonomic revision. Another casual suggestion was to turn the binomial around, i.e. the first name would indicate the species, and the second the genus (as epithets in current binomials are adjectives, a simple switching of words in current names would be out of the question and new names would have to be coined).

4. This leaves the Lists of Names in Current Use solution, which was the focus of the meeting. Such lists could be non-protected or protected. It is the issue of protecting such lists that is contentious. Several people (Anderson, Hine, Mabberley) suggested that straightforward NCU lists may achieve sufficient stability, without the need for protecting the names on them.

Lists of NCU are being produced for vascular plant families (Reveal & Hoogland, University of Maryland, U.S.A., and Museum National d'Histoire Naturelle, Paris, respectively) and for plant genera (see Hawksworth & Greuter, *Taxon* **38:** 142-148 [1989]). As far as I know, the list of generic names that is furthest advanced is that for vascular plants, being organized by Brummitt. This is a list based on a subset of the names in the IAPT "Index Nominum Genericorum" (ING) database at the Smithsonian Institution.

As emphasized by McNeill (Royal Ontario Museum, Canada), Subcommittee B of the Special Committee for Lists of NCU clearly intends to propose that such lists be given protected status. In Appendix 1, I give the full text of the proposals from this subcommittee (drafted by Greuter), as circulated at the meeting. I do not know if (or how) these proposals were modified in the "closed" meetings on the Saturday following the general sessions. "Protection" is defined in Article 15bis.2 (Appendix 1); it is not the same as sanctioning or conservation. Note especially that Subcommittee B's proposals are to be phased in. Amendments to the ICBN would occur in four steps. The first step relates to families and genera only. Step II introduces application of NCU "rules" to the species level. This is not clear until you read the proposals right through, so I emphasize it.

Difficulties with lists of NCU

Both Greuter and McNeill stressed that the danger of introducing NCU lists was not that they would threaten taxonomic progress — they deal with extant names, and not the names of taxa published in future revisions. The danger is that their quality may not be good enough. Greuter thought

that the quality was in doubt. This quality would depend upon adequate feedback from the broad taxonomic community once the lists were circulated for comment. There would be no dictatorship, alternative taxonomies would be accommodated in the lists, and they would be distributed free. He urged taxonomists to make sure that they perused the lists if they appeared, and to provide feedback because "you may have to live with it". The main function of the lists is to protect names against earlier homonymy and synonymy.

Anderson questioned the myth that the needs of users demand change to the ICBN, and that taxonomists should respond to threats that users will produce their own lists. He said that there was no evidence that taxonomists will lose their funding if they do not appease their clients. He believes that lists of NCU will not, in the long run, impress clients. D. Jones (microbiologist, University of Leicester, U.K.) considered Anderson too fearful. She exhorted botanists to "Be ruthless — don't be democratic. Have the lists." She has had "much more respect for nomenclature" since the International Code for the Nomenclature of Bacteria (ICNB) introduced such lists.

Brummitt is a proponent of the lists, believing that their value will mainly be in the immediacy of the protection they will give to names (cf. slow conservation), yet his paper expressed grave concerns about how to compile them. As Anderson noted, the lists are being put together without any agreed guidelines. Anderson was evidently so impressed with Brummitt's exposition of these concerns, that he had the following to say (n.b. Brummitt's paper immediately followed Greuter's): "My dad said: 'Never buy a pig in a poke'. Greuter in his talk nearly sold us a pig in a poke. You [Brummitt] have opened the poke so that we can see the pig — I don't like the pig! ... The lists will be quick and dirty." Even Stace, who had earlier implored everyone to set about drawing up world lists, was obviously concerned about procedures adopted in selecting the publications from which names would be compiled. The reassurance that alternative taxonomies would be incorporated did not seem to assuage such concerns. Are we aware of the alternative taxonomies? Are they all to be accommodated? Who decides? Would we not end up with almost as many names as we have at the moment? Who decides to whom the lists are sent for review?!

Brummitt himself posed the question: "Will NCU lists for genera reduce name change problems or increase them?" If lists are wrongly compiled, perhaps the latter will be the case. He stated that he had inherited many errors from ING. Guidelines are needed as to what sources to accept. Which Floras ..., which monographs ..., which minor publications should be screened? Should the lists be compiled mechanically (= uncritically) or should they be taxonomically judged. He favours taxonomically-judged and refereed lists, and uniform format and editorial and publication procedures for lists at any rank. He is opposed to regular supplements, favouring a major update every 30 years or so.

Fosberg had complained earlier that no really significant reference had been given in Greuter's talk to typification. If names are not typified, how do we know what we are talking about? Farr (Index Nominum Genericorum, Smithsonian Institution) highlighted the difficulties of including type information in lists of plant names, pointing out that ING "is about nomenclature, not taxonomy". ING is compiled rather like the "Australian Plant Name Index"; there are bibliographic checks only. ING contains many lectotypifications made under the old American Code. Many names were erected using the "residue method". She said that it has not been possible for ING to update these older lectotypifications. In addition, with the recent changes in the Berlin Code, the two editors at ING simply do not have the time to search for lectotypes. There are still some ING names which are not typified. Two years had not been enough time to get the lectotypifications done, so the lists of NCU should go to reviewers to comment on types, and then four "type categories" could perhaps be given in the lists:-

(i) Names typified and checked fully
(ii) Names typified, but not fully checked; agreed by reviewers
(iii) Names with provisional types listed, i.e. doubtful
(iv) Names not yet typified

In discussion, Brummitt did not think types needed to be cited — "putting them in asks for trouble". After all, the information on typification status is in the original sources. He has not checked the typification for the 14,000 vascular plant generic names on his list. Greuter, on the other hand, wants names, author, orthography and, hopefully, type given in the lists.

Subcommittee B's proposals on NCU lists

Anderson had the following to say about Greuter's proposals (Appendix 1):-

• Taxonomists should not write blank cheques,

without knowing the costs.
• Nomenclature must not become the tail wagging the dog.
• With reference to 15bis.4, he says "Read it! — especially the bit 'only by a process analogous to conservation'. This is an absurdity! If adopted, it will represent a schism between vital taxonomy and sclerotic nomenclature! ... To begin with, any draft proposals have to deal explicitly with the inclusion of new names, and not handle them in this manner [ref. to 15bis.4]."
• "Dumping" all old names not in current use means that many previously described, real, recognizable taxa that have not been found or recognized subsequently, will probably be lost to science. If they are rediscovered, do we describe them all from scratch?
• When taxa are split, Anderson predicts that havoc will be caused under such an NCU system. (I think the major concern here is the time lag between the publication of the new taxonomy and the appearance on the lists of new generic and specific names, new combinations, etc.; this could be 30 years! This would have a stultifying effect on the usage of many (most?) of these names, whatever their taxonomic worth, by general "users". How would a user distinguish between the absence of a name from the list for this reason, and its absence due to purposeful exclusion by taxonomists, without searching the whole literature?)

Anderson strongly suggests deleting 15bis.2 (b,c,d) and rewriting 15bis.4. He stated that there is "a distinct small clique of taxonomists who opt for nomenclature rather than taxonomy. Most taxonomists are not nomenclaturists. They resent new rules. Taxonomists themselves could opt to ignore the new Code."

Precedents

The International Code of Nomenclature of Bacteria (ICNB) was often cited as a precedent when advocating the use of stabilized lists of names. Ride outlined the policy decisions of bacteriologists for constructing their present Code. They include:-

(i) preserve names in current use by listing them;
(ii) ensure application of listed names by appropriate typification;
(iii) exclude from nomenclature names not listed;
(iv) establish a new starting date for case-by-case treatment of new names;
(v) require new standards of description, diagnosis and typification;
(vi) ensure application of standards by confining valid publication to a single journal; and
(vii) maintain freedom to publish taxonomic conjecture and refutation.

The introduction of living types played a large part in the success of the ICNB. Ride compared the ICNB and ICZN, their operational differences, and their level of success in promoting stability. The bacteriologists do not seem to be having problems, whereas the zoologists do. He advocated registration of names, abandonment of authorship ("It is not justice for the dead zoologist that we need, so much as justice for the living."), progressive introduction of lists of NCU, and protection of names on the list — "Be relaxed!"

It was pointed out that the bacteriologists have far fewer taxa to deal with (2,300 names were on the stabilized list when it was introduced) than flowering plants, for example. Also, the literature was not nearly as extensive, and the nature of the organisms themselves dictated a different approach.

In addition to the ICNB, Greuter cited the following ICBN examples as precedents for introducing protection of names:- the sanctioned names of fungi; conservation of family and generic names, and even species names (two to date). The zoologists may move in that direction. too.

De facto lists of stable names

User lists that were cited as already in existence, and outside the *aegis* of ICBN, ICZN, ICNCP (International Code of Nomenclature for Cultivated Plants) and ICNB, included:- a list introduced by the International Seed Testing Association (ISTA) in 1966 (Gunn and Brandenburg testified to its success); a list of stabilized fossil names drawn up by petroleum companies (cited by Hughes); and a list of fossil names compiled by Norman Hughes himself. Hughes stated that he had effectively withdrawn from ICZN and ICBN. He was asked after his paper, how he dealt with competitors, to which he replied that he did not have any. Bill Chaloner (University of London, U.K.) said he could not at all agree with Hughes' approach to fossil names, and that Hughes had advertised only the bad news concerning the naming of fossils (e.g. the fragmentary nature of fossils, with a compounding, therefore, of the 'normal' problems of synonymy and priority) — "What about the good news?! Fossils have no commercial varieties, and no genomes!" (He may be wrong on the latter point, as DNA is being recovered from certain sorts of fossils, such as leaf compressions.) In any event, Chaloner's answer to the question of which names should be

Boulter (IOP, N.E. London Polytechnic, U.K.), Chaloner, and Holmes (Polytechnic of E. London, U.K.) have nearly finished the International Organization of Paleobotany's (IOP) automated "Fossil Plant Record" database (to sell for £100), which includes all names.

Registration of names and publications

I have referred already to the fact that "registration" seems now concerned only with new plant names, i.e. those names published after a certain date to be designated. Neither Subcommittee A (Names) nor Subcommittee B (Publications) had been very active. Subcommittee A's Secretary did not address the conference, and Faegri (Botanisk Institut, Norway) stated that the subcommittee had not performed its task.

Faegri reported that a machinery for publishing names would not be proposed, as there "were uncertainties with regard to which are going to be the formal demands". He distributed commented proposals on effective publication. I have a copy of these and can send them to anyone interested, but I imagine that some of them will be/have been altered in view of the Proposal to Amend the Code agreed at a joint meeting of members of both subcommittees of the Registration Special Committee at the "closed" meeting on 23rd February (reproduced here in Appendix 2).

There was brief debate during the general meeting on registration of publications. This is an issue which has received few objections, the aim being to restrict publication of new names to a limited number of journals so as to make it easy to search for such names. No consensus emerged on the selection or number of journals which should be registered. In any event, Roger Hnatiuk informed the ASBS Canberra chapter in April that the subsequent "closed" meeting of the Special Committee on Registration abandoned registration of publications. It is proposed instead that one should still be able to publish anywhere (as now), but that the names should be registered, with one authority at several locations. To be valid, a name would have to be registered as well as published. The date of registration, not publication, would be the date used for priority purposes. The registry would require the name and protologue to be submitted to it (see Appendix 2). There would be a 6-year post-registration period during which it would be possible for other taxonomists to register any prior names that might exist (or, if prior names exist, to have the registered name removed from the register?); after 6 years, new names on the register would (automatically?) be placed on the protected NCU list. (This would overcome to some extent, the concern Anderson expressed about the "schism between vital taxonomy and sclerotic nomenclature".) The ISTA stabilized names list referred to above also has a 6-year waiting period. Here though, as I understand it, stabilization is guaranteed for 6 years, when it is then reviewed. (This is unlike conservation, which is indefinite.)

Registration of future names in itself has the advantage that all names being published would be available from one source, thus circumventing the possibility that, in the future, names will not have been "discovered" because they reside in some obscure publication. Taxonomists would need to search only the register for post-1995 literature. The registry would merely play an administrative role, checking that all registration requirements have been met. All names and protologues properly submitted would be registered. Note that such a registry could still operate and confer the advantage specified above, without necessarily linking in to NCU lists.

Discussion and summary

Some concluding remarks

The meeting facilitated an open exchange of a multiplicity of views on how name stability might be improved. I was left with certain, perhaps mistaken, impressions:-

1. There is an overall will to want to improve name stability.
2. There is no consensus on how this can best be done, e.g. even among proponents of NCU lists, there is debate as to whether types should be listed and protected.
3. There is no clear appreciation of the impact of proposed Code amendments on name stability.

It seems necessary for experienced taxonomists to take the time to examine proposed amendments carefully, and to assess their impact on name stability. Will they improve it or not? This appraisal should not set out to prevent establishment of NCU lists or registration, but should make sure that these are effected properly if lists are to be established, or should objectively document and demonstrate their shortcomings. If this is not done, and Code changes are made and do not have their designed effect, perhaps more chaos will have been created than prevented. On the other hand, to do nothing will not improve the situation.

will not improve the situation.

Names have to change as taxonomic revisions occur. Name change is inevitable as taxonomic affinities are reassessed and as new taxa are described (including when existing taxa are split). With a hierarchical system of classification, some change is also inevitable when circumscription of taxa above the lowest hierarchical levels under revision is altered and when rank is changed; molecular systematics and cladistic analyses may result in major reassessments of relationships of supraspecific taxa leading to much name change. Such name changes will never stop, but they may slow down as plant groups become better known. Here, taxonomists already have some control over stability, e.g. taxonomists could redirect efforts from fine-tuning rank in well-worked groups (such as species vs. subspecies) to groups that have yet to have modern revisions.

The priority rules were invoked as a method for choosing automatically between competing synonyms. Its flaw is that all prior names for a given taxa are often not known, or not even known to be known. The NCU concept is a realistic way of setting a new starting date for names, but good NCU lists will be difficult and laborious to construct (especially for species), and alterations to the Code need to be very carefully made, as always. Are there not simpler ways? For example:- institute a system of voluntary rejection of names, i.e. when a prior name (say "a") is judged to have been found for a taxon (currently named "b"), the name "a" could be listed in an ICBN appendix as a voluntarily rejected name, together with the currently used synonym "b". Only names published before January 1890 could be listed (i.e. no names could be listed that were published by persons still living). After names had been on the list for a number years (say 10 or 20 years) they could be transferred to a compulsorily- rejected list of names unless a prior appeal to prevent this pending action was successfully lodged with the nomenclatural committees. Such a system does not require complicated rewriting of any part of the Code, does not exclude names from science that have not been rediscovered, assessed and listed, and does not conserve or protect names (especially binomials or trinomials).

To avoid or help prevent the publication of nomenclatural synonyms resulting from future taxonomic revision, some form of registration of new names seems desirable. Such a register would at least have some stabilizing effect, whether or not names on it automatically became protected at some stage.

Biocodes and parallel naming

"Must a name mean something?" Alice asked doubtfully. "Of course it must," Humpty Dumpty said with a short laugh: "my name means the shape I am — and a good handsome shape it is, too. With a name like yours, you might be any shape, almost." (*Through the Looking Glass*, Lewis Carroll [1871])

"What's in a name? That which we call a rose by any other name would smell as sweet." (*Romeo and Juliet*, William Shakespeare [c. 1594-1599])

The ICBN is already complex, having had a long evolutionary history. As with biological evolution, what it can change into is limited by what it already is. A major feature of the ICBN that limits prospects for improving stability is that some names are binomials or trinomials, which are the names given to the taxa most frequently cited by biologists in general. Here, species and infraspecific taxa can have their names altered when revisions are made at genus level. This could be avoided by introducing an immutable and unique parallel tag to the species rank. This could be a numeric tag. If the upper limit of extant total 'plant' species is two million, the tag could be a seven-digit number, or comprise only five letters. Thus bread wheat could be assigned this stable tag in parenthesis, and be named *Triticum aestivum* L. (1 234 567) or *Triticum aestivum* L. ("wheat" or, using random letters, perhaps "eovkl"). "Wheat" or "eovkl" would not then be available for any other taxon. Both parallel tags would have the same type. If *T. aestivum* was transferred to *Aegilops* by Bloggs, it would be known as *Aegilops aestivum* (L.)Bloggs (wheat). The rule would be that all name-users and taxonomists must always use the name "wheat" when referring to this organism, but taxonomists would still rename plants in the regular way by changing the binomial. A parallel scientific *naming* system could be introduced, and for all ranks not just species. NCU lists could be established, with names on them being typified. Taxonomists could use the *first* cited name in such duonames in the same way that names are now used, without alteration to the Code. Thus, *Triticum aestivum* L. (*T. aestivum* L.) would become *Aegilops aestivum* (L.)Bloggs (*Triticum aestivum* L.).

Another possibility is to establish biocodes, which only taxonomists use, to be cited in parenthesis after an immutable, unique NCU list name, e.g. *Triticum aestivum* L. (1.65.072.145, etc. — fictional) where the biocode would comprise a

unique stable typified tag (an added bonus for information scientists), identifiers indicating taxonomic rank and position, and a checker. Heppell has recently discussed biocodes in relation to registration (Heppell, Biocodes and registration of names, in D.A. Roberts (ed.) *Terminology for Museums* pp. 456-463. Cambridge: Museum Document Association [1990]). Biocodes are presumably preferred by information scientists, for ease of data entry and bar coding (cf. ISBN bar codes). Type specimens could be additionally labelled with the unique stable part of their biocode.

Two-Tier Taxonomy?

Ride emphasized the importance of "sociological" factors forcing change in nomenclatural practice. Advances in information technology have created a demand for an easily accessible "authoritative product" from taxonomists. A current example in Australia is the demand for a taxon schema module to function as a cornerstone for the networking of biological resource information (the "Environmental Resource Information Network" [ERIN], part of the new Australian Biological and Environmental Survey, now in ANPWS). The databases of these networks have plant taxon names that have been, are, and will be unstable. Clearly the quality of the products produced using the databases will deteriorate with time if names are too unstable to be accurately monitored and interpreted.

Bisby also alluded to the significance of taxonomy to biological information systems. He believes taxonomists themselves should be providing these information systems and he referred to the "International Legume Database Information System" (ILDIS) as an example, with its up front TAXAPOINT (a 'nomenclatural flexiteller'). The need to form such taxonomic databases is increasingly acknowledged. Mike Dallwitz's (C.S.I.R.O.) DELTA format is the Taxonomic Database Working Group (TDWG) data interchange standard for such systems. Here in Australia, Les Watson (Australian National University) has developed a "Grass Genera of the World" information system (among others), which is an 'authoritative product' of the sort referred to by Ride.

Because all biological data can have taxonomic usefulness (and for this reason taxonomists need to be broadly-trained biologists), it is logical to argue that taxonomists should be involved in developing all-embracing taxon information systems (e.g. Watson, Gibbs Russell & Dallwitz, *South African Journal of Botany* **55:** 452-463 [1989]). Who else will do it? But taxonomists usually still identify their roles as those of (1) the discovery and morphological description of diversity, (2) classification, and (3) the cataloguing of diversity in Floras (*vide* Stace's remarks). It is the latter that needs to be expanded, so as to create, for example, a species diversity information system (to grow out of the "Species Plantarum" project).

In the last paper of the meeting ("Why something must be done to save systematics"), Bisby stressed that there is a need to uncouple "the flux of debate among taxonomists about taxonomy" from the "considered production of diversity information systems". A two-tier taxonomy is suggested. By "flux of debate among systematists" Bisby means the "essence of scientific systematics, the constant revisions and changes, technical publication in taxonomic literature, and what taxonomists are paid for", i.e. most current taxonomic practice. The second tier, "the considered production of a species diversity information system", should be produced "by taxonomists for non-taxonomists, with user-driven high quality services, with stabilized nomenclature, and with periodically standardized taxonomy".

Even if this challenge is not shouldered in the final analysis primarily by taxonomists, the expertise of taxonomists is needed for it, not least for devising a system of stabilized names and/or interpreting and explaining name changes.

Appendix 1

Proposals to Amend the Code
(as circulated by the NCU Subcommittee B at Kew, February 1991)

The draft proposals are subdivided into four "steps" corresponding to successively wider applications of the NCU concept. The first step is again subdivided in three:- the basic provisions, the aspects related to sanctioning (to be dealt with primarily by the Committee for Fungi and Lichens), and editorial matters.

Ia: Draft of Code changes related to NCU. Basic proposals enabling protection of names of families and genera

New Art.15bis
(to become Art.15 if the present Art.15 is transferred to Art.14)

15bis.1. In order to stabilize the application of names in current use, and to prevent their being

threatened or displaced by names that are no longer in use, published lists of names of families and genera can, upon recommendation by the General Committee, be approved by an International Botanical Congress. Such lists, once approved, are enumerated in Appendix V. [New matter.]

15bis.2. Subject to specified restrictions and exceptions (Art.15bis.3), all names on lists enumerated in Appendix V, together with their autonyms, are protected, i.e. (a) they are treated as if conserved against earlier homonyms and competing synonyms; (b) they are accepted as validly published in the place and on the date cited in the lists; (c) their type, when listed, is treated as if conserved under Art.14.3; (d) their adopted spelling and, when specified, their gender are treated as if conserved under Art.14.10. [New matter.]

15bis.3. Protection can, for individual lists, be restricted with respect to the options described in Art.15bis.2, and individual entries on a list can be excepted from protection. Such restrictions and exceptions are to be specified in Appendix V. [New matter.]

15 bis.4. Once a list has been approved, entries can be added to, modified in or removed from that list only by a process analogous to conservation (see Art.14.11 and Art.15.1). Stated restrictions and exceptions (Art.15bis.3) can be waived or modified only by the decision of an International Botanical Congress. [New matter.]

Ib: Draft of (partly editorial) Code changes related to sanctioning and to correlation of protection with sanctioning (only family and genus NCU lists)

New Art.15bis (continued)

15bis.5. Names sanctioned under Art.13.1(d) are treated as if conserved against earlier homonyms and competing synonyms. Such names, once sanctioned, remain sanctioned even if elsewhere in the sanctioning works the sanctioning author does not recognize them. [Transfer from 13.1(d) – replace by a cross-reference – and 14.12 – delete there.]

15bis.6. An earlier homonym of a protected or sanctioned name is not made illegitimate by that protection or sanction but is unavailable for use; if legitimate, it may serve as a basionym of another name or combination based on the same type (see also Art.68.3). [To parallel Art.14.9, where mention of sanctioning can then be deleted.]

15bis.7. When, for a taxon from family to genus inclusive, two or more protected or sanctioned names compete, Art.11.2 governs the choice of the correct name (see also Art.15bis.10). [New matter — but partly covered by Art.14 Note 2, 2nd sentence.]

15bis.8. When, for a taxon below the rank of genus, one or more sanctioned names and/or one or more names with the same epithet and type as a sanctioned name compete, Art.11.3 governs the choice of the correct name. [New matter.]

Note 1. The date of protection or sanction does not affect the priority of a protected or sanctioned name, which is determined only on the basis of valid publication as specified in Art.11. In particular, when two or more homonyms are sanctioned only the earliest of them can be used, the later being illegitimate under Art.64. [Results from splitting and rewording Art.14 Note 2, to be changed accordingly — see below.]

15bis.9. A name which is neither sanctioned nor has the same type and epithet as a sanctioned name in the same rank may not be applied to a taxon which includes the type of a sanctioned name in that rank. [New matter, to parallel Art.14.7.]

15bis.10. Conservation (Art.14) and explicit rejection (Art.69.1) override the effect of sanctioning. [New matter.]

Ic: Editorial changes required upon adoption of Ia and Ib, and upon approval of first NCU lists

NB.:- Editorial cross-references to Art.15bis will have to be added wherever appropriate, e.g. under Art.11.3(a).

Changes to Art.14

Note 2. The date of conservation does not affect the priority of a conserved name, which is determined only on the basis of valid publication, as specified in Art.11. [The present Note reworded - and delete the word "other" in Art.14.5, 1st line.]

Art.14.5. [Delete reference to sanctioning and, upon approval of a list of family NCU (the present App.IIB), also reference to that Appendix.]

Art.14.12. [Delete the remaining first sentence upon approval of the list of generic NCU, the latter to include at least type citations for nomina conservanda; Art.15 to become the new Art.14.12, and Rec.15A to be renumbered Rec.14A]

Change to Art.18

Art.18.7. [Upon approval of a list of family NCU (the present App.IIB), transfer here the present §2, 2nd sentence, heading that Appendix.]

Art.18.3. [Upon approval of a list of family NCU (the present App.IIB), add the words "or protected" in line 2, after "conserved".]

II: Further Code change enabling the protection of types of Linnaean names at species rank on the assumption that a list of such types is submitted by the appropriate Special Committee

After adoption of the new Art.15bis (Ia-b, above), delete the words "of families and genera" in Art.15bis.1.
NB.:- approval of such a list of types would be restricted to the options (b) to (d) of Art.15bis.2, and exclude option (a).

III: Further Code changes enabling the protection of NCU at other ranks, notably of species NCU

After adoption of the new Art.15bis (Ia-b and II, above), insert the words "protected or" in Art.15bis.8, before "sanctioned" (twice); insert the words "protected nor" in Art.15bis.9, before "sanctioned" (first time), and the word "protected or" in Art.15bis.9, before "sanctioned" (second and third time). Add "(see also Art.15bis.10)." at the end of Art.15bis.8.
NB: Even if no lists of species NCU would be ready for approval at the Tokyo Congress, some might welcome the adoption of provisions enabling the set-up of such lists in the future.

IV: General proposal on procedural matters

The Section to authorize the appointment of a Standing Committee on Lists of Protected Names, to initiate, assist, co-ordinate and vet the production of further lists of NCU and of updatings of the approved lists of NCU (if any) and to report to each subsequent IBC through the General Committee.
NB: This should not be proposed as a Permanent Committee, and not included in Div.III of the Code, but should rather have a status analogous to that of the defunct Committee on Stabilization.

Appendix 2

Proposals to Amend the Code

The following proposals to amend the Berlin Code were agreed at a joint meeting of members of both subcommittees on 23 February 1991. Those present were H.M. Burdet, A. Cronquist, K. Faegri, W. Greuter (non-voting), R. Hnatiuk, D.H. Nicolson, C.H. Stirton, and R.A. Zander.
(1) Add at the end of Art.32.1:- (e) in addition, names (autonyms excepted) published on or after 1 Jan. 1995 must be registered.
(2) Add a new paragraph to Art.32 (perhaps after the present Art.32.2):- Registration is effected by sending to any registering office designated by IAPT the printed matter including the protologue (s), with the name(s) to be registered clearly identified. The date of registration is the date on which the requisite printed material is received at the registering office.
(3) Add at the end of Art.45.1:- After 1 Jan. 1995, when not all the conditions of valid publication have been met prior to registration, the name must be resubmitted for registration once these condi-

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Jones on Greene

John Clarkson
Botany Branch
Department of Primary Industries
Mareeba

Introduction

Visitors to QRS, Atherton, might have noticed, amongst the interesting jumble of papers pinned to the notice board in the laboratory, a short biography of Edward Lee Greene prepared by Marcus Eugene Jones. At first, it might seem odd to find an obituary of a turn-of-the-century American botanist posted on a herbarium notice board in tropical Queensland, but this is no ordinary obituary.

There is a maxim that cautions against speaking ill of the dead. Marcus Jones obviously did not subscribe to this, at least where Edward Greene was involved. Time, too, had done little to temper the obvious dislike Jones had for Greene and his work. The obituary, which is printed below in an

abbreviated form, did not appear until June 1929 [*Contributions to Western Botany No. 15* pp. 25-27], nearly 15 years after Greene's death in November 1915.

Biography

"There have been several notable deaths in the botanical world since my last Contributions [*Contributions to Western Botany No. 14*, June 1912]. Greene, the pest of systematic botany, has gone and relieved us from his botanical drivel. They say that the good that men do lives after them, but the evil is interred with their bones. I suspect that his grave must have been a big one to hold it all.

..... Greene was first, last and all the time a botanical crook, and an unmitigated liar, when it suited him to try to make a point against someone else. ..... One can have patience with a fool but not with a crook. .....

Greene's assurance was limited only by his opportunities, and his assumed superiority in first hand knowledge was sublime to those of us who knew he did not know straight up about what he was writing about. In fact it never had its equal except in some of the writings of Rafinesque and recent pronouncements of Rydberg on Astragalus.

..... Ranunculus populago Greene Erythea 3 19 1895 is another case of sharp practise on the part of Greene. I had that sheet out and under study and named Ranunculus Cusickii in the winter of 1894-95. Greene comes along and finds out what I had done, and gets his name in print before I did mine. This to him was a piece of cute work, like the Mexicans do when they cheat you out of a few cents when you make a bargain with them. ..... Greene was first, last and all the time a cur. .....

Recently I have been going over Greene's Leaflets and notice his treatment of Rhus trilobata, which makes one feel like committing murder, but fortunately Greene has passed beyond human retaliation. His case makes one half inclined to believe in Hell, for no other place would be suitable for him.

..... The only thing I regret is that I did not get after him years before I did, and show him up as he deserved. Had I seen all these remarks when they came out I certainly would have roasted him to the queen's taste. ....."

In a short biography of Greene [*Torreya* **16** (7): 151-175 (1916)], Harley Bartlett noted, "of enemies he had many; of friends but few". There is little doubt where Marcus Jones stood.

Notes on Nancy Burbidge

George Chippendale
Australian National Herbarium

Introduction

Perhaps because I had submitted a list of botanist's names to be considered for entries in the *Australian Dictionary of Biography*, I was asked in August 1990 to prepare a biography of Nancy Burbidge, who died in Canberra on 4th March 1977. I started getting copies of the extant obituaries or biographies, and found that the *A.S.B.S. Newsletter* had not had such an obituary. In writing the requested biography, I found some information that should be of interest to members. I will not duplicate the biography I have prepared, but just some notes.

Nancy was born in the Vicarage, Cleckheaton, Yorkshire, England, on 5th August 1912. Her parents were William Burbidge and his wife Mary Eleanor (known as Eleanor) née Simmonds, who first met on a voyage to Australia c. 1900. William was a member of the Bush Brotherhood at Longreach, Queensland (later becoming an Archdeacon), and Eleanor taught school in Sydney. They were married in England on 3rd October 1906. The couple's only other child was Edward Humphrey, born in 1907, who became an Anglican priest, and was appointed to the Parish of Katanning, W.A. Eleanor started the Kobeelya Church of England Girls School at Katanning in an old residence, and Nancy attended this school before proceeding to Bunbury High School.

William Hartley published an excellent obituary of Nancy in *Brunonia* **1**: 123-129 (1977), with a full list of publications. Kathleen McDonald published an obituary in the *Australian Federation of University Women Newsletter* No. 62 (May 1977), Nancy having been an active member of that Association over a long period. Kathleen McDonald also published an obituary in the *National Parks*

Association Bulletin (A.C.T.) March 1980: 2-5, mentioning that Nancy was a Foundation Member of that Association. Indeed, it was Nancy's enthusiasm that caused me to join N.P.A. while she was president. Brian Lee published an appreciation of Nancy in the *Canberra Times* of 9th March 1977, there having been a short obituary in that newspaper on 5th March 1977.

Another obituary appeared in *The West Australian* newspaper on 8th March 1977; and in the same paper on 18th November 1978 there was a photo and note stating that:- "A pioneer Woman Botanist in W.A., Dr Nancy Tyson Burbidge, has been commemorated with an altar-piece at Mt St Pleasants' St Michael's Anglican Church. The festal frontal shows three banksias with a pair of attendant honey- eaters moulded over blocks of colour that suggest stained glass windows. .... The festal frontal is believed to be the first of its kind in W.A. and used applique, hand and machine stitching and metal thread work."

Nancy's nephew, Dr Andrew Burbidge, made me aware of a short biography of Nancy's mother in the book *Reflections*, ed. by Daphne Popham, published by Carroll's Pty Ltd (1978), and also a biography of Nancy in the book *As I Remember Them*, by Noel Stewart, published by Artlook Books (1987).

After Nancy died, William Hartley assisted Nancy's brother Edward in clearing some things from Nancy's home, including a number of family photographs, several of which were of the wedding of her parents in 1906. I mentioned these to Andrew Burbidge and asked if he would like them, and I will be sending them to him. I have, however, copied some of them onto slides and also photographs.

In the *Canberra Times* of 10th September 1980, there was an advance article about the opening of the Nancy T. Burbidge Amphitheatre in the Australian National Botanic Gardens, Canberra, which actually took place on 14th September. This was a fitting tribute, with funds for its construction being contributed by societies she had been associated with, including the Pan-Pacific and South-East Asia Women's Association ACT, the National Parks Association ACT, the Royal Society of Canberra, the Australian Federation of University Women, and the Australian Systematic Botany Society.

A method for dealing with unordered multistate characters

Y. H. Hwang
Research Division
Natural Science Museum
Taichung, Taiwan

Discussion

It is known that unordered multistate characters should not be treated the same way as other types of characters when practicing numerical taxonomy. For example, it is inappropriate to say that a red organ is more different from a green organ than a yellow one. Some workers transform such characters into several binary characters, but in doing so the characters take on different weights, and it is difficult to determine the precise effects of these changes.

An alternative method would be to use letters instead of numerals for the coding, with only identical letters matching (Sneath and Sokal, 1973). For example, instead of coding yellow, red and green organs as 1, 2 and 3 respectively, we can simply use letters Y, R and G, and any difference between OTUs would be scored as 1 irrespective of the order of states. This seems reasonable, but in practice a programme involving letter comparison is usually less desirable.

This problem can be solved using the following algorithm:-

1) Use integers such as 1, 2, 3 and 4 for coding different states.
2) Reduce to 1 any difference between OTUs greater than 1
   (e.g., ABS(I-J)=D, IF D>1 THEN D=1).

This would produce nearly the same results as letter coding.

Reference

Sneath, P. H. A. and Sokal, R. R. (1973) *Numerical Taxonomy*. (Freeman, San Francisco).

COMMENTARY

Samaras and feathers, or Casuarinas on the wing?

I read with interest the short article by Yee Hwang on problematic fossil Casuarinaceae in the March 1991 issue of the *ASBS Newsletter* (Hwang, 1991). Hwang considers Wilson and Johnson's (1989) identification of early Miocene cones from New Zealand as belonging to *Allocasuarina* "very surprising" [he incorrectly attributes the *Flora of Australia* treatment of Casuarinaceae to Johnson alone]. Noting that it is generally recognized that New Zealand began to separate from Australia in the late Cretaceous, reaching its present position before the Eocene, he suggests "four logical possibilities" to explain that occurrence (if real):-

(i) *Allocasuarina* is polyphyletic
(ii) *Allocasuarina* existed before the Tertiary
(iii) New Zealand began to separate from Australia shortly prior to the Miocene
(iv) Birds carried *Allocasuarina* seeds across the Tasman Sea early in the Miocene

Hwang concludes:- "It is doubtful that any biologist will consider any of these to be plausible except the last one, which might have a slim chance. And even this requires the premise that *Allocasuarina* was well differentiated in the early Miocene."

Perhaps I am no biologist, but I fail to see the logic of this argument. What firm evidence is there to reject (or even doubt) this record? I am no palaeobotanist; nor am I an expert in the Casuarinaceae. However, I do not accept Hwang's inference, given the evidence that he has presented; and it is this aspect of his article that I wish to re-evaluate.

Before I proceed further, Hwang's "four logical possibilities" need clarification. Implicit in his his argument is the fifth possibility, which I shall call (v), that Wilson and Johnson (1989) misidentified the fossil. I shall consider this first. Hwang gives no indication that he has actually seen the fossil in question, and if not, his implied disagreement must be based upon circumstantial evidence. Campbell and Holden (1984), who described the fossil, named it *Casuarina avenacea*. However, they placed it in the "division" Cryptostomae (see Johnson, 1980), which now consists of *Casuarina* s.str., as well as the segregate genera *Allocasuarina* and *Ceuthostoma* (Johnson and Wilson, 1989). (The genus *Gymnostoma* was raised from the 'division' Gymnostomae.) This specimen was re-identified as an *Allocasuarina* by Johnson and Wilson (1989) and Wilson and Johnson (1989), although Johnson (see below) now allows that the detail preserved in the infructescences is poor and the fossil is likely to belong to *Casuarina* s.str. Even though the generic identity of the fossil is uncertain, it does not affect the following argument, which is based upon the internal logic of Hwang's commentary. The same logic would apply to the genus *Casuarina* s.str. and whether it occurred in New Zealand in the Miocene.

Now I shall consider Hwang's four "logical" options:-

(i) It seems unlikely that *Allocasuarina* is polyphyletic, but can the possibility be rejected out of hand? It would require convergence only in a couple of bracteole characters (since only cones were preserved in the New Zealand fossils). However, a much stronger possibility that Hwang has overlooked is that *Allocasuarina* might be *paraphyletic*. That is, it may be a basal assemblage within the Casuarinaceae, having only primitive characters in common. Given the almost total ignorance of outgroup relationships in the Casuarinaceae (Johnson and Wilson, 1989), and therefore the current difficulty of rooting a phylogenetic tree of the family, *any* group within it could be paraphyletic. In other words, *Allocasuarina* could be as old as the family itself.

However, a cladogram of the Casuarinaceae could be rooted using the direct method, i.e. ontogeny (Nelson, 1973; Weston, 1988). All members of the family have their scale-leaves in whorls of four at an early stage of development, and some go on to develop whorls with higher numbers, namely all species of *Casuarina* and most of *Allocasuarina* (Johnson and Wilson, 1989). All species of *Gymnostoma* and some of *Allocasuarina* retain whorls of four through to the mature plant (Johnson and Wilson, 1989). Using the direct method (Weston, 1988), this suggests that whorls of four are primitive, and whorls of higher numbers are more advanced. Therefore, either *Gymnostoma* or *Allocasuarina* could be basal within Casuarinaceae, and either could be monophyletic or paraphyletic. I should make it clear that I am not suggesting that either genus *is* paraphyletic, only that either *could be*, given the evidence at hand.

(ii) That the Casuarinaceae might have existed

before the Tertiary seems very plausible to me. Given the interrupted nature of the fossil record, any group is likely to be older than its earliest known fossil. Pollen of the Casuarinaceae is known from the Palaeocene (Johnson and Wilson, 1989) i.e. from the very beginning of the Tertiary. Therefore it is very likely that the Casuarinaceae existed in the Cretaceous and pre-dated the separation of Australia and New Zealand. If *Allocasuarina* is paraphyletic and basal, this would be consistent with a Miocene record for New Zealand. Even if *Allocasuarina* is monophyletic, it may well have diverged early in the evolution of the family, during the Cretaceous.

The Australian megafossil record suggests that *Gymnostoma*, which first appeared in the Palaeocene, is older than *Casuarina* and *Allocasuarina*, both of which appeared more or less simultaneously in the Oligocene (Cristophel, 1989). However, I must re-emphasize that the fossil record is notoriously incomplete, and only ever gives a minimum age for a taxon.

(iii) A Tertiary separation of Australia from New Zealand would put egg on a lot of geologists' faces, and I must agree with Hwang that it is unlikely.

(iv) Hwang's "most likely" scenario seems among the least likely to me. As he correctly points out, it would require a pre-Miocene origin of *Allocasuarina* anyway. Additionally, it requires an unlikely dispersal event across the Tasman Sea, followed by successful establishment in a new environment. And even if *Allocasuarina* did disperse across the sea, why assume that birds were the agent? Miocene birds? Why not marine crocodiles? Or turtles? Or perhaps the winged fruits (samaras) were carried by the wind? Or floated on the sea? The point is that any of these scenarios require three unsubstantiated assumptions:- (a) that dispersal occurred, (b) that the dispersal agent was a bird (or whatever), and (c) that establishment occurred in New Zealand. This is a much less likely explanation than either (i), (ii) or (iii), each of which requires fewer *ad hoc* assumptions. Therefore, to compare the four scenarios on a more equal basis, (iv) should be simplified by making no assumptions about the dispersing agent.

All of the possibilities (i), (iii) and (v) require the *ad hoc* rejection of hypotheses based upon real evidence, i.e. Johnson and Wilson's observations of morphology in Casuarinaceae, and geological evidence of the history of the Australian and New Zealand land masses. Even if the identification of the fossil is changed (given the present uncertainty), the same logic applies. Hypothesis (iv) requires the *ad hoc* assumptions that dispersal and establishment occurred.

By contrast with all the other hypotheses, (ii) requires no *ad hoc* assumptions at all. A fossil was found in New Zealand, detemined as *Allocasuarina*, and dated to the Miocene. *Ergo*, the genus existed in New Zealand in the Miocene! The simplest explanation of its presence there, as well as in Australia (both in the recent and as fossils), requiring no unsubstantiated assumptions, is that it originated just once, in Australia–New Zealand when they still formed a single land-mass during the Cretaceous (or earlier?). As for Hwang's surprise that *Allocasuarina* could have existed during the Cretaceous:- is there any evidence to suggest that it could not have existed that long ago? If the fossil is re-identified as *Casuarina*, the same arguments apply.

Finally, Hwang has not considered another possibility, presumably because he did not accept the Miocene record of *Allocasuarina* in the first place. This is that *Allocasuarina* evolved in New Zealand after separation of the land masses, and then dispersed across the Tasman to Australia. Kiwis would love this hypothesis, but unfortunately, it also requires *ad hoc* assumptions about dispersal and establishment.

Acknowledgements

Thanks to Pete Cranston, Chris Reid and Peter Weston for their comments on a draft of this article.

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Mike Crisp
Division of Botany and Zoology
Australian National University

Casuarinaceae — some clarifications

Mike Crisp (see above) has dealt effectively with the deficient logic of the article by Hwang (1991).

However, some points in his analysis call for comment.

(1) The taxonomic position of the fossil species *Casuarina avenacea* Campbell & Holden.

Karen Wilson and I (refs in Crisp, l.c.) formerly considered that this was probably referable to *Allocasuarina*. Having re-examined the photographs, I now think it likely that we misinterpreted the nature of the fruiting bracteoles in the poorly preserved infructescences. I hope to be able to examine some of the actual material, and would have preferred not to be compelled by Hwang's note, and the reaction to it, to comment at this time. At present, I consider it likely that the squashed material gave us a false impression of the fruiting bracteoles (so-called "valves" of the so-called "cones"), and that these were probably unthickened and devoid of dorsal protuberances as in *Casuarina* s.str. In that case, it seems likely that a species not too unlike the eastern Australian *C. cunninghamiana* Miq. may be involved.

(2) Is *Allocasuarina* monophyletic in the Hennigian sense, that is holophyletic (neither paraphyletic nor polyphyletic)?

I may say that had I not believed that it was holophyletic, I would not have set it up as a genus. Consideration of this point depends, of course, and as Crisp has pointed out, on whether one can assign evolutionary polarity with reasonable confidence to the character-state transformations involved in the internal phylogeny of the Casuarinaceae.

Crisp claims that "almost total ignorance of outgroup relationships" in the family demonstrates the "current difficulty of rooting a phylogenetic tree of the family". This is the basis of the further logical conclusions that Crisp draws, but I see no reason to accept it, not to accept that the only 'evidence at hand' is that used by Crisp.

The outgroup of the Casuarinaceae can be taken as the progenitor of the Hamamelidae as a whole, or, if necessary, of the dicotyledons as a whole, and the likely polarity assigned on this basis. Hypotheses of descent within the family can be based on this, in conjunction, if necessary or possible, with ontogenetic considerations, rather unjustifiably called the "direct method" by Crisp and by Weston (1988).

This *Newsletter* is not the place to substantiate our conclusions in detail, but such considerations lead to the conclusion that:-

(a) all of the four genera of the Casuarinaceae are holophyletic;

(b) either (i) *Gymnostoma* + *Ceuthostoma* together are the sister group of *Casuarina* + *Allocasuarina* (Figure 1a); or (ii) (less likely, I believe) *Gymnostoma* is sister to the other three genera, with *Ceuthostoma* as sister to *Casuarina* + *Allocasuarina* (Figure 1b). To consider *Allocasuarina* as paraphyletic involves an unparsimonious set of additional hypotheses.

(3) What is the most reasonable scenario to 'explain' the neo- and palaeo-phytogeography of the Casuarinaceae?

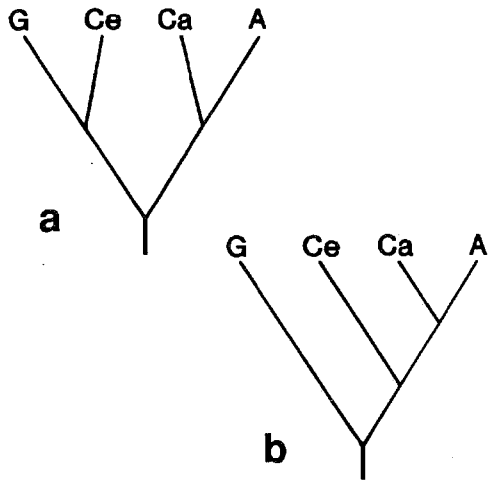

Figure 1. The two most likely alternative phylogenies of *Gymnostoma* (G), *Ceuthostoma* (Ce), *Casuarina* (Ca), and *Allocasuarina* (A). All internodes would represent at least one synapomorphy.

The answer to this will depend partly on more definite conclusions as to the position of *C. avenacea*, but it will have to take account of:-

(a) the apparent (and certainly present-day) restriction of *Allocasuarina* to Australia and to generally oligotrophic substrates, as well as the number of apomorphic and specialized characters found in that genus, and its very considerable morphological radiation in such habitats *in Australia alone*;

(b) the relative lack of such morphological and substrate-related specializations in *Casuarina*, together with the wide distribution of that genus with closely-related (non-maritime but often riparian) species in Australia, New Caledonia, Vanuatu, the island of New Guinea, the Sunda Islands, Celebes, the Philippines, and probably New Zealand in the Miocene). I believe that the evidence does suggest dispersal (but, I would think, by wind rather than birds or what-have-you) in *Casuarina*; and

(c) the former existence of *Gymnostoma* in southern parts of Australia (including Tasmania), New Zealand, and southern South America.

The present and known past distribution makes sense in relation to the phylogeny that Wilson and I have concluded is most probable, and less sense otherwise.

Acknowledgements

I am grateful to Mike Crisp for a preview of a draft of his article.

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Lawrie A.S. Johnson
National Herbarium of New South Wales
Royal Botanic Gardens, Sydney

Casuarinas on the wing, or fancies on the wing?

This is a response to Mike Crisp's and Lawrie Johnson's commentaries (see above) on my last *Newsletter* article (Hwang, 1991). I admit that I was rather conservative in that article. However, I believe that scientists live in a world of evidence and probability. So, we need to start by getting our focus clearly defined. The point I tried to get across in my article was:- when Johnson told us about the Miocene *Allocasuarina* from New Zealand, it would have been useful to explain the possibilities concerning its origin. Otherwise, we need some further convincing diagnostic data about the fossils. If this is impossible, then we might consider an option from my four possibilities, being aware that many people do not like an extant genus being polyphyletic.

More details concerning the points raised are as follows:-

1. I have, in the past, considered the possibility that *Allocasuarina* is a polyphyletic 'genus' (Hwang, 1989). Should this name survive, I would like to see at least *C. pinaster*, *C. acuaria*, *C. grevilleoides* and *C. fibrosa* removed from it. Crisp argues that *Allocasuarina* could be as old as the family itself. I feel that he needs to specify a particular sub-group in this heterogeneous *Allocasuarina*, otherwise the discussion cannot proceed. However, I certainly cannot imagine that the "distyla group", the nucleus of *Allocasuarina*, be seen as being as old as the family.

Crisp points out the current difficulties with rooting a tree of the family, then moves on to the words "*Allocasuarina* could be as old as the family". This is like saying that, without playing the game of cladistics, classical taxonomists should not and cannot determine which genus is more derived in a family. I think that some classical works are still better than some of the mysteriously-polarized, lengthy "shortest" phylogenies produced using arbitrarily-subdivided continuous characters. Of course the reverse also applies, so do not misunderstand me if you agree that the major factor is whether one is careful and responsible in one's work (and maybe not in a frenzy to produce papers and new names).

Johnson's proferred phylogenies do not contribute to the discussion, because "this *Newsletter* is not the place", apparently, to present any data. Until the data on which these trees are based is presented, we cannot assess their relative merits, either to each other or to the other possibilities.

2. Nothing is impossible, and any taxonomic group is likely to be older than its earliest known fossil, but the earliest definite occurrence of the Cryptostomae is from the Miocene, and it was a long time between this age and the age of the separation of New Zealand from Australia. Whilst Crisp

reminds us that the same logic applies to *Casuarina* s.str., I would like to remind him that the simple facts we learn from the report of Campbell and Holden (1984) are:- (a) *C. avenacea* belongs in the Cryptostomae, and (b) this species is extinct.

Both Crisp and Johnson seem determined to assign a modern generic name to a Miocene species. I am not prepared to accept a polyphyletic genus at all, but a polyphyletic Cryptostomae looks likely to me.

I have not seen the fossils of *C. avenacea* in person. What I stressed was an 'IF A THEN B' logic, i.e. 'IF Johnson insists ... THEN ...', whereas Crisp's logic is 'IF NOT *Allocasuarina* THEN *Casuarina* s.str.'. In doing so, he implies that Miocene fossils from New Zealand must belong in modern Australian genera (with no political implications intended). Of course there is a possibility that *C. avenacea* belongs in either *Allocasuarina* or *Casuarina* s.str., but let us reserve our energy until somebody has analysed trustworthy data about the fossil, as, for example, Christophel (1980) has already done for some other fossils. Now that Johnson does not insist on his previous identification any more, further arguments in this respect seem unnecessary.

3. Some birds (e.g. cockatoos) are interested in casuarina fruits. I apologize that I did not and cannot give a literature reference for this — it is a personal observation. I do not understand what is wrong with the concept of Miocene birds (see Feduccia, 1980). Let us not talk about marine crocodiles, turtles or Miocene mermaids, but let us distinguish between the flying ability of the casuarina samara and *Taraxacum* achenes. If only a dozen of the *Allocasuarina* species could be dispersed by seawater, I probably would not have bothered to take wing to Australia myself. Anyhow, I wrote "... which might have a SLIM chance". Presumably my English expression was not good enough to express that I consider the fourth possibility to be almost as implausible as the first three.

Finally, let us not say that New Zealand might have been the headquarters of casuarinas had the Kiwis been diligent in digging for their fossils. Nothing is impossible, but I prefer to rely on existing evidence. In my opinion, it is not necessarily a shame that one modifies one's hypothesis upon new discoveries, as long as we carefully express our views without forcing an acceptance of them.

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Yee H. Hwang
Research Division
Natural Science Museum
Taichung, Taiwan

Publishing of ASBS Council meetings

After reading David Morrison's "Not-so-brief review of the history ..." of the *Newsletter* (*A.S.B.S. Newsletter* **66:** 4-14 [1991]), it is necessary to provide a brief comment on the reasons why the publishing of ASBS Council Meetings has declined in recent years. There is one reason. All matters discussed in Council Meetings are also discussed in the General Meeting, and are fully reported in the minutes of the latter. The only additional aspects discussed in the Council Meetings concern the implementation of those matters arising from the General Meeting.

It would be most unfortunate if 'history' concluded that a deterioration in relationships between Council and the *Newsletter* editor had resulted in these reports not being published in recent years.

Barry Conn
Secretary, ASBS Inc.

My comment on the *Newsletter* is that I would certainly like to see more personal matters mentioned, as Selwyn Everist suggested. To me, this is a most important function, considering that many botanists only know one another by repute, until they can attend a national conference, and even then only meet a sprinkling of people.

George Chippendale
Raoul Place, Canberra

A.S.B.S. Inc. BUSINESS

Fourteenth General Meeting

The 14th General Meeting of the Australian Systematic Botany Society Incorporated will be held on Wednesday 27th November 1991 at the University of Auckland, Auckland, New Zealand, following that day's symposium proceedings.

Any member wishing to place an item (items) on the agenda should notify the Secretary (Dr B.J. Conn) in writing by Wednesday the 13th November 1991.

Council Elections

In accordance with the Constitution of the Society, nominations are called for all positions on the Council for the 1991–1992 term of office:- President, Vice-President, Secretary, Treasurer and two Councillors.

Judy West, the current President, having served two consecutive terms on Council as President, is ineligible to continue in that position.

Don Foreman, the current Treasurer, having served four consecutive terms on Council as Treasurer, is ineligible to continue in that position.

Each nominee must be proposed by two members and his/her acceptance of nomination must accompany the nomination itself. Nominations must be on the form enclosed in this *Newsletter* or on a facsimile of that form. All nominations must be in the hands of the Secretary (Dr B.J. Conn) by Thursday the 3rd October 1991.

Systematic and Ecological Relationships of the South Pacific Floras

November 22-27, 1991 Auckland, New Zealand

Planning for this joint meeting of the Australian Systematic Botany Society Inc. and the New Zealand Botanical Society is continuing. The first circular was distributed in early May, calling for provisional registrations of interest. If you have not seen this circular and would like to, please write to Barry Conn (NSW) or Anthony Wright (Auckland Institute & Museum, Private Bag, Auckland 1, New Zealand).

A second and final circular giving details of requirements for papers and abstracts, registration fees, programme, tours, and including a booking form to be returned with the deposits, will be sent to those who have responded to the first circular. The second circular will be posted in July.

Reduced airfares have been offered by Qantas to participants of the Conference. The cost of return flights (Brisbane–Auckland, Melbourne–Auckland and Sydney–Auckland) will be $474.00, a saving of about $100.00 on the current Super-Apex fares. Those who plan to join the post-conference tour will be able to directly return via Christchurch. Internal reduced fares between Auckland and Christchurch will probably be available. There may be some problems obtaining these discounted fares for those using government warrants and/or for those whose departments have contracts with a particular travel agent. All potential participants who wish to obtain more information about these discounted fares should contact Barry Conn, as soon as possible.

Barry Conn
Secretary, ASBS Inc.

A.S.B.S. Member Profiles

Judy West
A.S.B.S. President

My early career was not quite as exciting as that of Barry Conn (see below), but it did also start in a small town, that of Harden, N.S.W.

I began my botanical journey under the influence of the indomitable Roger Carolin, at the University of Sydney. After my undergraduate degree I took a year off to attempt to sort out where I was going. Part of that time was spent as a technical assistant at the National Herbarium of New South Wales, working on the Surrey Jacobs and

John Pickard *Plants of New South Wales* census; with personalities like those to associate with I completely lost any sense of direction that I already had. However, with courage, I returned to Sydney University to do Honours with Roger; and that really cured me of any wandering from the straight and narrow, and convinced me of the direction I wanted to take — plant systematics. Roger's lateral thinking, stimulating ideas, great personality, and friendship couldn't help but enthuse me with the area of our science.

From my early days I was keen to be a scientist, and I can remember telling one of those adults who incessantly asked: "What do you want to do when you grow up?", that I was going to work for CSIRO! I had probably just learnt what it meant, but I think my interest in the biological sciences was also partly influenced by my farming father's persistence that I should "do something" about that weed, this tree, or the other insect pest.

The year 1973 took me to Adelaide, and a tutorship in the Botany Department at the University of Adelaide. This teaching period I thoroughly enjoyed, but I struggled to find enough continuous time to work on my Ph.D.; and I finally gave up, resigning the position to complete my doctorate full-time. My thesis was centred on a revision of *Dodonaea* in Australia. The one advantage of the part-time Ph.D. position was that it gave me a longer period for obtaining some valid results on breeding system data from these perennial plants.

I took up my present position in the Australian National Herbarium of the Division of Plant Industry, C.S.I.R.O., in 1980, although I had a temporary position at first. Coming to Canberra for the first time was a major change, as I had become very attached to Adelaide and the state of South Australia (not only because of the Barossa Valley).

My interests in the semi-arid and mallee areas are still extremely strong, and partly lead me to my recent work on *Calandrinia*, with its many ephemeral species. My other major research interests include revisionary work, biogeographic analyses and phylogenetic studies of groups such as *Portulaca* and the major native Australian Caryophyllaceae genera, as well as breeding system studies in selected taxa, and molecular systematics. My broader interests include genetic diversity, conservation biology, and options for integrated land use and management.

Much of my time in my present job is occupied by my role as leader of our divisional research programme involving native plants and vegetation (called Australian Flora Resources and Management). I seem to spend more time pushing paper, chasing money, and discussing and deciding upon research options and projects for parts of this diverse group than I do on my own systematics research. I'm hoping that it will get better, but I've heard others say the same, and I feel that the whirlpool is sucking me in. Even so, it's extremely stimulating and challenging to be involved with not only the systematics group (Systematics of Australian Plants), but also fire ecology (Fire Effects in Australian Plant Communities), plant population biologists (Australian Plants as Genetic Resources), and horticultural research (Management of Australian Plants in Cultivation).

In contrast to all of those non-ACT-resident Canberra-bashers, I have found Canberra to be a great place to live. There are many occasions when you can feel that you would rather be further afield from the unbelievably frustrating bureaucracy and the hypocrisy up on the Hill, but the lifestyle is very suitable for me. From the work point-of-view, one small area enables endless interactions and opportunities. Besides, I think Ian Noble and I would be struggling even more to co-ordinate two busy careers and something like family life in a city that did not offer such easy access to facilities, including the various options for quality child-care. Somehow, our daughters, Kim and Leah, seem to cope with two crazy parents attempting to juggle pretty active lives.

On a more taxonomic note, I think that the relevance and importance of our discipline must be more widely recognized, and, as systematists, we should pay more serious attention to the communication of our science in these times of awareness. I have had involvement with the council of our society for several years, and I believe that it has much to contribute to the scientific and environmental arenas. When you compare our systematic community with other countries, it is obvious that we are in a very privileged position. It is to our advantage that we have a smaller group of systematists, because we are closely-knit and able to communicate easily and (in most cases) to work together towards common goals. In many aspects we are holding our own, if not leading the way, in our science — we must maintain these standards and keep the links. As the current president, I would like to see the society more active on the national front. I think that we have the potential to use our collective talents to more effective actions. We are following up some specifics along these lines, and I'll write more on that in the next *Newsletter*.

Barry Conn
A.S.B.S. Secretary

The trouble started at midnight — I was born. Unfortunately, none of the doctors or nurses looked at the clock to record which side of midnight this important event happened. Anyway, Dr Hitchley decided it was to be the 4th because the 'Registration of Birth' certificate did not allow for a "We didn't look" category. Even though I was born because I wanted to be with my mother, the whole thing was so stressful to her, that she chose not to be there; she was unconscious for the entire performance. As an expression of her interest in this event, she could never remember if it was a choice between the 3rd and 4th or the 4th and 5th.

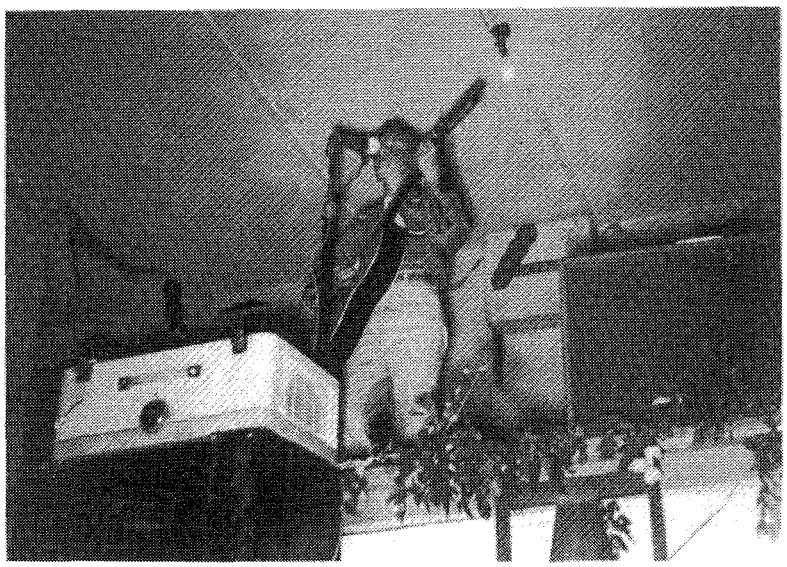
The day after whatever day I was born, my father was feeling so guilty about contributing to the post-war baby boom that he crushed his finger in the steerage of a plough. With 'true grit' he drove himself the 5 miles to the Hospital to have the remains of his finger amputated. My mother, still drugged to the eye-balls, now breast-feeding another lady's twins, plus me, greeted my father with "It's nice to see you" and "How's the weather?" Country folk are like that.

The next problem was that I was born in Dimboola, Vic. Not that this would have been a problem in itself, but unfortunately some out-of-work writer wrote a play about a wedding in the Railway Institute. The only other historically important event to occur there was a performance of the song "Teddy Bear" by 12-year-old Barry Conn (see photo). It is rather frightening to imagine what it must have sounded like; some of you may be aware of the slightly more popular Elvis Presley version.

My first herbarium was collected when I was about 15 years old — more a piece of art really. "Six Important Forest Trees" consisting of leaves and fruits, accompanied by wood and bark samples. My grandfather, who was a forester, chopped the trees down for me, and my father repaired the hole in the pig sty with the bits that I didn't use. This was probably the first indication that I may have had a flair for taxonomy. The second flares came much later, in the 1970's, after I was allowed to wear long pants. It was at about this time that my interest in taxonomy was re-kindled.

Sue Duigan tried to inspire second year botany students at the University of Melbourne to be enthusiastic about taxonomy. However, it was not the inspirational manner in which she presented such riveting concepts as the ICBN that was the main incentive for passing, nor was it the fear of having to repeat the course; rather, it was chronic muscular dystrophy induced by carrying *The Families and Genera of Victorian Plants* and her exhaustive notes on plant families. Two years later, I completed a double major in Zoology and Botany, specializing in ecology, embryology, mycology and phycology, plus education. Although I knew that taxonomy was not for me, I obviously could not decide what I really wanted.

Rather disappointed that I couldn't continue



with ecology for Science Honours, I inflicted as many radical views as I could think of on to students at Horsham High School. By chance, next to the comics in the saturday edition of *The Sun News-Pictorial* was an advertisement for a botanist in Papua New Guinea. I applied because I wanted the New Guinea stamps that would come on the return envelope. Unfortunately, I was notified from Sydney, and they used a franking machine. Anyway, I was offered the job. After meeting John Womersley, my new boss, I realized that Bill Barker and I were the only people to apply for the two botanist positions; all the other potential applicants knew more about John Womersley than we did.

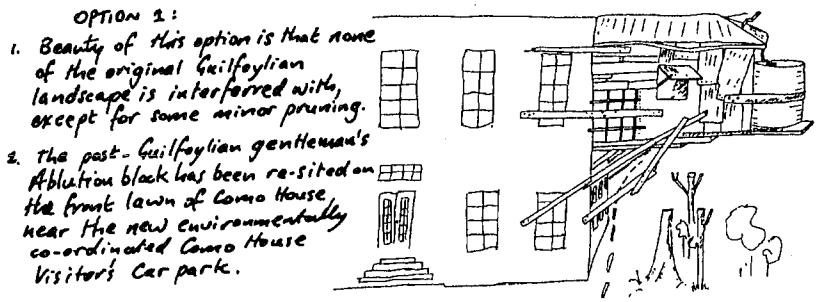
Ted Henty's (LAE) introduction to New Guinea taxonomy was to suggest that I spend the first 2 days in the library, familiarizing myself with the available literature. To my horror, half the books were in German (I knew that from my war-comic days), most of the others were in Latin (although I only guessed that) and the others were in a form of English that was equally incomprehensible. It's very exhausting trying to maintain a learned expression in a botanical library. After I asked him what I should do next, he suggested that I pick a family of plants from the herbarium and "look" at them. Of all the plant names I saw, only two meant something to me — *Casuarina* and *Drosera*. Ted told me that I could not "do" *Casuarina* because some bloke named Johnson had been "working" on them for over 20 years; I couldn't believe it. Anyway, Don Foreman patiently explained what "working" on a group actually meant. Bill Barker was too busy playing post-Ph.D. somethings to be bothered introducing the real world of taxonomy to an ex-school teacher.

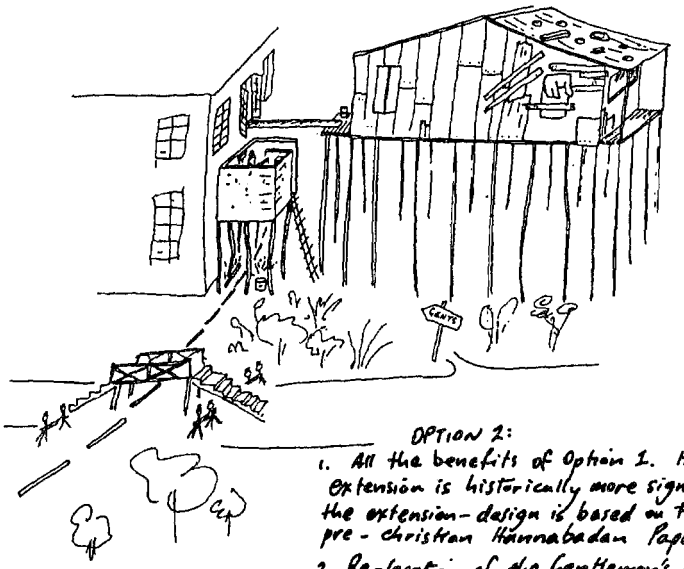
While at Lae, I was awarded the Joe Havel Award (I gave it to myself and Bill Barker), for re-writing *Forest Botany, Part 2, Botanical Taxonomy* by J.J. Havel under rather difficult circumstances. For the full significance of this award you had to know John Womersley and his secretary Sue Osborn. You had to be there. (Ask Bill, I'm already in enough trouble for writing this article.)

Three years of my time in New Guinea were spent as a lecturer at the Forestry College in Bulolo. Sue Duigan and Carrick Chambers (plus others, I believe) supported my efforts to submit a thesis for an external Master of Science degree at the University of Melbourne. Although Sue must have wondered why I hadn't listened to her lectures, she patiently made extensive comments on the manuscript — her subtle way of getting back at me for not paying attention all those years ago.

Fortunately, Judy West then decided to give up living in the fast lane as a tutor while doing a Ph.D. at the University of Adelaide; she wanted to concentrate on completing her Ph.D (see above). This position was offered to me. John Jessop suggested that I "work" (I now knew what this meant) on *Prostanthera*. After three and a half years of such intellectual pursuits as bouncing tennis balls off water (at the beach with the Barkers), collating *A.S.B.S. Newsletters* (with others, but mostly with the Barkers), many card evenings (but never allowing Bill and Robyn to play on the same team), a Ph.D., and writing up other bits and pieces left

Proposed architectural designs for the extensions to the National Herbarium of Victoria, none of which were accepted



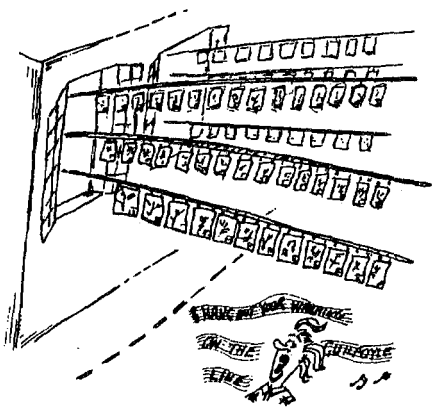


OPTION 2:

1. All the benefits of Option 1. However, the extension is historically more significant because the extension-design is based on traditional pre-christian Hannabadan Papuan design.
2. Re-location of the Gentlemen's Ablution Block unnecessary with this pseudo-Papuan architectural conversion.
3. The Guilfoylian pedestrian by-pass install so as to avoid visitors interferring with the Guilfoylian line.

OPTION 3:

The need for an extension would be reduced if the Herbarium was designed on the principles of "OPEN-PLAN"



The key structural component in all three options derive from one invented in the 18th century— "the National Truss."

over from New Guinea, I was offered a position at the National Herbarium of Victoria.

While there I became the Scarlet Pimpernel of the MEL Resistance Movement. It was to be my renaissance into managerial philosophy and theory. I didn't always fully agree with the direction that David Churchill seemed to be leading us. It might be true that "There will always be an England", as one of his former namesakes once said, but I was not convinced that the same standing-alone mentality would guarantee that "There would always be a MEL". I was always disappointed that my proposed architectural designs for the MEL herbarium extension were rejected. They met all of the specifications:- no observable increase in size, and no encroachment onto the Guilfoylian landscape (several of the better rejected designs are included here).

Meanwhile, the National Herbarium of New South Wales had collected all these unrelated, extremely difficult plant families that no-one wanted to curate. So, they hatched an elaborate plan, which was something like this:- "Let's create a new botanical gardens at Mt Annan; we'll need an extra botanist to assist with the increased amount of identifications; we'll call him the Mt Annan Botanist; his office will be in Sydney because [only spoken when the Minister wasn't listening] we are not really going to use him just for Mt Annan". It took me over 12 months to actually get to Mt Annan, and then I tore my trousers jumping a fence while trying to avoid getting my feet wet in the tyre-washing trough around the plant nursery.

It's all been downhill since then.

REPORTS

Australian Biological Resources Study

By the closing date of 10th April, applications for ABRS Flora grants in 1992 totalled 65, of which 21 are for renewals of existing projects and 44 are for new projects. The Advisory Committee will meet on 29-30th August to make its recommendations.

The complete text for the *Australian Plant Name Index* (*APNI*) is now with the printer, and is scheduled for publication in late June. The retail price for the 4-volume set (AGPS Cat. No. 91 5157 5) will be $195, with individual volumes priced as follows:- Vol. 12, letters A-C (Cat. No. 91 0053 8) $59.95; Vol. 13, D-J (91 0054 X) $54.95; Vol. 14, K-P (91 0055 1) $49.95; Vol. 15, Q-Z (91 0056 3) $39.95.

As a special offer, however, *APNI* will be available (complete set only) for $150 until 30th September 1991.

APNI may be obtained from AGPS bookshops in the state capital cities, by mail order from AGPS Mail Order Sales, G.P.O. Box 84, Canberra, A.C.T. 2601, or by phoning (008) 02-0049 (24-hour, 7-day service) (Canberra customers, 295-4861). The prices include postage.

Alex George has been appointed to the Steering Committee for the "Species Plantarum Project", and will attend its first meeting at the Royal Botanic Gardens Kew, on 27-29th June. Reports on the project will be made regularly through this *Newsletter*, but anyone requiring information or wishing to make any input to the committee is welcome to contact Alex at ABRS (06 250-9440).

Alex George
Acting Associate Director, Flora

A.S.B.S. Melbourne Chapter

A.S.B.S. Melbourne Chapter seminars are held at 6 pm on the first Wednesday of each month in the School of Botany, University of Melbourne. Refreshments are served at 5.45 pm. Parking is available in the University grounds after 5.15 pm. Enter from Grattan St or Swanson St (but not into Tin Alley) and park in the underground carpark.

Onstreet parking is usually also available along Royal Pde at about this time. Members and non-members are most welcome.

Seminar Program, May-September 1991

Wednesday, May 1st
Greg Howell
School of Botany, University of Melbourne
"A botanical glimpse of southern Africa"

Wednesday, June 5th
Stephen Forbes
Royal Botanic Gardens Melbourne
"Flora of the Canary Islands"

Wednesday, July 3rd
Sue O'Brien
School of Botany, University of Melbourne
"Pollination and reproductive ecology of *Leptospermum* in coastal Victoria"

Wednesday, August 7th
Ian Lunt
Department of Botany, La Trobe University
"Conservation and management of native grasslands and grassy woodlands"

Wednesday, September 4th
Tim Entwisle
National Herbarium of Victoria
"Freshwater macroalgae of Australia: out of the unknown"

Further information can be obtained from me on (03) 344 5252.

Andrew Drinnan
Convener

Report on "Advances in Labiatae Science"

The first "Advances in Labiate Science" conference was held at the Royal Botanic Gardens and Jodrell Laboratory, Kew (Richmond, England) from 2-5th April 1991. This was a joint conference, supported and organised by the Royal Botanic Gardens Kew. Approximately 120 participants, representing 25 countries, took part.

The conference was introduced by three 'overview' papers, namely, "A global survey of the biogeography of the Labiatae" (I. Hedge), "A chemical overview of the Labiatae" (P. Richardson) and "A phylogenetic evaluation of suprageneric groupings in the Labiatae" (P. Cantino). The endemic Australian Labiatae was discussed in a paper by myself ("Relationships within the tribe Prostanthereae"). This paper also provided a different interpretation of the phylogenetic relationships between the Verbenaceae and Labiatae, compared to that presented by Philip Cantino.

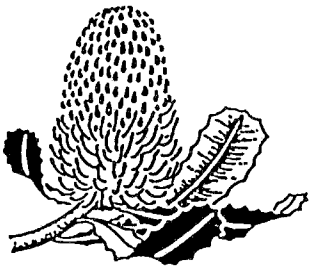
Several papers were presented on the chemistry of the family, namely, "Essential oils as taxonomic markers in *Mentha*" (S. Kokkini), "Significance of the terpenoids on the Labiatae" (M. Cole), "Flavonoides of the Labiatae" (F. Tomás-Barberán) and "Chemical components of Labiatae oils and their exploitation" (B. Lawrence). Phylogenetic relationships using chloroplast DNA variation was presented by R. Olmstead ("Chloroplast DNA in the Asteridae: phylogenetic implications for the Lamiales") and S. Wagstaff ("A phylogenetic analysis of chloroplast DNA variation in tribe Mentheae, Labiatae").

Several papers were presented on the breeding systems and pollen morphology. These included:- "Breeding systems in Labiatae" (S. Owens & J. Ubera), "The megagametophyte in Labiatae" (P. Rudall & L. Clark), "The potential value of pollen morphology as an additional taxonomic character in subtribe Ociminae (Ocimeae: Nepetoideae)" (M. Harley), "Pollen morphology in subfamily Lamioideae (Labiatae) and its phylogenetic implications" (Mones Abu-Asab), "Pollen morphology of the genus *Salvia* in Jordan and the neighbouring countries" (S. Al-Eisawi) and an "Overview of pollination biology in Lamiaceae" (R. Huck).

The taxonomic usefulness of the fruits was presented by A. Paton ("The adaptive significance of calyx and nutlet morphology in *Scutellaria* L.") and O. Ryding ("Pericarp structure and phylogeny within Lamiaceae subfamily Nepetoideae tribe Ocimeae"). R. Harley discussed "The *Hyptis* alliance in the New World" and T. Krestovskaja presented "A systematic study in *Leonurus*". Ethnobotanical aspects were dealt with by two presentations, namely "Ethnobotany of Labiatae in the Old World" (A. Rivera-Núñez & C. Obón-de-Castro). Dr Ramamoorthy provided an excellent summary of the meeting and chaired an informative general discussion session.

I offer my sincere congratulations to the organizers of what is hoped to be the first of many conferences on the Lamiales.

Barry Conn
National Herbarium of NSW



A.S.B.S. Sydney Chapter

Australian Botanical Liaison Officer

A.S.B.S. Sydney Chapter seminars are held at 6 pm on the 2nd Tuesday of the month (although note the extra July seminar on the 1st Tuesday) in the George Caley seminar room in the Herbarium, Royal Botanic Gardens Sydney, Mrs Macquaries Road, Sydney. Members and non-members are most welcome.

Seminar Program, July-November 1991

Tuesday, July 2nd
Dr Michael Wynne
Department of Biology, University of Michigan
"Generic concepts in a family of red algae (the Delesseriaceae)"

Tuesday, July 9th
Dr Rod Peakall
Macquarie University
"Genetic consequences of pollination: implications for conservation"

Tuesday, August 13th
Mrs Karen Wilson
National Herbarium of New South Wales
"From Acacia to Xeronema - the relationships of the New Caledonian flora"

Tuesday, September 10th
Tuesday, October 12th
to be announced

Tuesday, November 12th
Mr John Benson
National Herbarium of New South Wales
"Status of vegetation mapping in New South Wales"

Further information can be obtained from me on (02) 231-8138 or (042) 21-3440.

Siegy Krauss
Convener

The skyline of Kew Gardens has been recently altered by the addition of a large orange construction crane marking the building site of the new Jodrell Laboratory extensions. The extensions will double the amount of laboratory space at a cost of about $6 million, and it's expected that they will be completed by the end of 1992. A new department of molecular biology will be amongst the Jodrell activities housed in the new building.

I had an interesting involvement with Christie's when they recently offered for auction a four-volume bound collection of 120 herbarium specimens collected in Tasmania. They were allegedly collected by Lady Jane Franklin, the wife of Sir John Franklin, Lt Governor of Tasmania from 1836-1843. When I contacted Tony Orchard, to see if HO were interested in the collection, it transpired that the collection had already been offered for sale in Sydney at a non-negotiable price of $7,000. Tony Orchard advised me that the collection had little or no scientific value, a view supported by Kew staff after examining a sample and some of the background to the collection. Apparently the collection was acquired by a dealer in South America, and there is only a circumstantial connection to Lady Franklin. Christie's had estimated the value at £700-1,000 — certainly a lot lower than the Sydney price. Christie's estimated value was based on the "scientific" value, as they considered it to have little value as a collectable item. The collection went to a dealer for £1,200 (c. $3,000) — it's not known if the dealer was purchasing on behalf of a client. For curators wishing to value their collections, this exercise provides a neat value of £10 per specimen — at least for those old collections without any precise collector, date of collection or specific locality! For the more entrepreneurial curator this is surely a more economical proposition than pulping.

I have had some inquiries from ASBS members wondering what has happened to the "Species Plantarum Project" and whether Australia has any involvement, as nothing has been heard

since the meeting at Kew last November. Time did pass by with apparently little happening, but the Secretariat at Kew has now organised a meeting of the Interim Council to be held in late June, once again at Kew. There was no mandatory representation of geographic areas on the council, but the Secretariat has been asked to co-opt more representatives. Because of interest from Australia, the Secretariat has approached ANPWS to provide a representative for the June meeting. I understand that Alex George has confirmed his attendance.

Things have been fairly quiet on the visitor front. Gillian Perry, Paul Hattersley and Roger Hnatiuk came to the "Improving the Stability of Names" meeting, and Barry Conn came over for the "Advances in Labiatae Science" conference. Paul Ferrar of ACIAR had a brief visit at Kew to discuss ABLO involvement in a project on S.E. Asian weeds, and John Beard spent a few days at Kew researching for a book on *Protea*. Alex George also had an "unofficial" short visit while in the U.K. on recreation leave.

I also turned visitor for a week at Leiden. While I was there M.C. Roos was appointed to lead the "Flora Malesiana" project. We can no doubt expect to see a new style and hopefully an increase in the rate of production of the *Flora*. I will be off to Berlin, Ulm and Zurich from 17-24th June should anybody have requests relevant to these places.

Finally, of course, the weather — unusually cold spells during the early spring which were not appreciated nor, as far as I could ascertain, at all necessary. The spring flowering has been wonderful — confirming for us that there is life after a London winter. However, it looks like another summer with severe water restrictions. Not like Darwin.

Pacific Science Congress Report

Judy West, Ebbe Nielsen and I recently attended a workshop on "Temperate Ecosystems Across the South Pacific", which was held prior to the Pacific Science Congress in Honolulu, Hawaii. The workshop was organized by Peter Raven and Ebbe Nielsen, and included participants from Argentina, Chile, North America, New Zealand, and Australia. The outcome was a particularly successful round of formal and informal meetings, which will lead to concrete research links across the various landmasses.

There was enthusiastic support for a conference in Hobart, probably in January 1993, which will focus on several aspects of southern temperate ecosystems. The exact details of this conference are still to be determined, but it will be sponsored by ASBS. The Ecological Society of Australia and the Palaeobotanical and Palynological Association of Australasia will be approached for co-sponsorship. It is probable that a strong contingent of South American and New Zealand scientists will attend this conference. There will probably be a pre-conference field trip in Tasmania and a post-conference trip in New Zealand.

More details will be published in the next *Newsletter*.

Bob Hill
University of Tasmania

Paper Records

Science **242:** 1130 (1988) reports the results of a number of investigations into how many authors appear on any one scientific publication. A couple of people have commented on this recently, so we though that we might as well reproduce the results, for those of you who haven't seen them yet.

Apparently, the most outrageous attempts come from the users of the JET Tokomak. A paper published in the *International Atomic Energy Association's Conference on Plasma Physics and Controlled Nuclear Fusion*, in 1986, was by 257 authors. However, their best attempt in a peer-reviewed journal was a paper in *Plasma Physics and Controlled Fusion*, which had only 246 authors. These efforts are followed by a paper in *Kansenshogaku Zasshi*, which had 193 Japanese authors from 20 institutions.

Another good source of multi-author papers is *Physical Review D*, which regularly has papers with more than 50 authors, and often more than 100. Their record seems to be a 1987 paper with 113 authors (not 108 as reported in *Science*) from 14 institutions (12 in the U.S.A., 1 in the Netherlands, 1 in Japan).

Other interesting points noted by *Science*, based on the INSPEC database of the Institution of Electrical Engineers in Britain, are:-

Shortest title: -"!".

Longest title:- 45 words.

Longest word in a title:- "Rauchgasentschwefelungsanlagen", which means "flue gas desulphurization".

Most alliterative title:- "Turning transducer triggers technique to tackle transformed torque". This follows the same author's earlier "Layering leisurely liquids leaves linear laminar flow languishing in line".

Shortest abstract:- "No". The title was a question.

Longest mathematical formula:- 16 pages.

Longest editorial delay:- 26 years 5 months. The paper was received by the *Bulletin of the JSME* (a Japanese journal) on April 1st 1960 and finally published in September 1986.

Razors

M. Bunde:- Ockham's razor – like all razors – must be handled with care to prevent beheading science in the attempt to shave off some of its pilosities. In science, as in the barber shop, better alive and bearded than dead and cleanly shaven.

P.J. Darlington:- I do ot trust Occam's razor. To choose the simplest explanation because it is simple is like a surgeon choosing to cut a patient's throat with one razor stroke rather than perform a complex operation. Occam's razor should be used to make an exploratory cut into a problem, not to solve it.

PERSONAL NEWS

Alexander William Jessep (1892-1991)

A.W. Jessep, Director of the Royal Botanic Gardens Melbourne and Government Botanist of Victoria from 27th October 1941 until his retirement on 27th March 1957, died on 20th March, a few days short of his 99th birthday.

Born in Maffra, Victoria, on 27th March 1892, he was educated at Sale Grammar School, Marshall College, Aberdeen, Scotland, and the University of Melbourne. His tertiary studies were interrupted by the First World War, for a period of five years. He joined the 53rd Battery as a gunner and went overseas with the 5th Division, where he saw active service. He ended the war with the rank of Lieutenant. After the war he resumed his studies, and graduated with the degrees of Bachelor of Science, Master of Agricultural Science and Diploma of Education from the University of Melbourne.

Prior to taking up his appointment at the Royal Botanic Gardens, Mr Jessep was the Principal of the School of Agriculture and Horticulture at Burnley, Victoria. Interested in plants and horticulture from his youth, he was widely known for his interest and research into camellias and roses. One of his first duties on assuming the directorship of the Royal Botanic Gardens was to devote a section of the gardens to the growing of camellias.

Over the years he served as President of the Rose Society of Victoria (then the National Rose Society of Victoria) for three terms, and was active in the Royal Horticultural Society of Victoria, the Australian Camellia Research Society and other societies. He authored several horticultural and botanical works, and won a number of awards for his horticultural achievements, among them the E.G. Waterhouse gold medal in 1967 for his contribution to the development of camellias in Australia, and a gold medal from the Royal Horticultural Society of Victoria in 1975 for outstanding service to horticulture.

Mr Jessep retained a strong interest in the Gardens and Herbarium, and until recently still attended some major functions. He never failed to write and acknowledge receipt of his copy of *Muelleria* each year and to congratulate the editor. He is survived by Dorothy, his wife of almost 65 years.

J.H. Ross
National Herbarium of Victoria

David Churchill

David M. Churchill, formerly Director of the Royal Botanic Gardens Melbourne and Government Botanist of Victoria, retired on 9th March 1991, after a period of just under 20 years service in the Victorian Public Service. David is actively continuing his work on an ecological modelling research project until the end of this year.

J.H. Ross
National Herbarium of Victoria

REVIEWS

Tropical Forests: Botanical Dynamics, Speciation and Diversity.

Edited by L.B. Holm-Nielsen, I.C. Nielsen and H. Balslev. Academic Press, New York. 1989. 380pp. ISBN 0-12-353550-6. $95.40.

This book is the result of a symposium held by the University of Aarhus (one of Europe's oldest) in August 1988, to celebrate the 25th anniversary of its Botanical Institute. It is also a lasting tribute to Prof. Kai Larsen, who was closely associated with the foundation of the Institute.

For an institute and a symposium to have a tropical vegetation focus so remote from the tropics is of interest in itself, but equally interesting is the list of contributors, which reads like a 'Who's Who' of tropical botany and ecology. To arrange so many well-known practitioners (e.g. P.H. Ashton, E.F. Brunig, A.H. Gentry, F. Hallé, G.S. Hartshorn, R.A.A. Oldemann, R.M. Polhill, P.H. Raven) in one place and at one time is an achievement in itself. There is wide- ranging material from the old and new tropics.

The hardcover edition of 380 pages is a typically good product of Academic Press. The contents are well-presented and easy to read. The rather poor quality of the few half-tone prints is offset by numerous, very good-quality line drawings. There are quite a few typos, but these in no way detract from the generally careful editing of a set of highly diverse topics. The rather ambitious title of the book is dealt with in four, more-or-less logical sections:- Dynamics, Speciation, Diversity, with a wrap-up on Past, Present and Future.

One might reasonably expect a rather erudite, standard view of tropical forests from such a Festschrift — but the book is anything but status quo. It directly challenges a number of comfortable dogmas — including the myth of forest ecosystems as stable disclimaxes, and the role of Pleistocene refugia in forest composition — and highlights some novel concepts about speciation, among other things. Coming from the land of Raunkiaer, one should not be surprised at the Danish tradition to tackle things laterally — and this is to the reader's benefit.

Following a useful introductory summary by the editors, Roelof Oldemann addresses forest dynamics, including a provocative account of 'Ecological interference' in terms of both stochastic and deterministic factors, which he links with resulting architectural pattern. I found his inferred relevance to silvicultural management a bit hard to accept from a practical perspective. The associated theoretical basis for describing forest architecture is described by Barthelemy, Edelin and Hallé — all well-known for their global work on tree architectural models.

Three separate dynamic treatments of the Amazon basin are given by Irion (Quaternary geological history), Salo and Rasanen (hierarchy of landscape pattern) and Junk (flood tolerance). Brunig and Huang then discuss tree species diversity and canopy structure in Borneo and China, where they assert that in Sabah (under certain circumstances) forest gap formation in connection with mobilization of the seed bank is the driving force in maintaining tree species diversity. Indeed, gap phase dynamics seem to be the order of the day, and Hartshorn derives some interesting conclusions based on studies of natural gaps, which have implications for forest regeneration based on strip cuts.

In a review of population dynamics of tree species in tropical forests, Swaine observes that "Plants perceive gaps very differently from people", and while he compares response differences between two overlapping rainforest species, he points out that these can be easily confused by large disturbances. Swaine's message is a clear warning to those who may be attracted to an anthropocentric view of population dynamics.

Gentry introduces the topic of speciation with an immediate challenge to accepted concepts of rates and processes. In so doing, he provides a startling account of species richness and taxonomic variability from a relatively small locality at Cerro Centinela in western Ecuador, where he speculates that some (orchid) species may have evolved in as little as 15 years in essentially sympatric 'explosive' conditions. In my opinion, this is the book's most provocative chapter, exposing as it does a number of new approaches to theories about Pleistocene refugia, and several alternatives for geographic patterns of speciation. But in relative physiological terms, I found it hard to accept Gentry's generalization that "tropical forests, as a rule, really do have more constant and more benign environments than do temperate zone regions". One only needs to examine plant water use in some of the more seasonal Indo-malesian and Australian

tropical forests to appreciate the incredible variability in seasonal water stress [the point being that you generalize about the tropics at your peril].

A number of papers then describe potential mechanisms of speciation in a variety of tropical growth forms:- these range from Madagascan palms (Dransfield) to *Heliconia* (Andersson); Zingiberaceae in China (Chen); Acanthaceae in the Indo-Chinese Peninsula (Hansen); *Asplenium* (Iwatsuki); Alismatidae in the Neotropics (Haynes & Holm-Nielsen); and patterns of speciation in African Loranthaceae (Polhill).

Ashton's paper on species richness in tropical forests concludes that variation in the frequency and scale of canopy disturbance almost certainly has a leading influence on species richness in clay soils, and that intense drought or other catastrophes may depress richness. In a time of concern about climate change, Ashton asserts that climatic stability may be linked with species diversity. In this and other papers dealing with diversity in this section, it is clear that in order to gain a better grasp of the underlying physical environmental factors governing species diversity, we are abysmally lacking in basic data.

Aspects of plant species diversity are also dealt with by Sumithraachchi (Sri Lanka), Spichiger & Ramella (Paraguayan Chaco), Huber (Venezualan Guayana), Balslev & Renner (Ecuadorean forests), Luteyn (Neotropic Ericaceae), Feuillet (Guianan Passifloraceae), and Mori (Guianan Lecythidaceae).

The final section on 'Past, Present and Future' is a mixed bag that includes:- a discussion on the future of tropical forests (Ortiz-Crespo); a short discourse about Danish botanists in the tropics and tropical botany in Denmark (Kai Larsen); and an extremely interesting and unusual dissertation (by Castrovejo) on Spanish floristic exploration in America. This is followed by a short paper on minor tropical tree crop diversity and its importance for the industrialized world (by Shukala and Nielsen).

Peter Raven's masterly synthesis of the symposium is in itself worth reporting. His primary message concerns the enormous problem facing scientists in dealing with the large number of flowering plant species (perhaps c. 180,000), of which about half occur in Latin America, one-third in Asia, and one-sixth in tropical Africa [Australia is not mentioned — despite occasional claims by various authors that our tropical forests may be the "cradle of the angiosperms"]. Raven urges increases in numbers of professionals to deal with this and related problems:- "At present we are consuming, diverting or wasting some 40% of terrestrial photosynthetic productivity ... we must increase our efficiency of sustainable use".

Among the special problems that Raven poses is the riddle of the evolutionary contribution of refugia, in particular the conditions surrounding the retreat of dryland forests during the last glacial period. He particularly emphasizes the diversity of form in tropical forests, and the species components — [did you know, for example, that most tropical species are epiphytes?]. On species concepts, he argues that "the concept of gene pools affecting entire, widespread species in some mystical way, and holding them together simply, is not supported", and further, "merely repeating outmoded species concepts or applying them on the basis of relatively superficial observation to the situation found in particular groups, may be appropriate at a descriptive level, but it can make no valid contribution to theory." Plant ecologists who, like me, continually probe for an underlying theory to help explain tropical forest dynamics, will find this a refreshing synthesis indeed.

And here's an observation about the perceived role of Australian science in this area:- of the innumerable references from the 30 papers that dealt with broad-ranging topics of tropical botany and ecology, there were only two from Australian authors (L.J. Webb and B.A. Barlow). In fact, the biological relevance of Australian tropical forests, and the general contribution of Australian botanists and ecologists in this theatre, is largely ignored. It is sobering, therefore, that Australian scientists have to make a lot more headway before their presence in tropical vegetation research is recognized by the scientific community at large.

Apart from the plethora of interesting biologicae in this book, an underlying pessimism percolates through these pages. It is that, in the short term at least, we are proceeding at an agonizingly slow pace in achieving a greater understanding of species behaviour in tropical forests. This is no doubt affected by the prospect of continuing rapid deforestation; but equally bleak is the continuing prospect of the lack of trained personnel to cope with the immensity of the problem. In addition to providing a welter of informative and interesting botanical and ecological material for both layman and professional alike, this book succeeds admirably in highlighting these issues in a truly professional manner.

Andy Gillison
CSIRO Tropical Forest Research Centre

Encyclopaedia of Australian Plants Suitable for Cultivation. Volume 5.

By W.R. Elliot and D.L. Jones. Lothian, Melbourne. 1990. xvi+512pp. ISBN 0-85091-329-2. $85.00.

It seems almost characteristic of Australian plant taxonomists that they deny the existence of plants in cultivation, as evidenced by a general absence, in revisions and other accounts, of reference to the horticultural use of species. With the increasing and more discerning interest in Australian plants as garden subjects, there is a greater duty on the part of taxonomists to address this aspect in their treatments, especially where name changes to widely-cultivated plants are involved. In many cases, the most frequent users of names will be horticulturists, and a failure to acknowledge this can bring into question the reviser's thoroughness.

The nursery and gardening trade already (mostly misguidedly) blame a substantial proportion of their ills on the botanical community for incessantly tinkering with the names of the plants they grow — therefore we should be careful to avoid adding indignation to their ire by ignoring their justified claims for information. For example, drawing attention to the widespread misapplication of a name in ornamental horticulture generally requires a minimum of additional effort by the monographer, but is of considerable help not only to the horticulturist but also those botanists identifying cultivated plants. This whole process has become much easier with the appearance of the *Encyclopaedia of Australian Plants*, a work that should be consulted not only by those botanists who happen to have an interest in gardening but also in the course of taxonomic studies.

The fifth volume of the *Encyclopaedia*, covering genera and other headings from Gr to J, is of the high standard that has come to be expected of this work. The coverage is comprehensive, the text is well-written in clear, concise English, and the setting-out is logical and consistent.

A surprising number of genera and species are covered, with not only the plants established in cultivation treated, but also those grown chiefly by enthusiasts or having potential in horticulture. Therefore, as well as the expected genera *Grevillea*, *Hakea*, *Helichrysum* s.l., *Hibbertia*, *Hibiscus*, *Hovea*, *Ipomoea*, *Isopogon*, *Jasminum* and many more, each represented by a substantial portion of their (Australian) species, others like *Guettarda* and *Guettardella* (Rubiaceae), *Gunniopsis* (Aizoaceae), *Haloragis* (Haloragaceae), *Halosarcia* (Chenopodiaceae), *Harmsiodoxa* (Brassicaceae) and *Hensmania* (Liliaceae) are included.

There are also useful entries under family names and general horticultural subjects such as Grasses, Groundcovers, Herbaceous Plants, Hedge Plants, Indoor Plants and Insectivorous Plants, together with cross-referencing of synonyms. All of this, when an account of *Grevillea* alone, with all of its problems of numerous taxa, differing species concepts and endless suites of marginally differing cultivars, is a mammoth achievement!

In addition to a description, an overview of eco-geographic occurrence and an account of horticultural aspects for each genus, each species treated is considered in full detail. As many systematists whose groups have been covered in successive volumes will be aware, one of the strongest recommendations for this publication is that the taxonomy presented reflects the most up-to-date concepts available at the time of compilation. The authors spare no effort in contacting specialists (who are acknowledged at the beginning of each volume) to ensure that this is the case. The species descriptions are concise, and include important distinguishing features. Useful notes on how morphologically similar species can be separated are also given, where relevant. Detailed notes on distribution, habitat, soils and horticultural aspects are provided.

The standard of presentation is high, with numerous high-quality coloured plates, line illustrations (by Trevor Blake), running heads, and 8-point type set out in 2-column format. A glossary, further reading list and index to common names are also provided.

Faults are few and far between. Very few of the photographs could be criticized, though one might be that of *Hakea grammatophylla* showing red rather than pink flowers (p. 210). Typographical errors are rare, as are mis-spellings of botanical names, sometimes all too common in horticultural texts. In noting the spelling *meisnerana* (instead of *meisneriana*) under *Hakea* in the main entries, I am well aware that this is a case where changes in the ICBN have actually caused confusion. The spellings *Hakea suleata* and *H. subsuleata* for *H. sulcata* and *H. subsulcata* in the caption of one of the less-useful line drawings appears to have resulted from mis-interpretation of handwriting.

A point of greater concern is the use of double quotation marks for cultivar names that are not registered. This practice is clearly incorrect; single quotation marks should be used for all valid cultivar names. While registration is undoubtedly desir-

able and to be encouraged, cultivar names need only be published to be valid. Another method should be used to indicate registered cultivars, with double quotes reserved for names of doubtful standing. The copious Further Reading list provided at the end of the volume might have been improved by grouping titles by subject areas. Clearly, the criticisms are all minor considering what has been accomplished.

All in all, systematists will find this a valuable addition to their libraries, whether as a useful reference in their research, as a source of excellent and widely-available illustrations to cite in flora and other treatments, or, for those who are gardeners, as the definitive text on cultivated Australian plants.

Laurie Haegi
Botanic Gardens of Adelaide

History of Systematic Botany in Australasia.

Edited by P.S. Short. Australian Systematic Botany Society, Melbourne. 1990. v+326pp. ISBN 0-7316-8463-X. $50.00 ($40.00 for members).

This is a well-presented volume, with A4-size pages, and a hardcover adorned with an attractive illustration of *Correa baeuerlenii* F.Muell. by artist Anita Barley of the National Herbarium of Victoria. Wilhelm Baeuerlen, for whom this *Correa* was named, is but one of a number of little-known botanical collectors whose stories are given in the book.

The subtitle in the frontispiece states "Proceedings of a symposium held at the University of Melbourne, 25-27 May 1988". You can therefore be excused for being amazed that one paper (by Barker & Barker) takes up almost one-sixth of the text (50 of the 320 pages), leaving the other 35 papers to share the remaining 270 pages. However, Philip Short explains in his preface that while some authors presented their papers for inclusion in the volume in much the same format as delivered at the conference in 1988, others chose to enlarge on their conference manuscripts. In addition, some of the papers are expansions of conference posters. Hence the unevenness in presentation.

Robyn and Bill Barker's paper "Botanical contributions overlooked: the role and recognition of collectors, horticulturalists, explorers and others in the early documentation of Australian flora" has been so substantially enlarged as to warrant a separate paper elsewhere. They have drawn together information from many sources, and reviewed a selection of published literature. Much useful material is presented that could save later researchers many long hours of work. Two examples illustrate this point:- Table IV "Some generic, sectional or species group names in Robert Brown's (1810b) Prodromus for which the authorship is disputed and sometimes an alternative name provided by R.A. Salisbury ...", and Appendix "Differences in pagination in 1838 and 1839 editions of Major T.L. Mitchell's *Three expeditions to the interior of eastern Australia*".

Barker & Barker also put in a plea for the lesser-known — the ordinary people in the background who did the hack-work for the important people in the forefront. Such people included the labourers, the convicts and the aborigines, some not even mentioned by name, who found and collected the specimens for the 'gentlemen', but whose names find no place on herbarium labels.

Some collectors were paid good money for their efforts. Barry Conn, in his paper on Mary Strong Clemens' collections in New Guinea, queries if all localities recorded on such specimens are accurate. If a collector is paid more for a specimen collected at say 2,000 m than one at 1,800 m, is it not conceivable that the collector preferred to record more from 2,000 m?

Within the topic, the scope of the book is broad. The largest section, Australian botanists and collectors, deals almost exclusively with phanerogams. Other sections are on cryptogams, with one paper each on freshwater algae and bryophytes, and there are also two papers each discussing New Guinea and New Zealand botany. Papers on certain aspects of Australian herbaria, botanical art, cultivated plants, and a bibliography are also included. However, the claim made in the flier that it is the definitive work is not justified. There is no overview, and many aspects remain to be covered.

The index is restricted to people and ships. The inclusion of an index to localities and botanical names would be useful for future workers, as many type localities and type specimens are cited.

Alex George states in "History is now" that we should be looking further at keeping not only journals and diaries but also notes with dates of telephone calls, personal notes taken at meetings, and even "doodles should also be kept". Heaven forbid! We need to recycle some paper! Storage of paper records is already a problem. Doodles are

valuable? I recall a former colleague, no longer with us, who was a great doodler and scribbler of notes. Should we have kept all of her bits of paper and all of her notes (which she carefully signed "C M E")?

A huge amount of sleuth work has gone into the preparation of many of the papers, and I recommend the book as good value for the money.

Enid Robertson
Sherbourne Road, Blackwood

Recent Publications

Introduction to the Principles of Plant Taxonomy. Second edition.
By V.V. Sivarajan, ed. by N.K.P. Robson. Cambridge University Press, Cambridge. 1990. 384pp. ISBN 0-521-35679-2. $40.

Australian Plants Identified.
By Gwen Elliot. Hyland House, South Yarra. 1990. ISBN 0-947062-63-7. $24.95.

Plant Molecular Systematics: Macromolecular Approaches.
By Daniel J. Crawford. Wiley, New York. 1990. 388pp. ISBN 0-471-80760-5. $60.

Quantitative Approaches to Phytogeography.
Ed. by P.L. Nimis and T.J. Crovello. Kluwer, Dordrecht. 1990. 304pp. ISBN 0-7923-0795-X. $240.

Plant Taxonomic Literature: Bibliographic Guide.
Ed. by James A. Mears. Chadwyck-Healey, Cambridge. 1990. 177pp. ISBN 0-85964-217-8. $240. A printed guide to the microfiche edition of 4,679 taxonomic works cited in the second edition of *Taxonomic Literature*.

Plant Names: A Guide to Botanical Nomenclature.
By Peter Lumley and Roger Spencer. Royal Botanic Gardens, Melbourne. 1990. 51pp. ISBN 0-7306-0615-5. $6.75

Melastomataceae.
By Edward P. Klucking. Borntraeger, Stuttgart. 1989. 283pp. ISBN 3-443-50004-8. $230.

Computer Presentation of Data in Science: A Do-it-Yourself Guide, Based on the Apple Macintosh, for Authors and Illustrators in Science.
By Doig Simmonds and Linda Reynolds. Kluwer, Dordrecht. 1989. 178pp. ISBN 0-89838-415-X. $60.

Molecular Systematics.
Ed. by David M. Hillis and Craig Moritz. Sinauer Associates, Sunderland. 1990. 588pp. ISBN 0-87893-279-8. $50.

Index Herbariorum Part I: The Herbaria of the World. Eighth edition.
Ed. by P.K. Holmgren, N.H. Holmgren and L.C. Barnett. New York Botanical Garden, New York. 1990. 693pp. ISBN 0-89327-358-9. $130.

NOTICES

Conference and Workshop on Conservation Biology

Conservation Biology in Australia and Oceania

The Centre for Conservation Biology at the University of Queensland and the Queensland National Parks and Wildlife Service are hosting a conference on the above topic, in the first week of October this year. The conference will run for five days from 30th September to 4th October, with four days of symposia, workshops and evening events, and a day off in the middle for excursions to field sites.

The conference will provide an exciting opportunity for researchers, conservation officers and policy makers to draw on the experience and expertise of others in solving the problems of biological systems management. This is an opportunity for a wide perspective of opinion and experience to be focussed on planning and research into and management of our native species and ecosystems. We are confident that the recommendations arising from the conference will be guidelines for research

and management during the remainder of the decade.

The programme involves key people from Australia, New Zealand and other parts of the Pacific region whose job it is to organize and carry out conservation research and management. The keynote speakers at the conference are Peter Ashton and Daniel Simberloff, while the plenary speakers are Andrew Burbidge and Peter Bridgewater. The theme presentation is by Harry Recher.

The symposia will review progress and gaps in understanding of key issues, in a regional context, through invited addresses and contributed papers. Sessions include:- Habitat fragmentation, Land degradation and restoration, Introduced species, Taxonomy and genetic resources, Ecosystem conservation and reserve design, Conservation biology in Oceania, and Marine conservation.

The aim of the workshops is to address practical aspects of management and research. It is hoped to define major research objectives and to establish an efficient means of co-ordinating research and management. Workshop sessions include:- Targeting species for research, The role of captive breeding, Population ecology and genetics, Commercial use of wildlife for conservation, Research policy and funding, Translocation and close order management, Biological surveys and reserve design, and Implementation of research results.

There will be an evening devoted to talks by representatives of non-government organisations, including industry and conservation groups. The conference proceedings will be published as a special publication.

For further information contact:-

Peter Hale
Centre for Conservation Biology
University of Queensland. Qld. 4072.
Tel (07) 365 2527
Fax (07) 371 0057

Gordon Grigg
Centre for Conservation Biology

TDWG 7 Conference

The IUBS Commission for Plant Taxonomic Databases (TDWG) is holding its Seventh International Workshop in Canberra, where it will be hosted by the Australian National Botanic Gardens on 21st-22nd September 1991.

In conjunction with TDWG, a one-day symposium will be held on 23rd September, on "The role of specimen-backed information in environmental decision making".

The workshop fee is $60 for TDWG members and $100 for non-members. The symposium fee is $40. There will be a conference dinner on 21st, for $35 per person.

For further information, contact:-

Mr Alan J. Bray
Australian National Botanic Gardens
TDWG 7 Conference
G.P.O. Box 1777
Canberra. A.C.T. 2601.
Tel (06) 250-9501
Fax (06) 250-9599

Alan Bray
Australian National Botanic Gardens

Workshops

The Second Australasian Bryophyte Workshop will be held at the Australian National Botanic Gardens (ANBG), Canberra, from 26th September to 1st October 1991.

Bryologists attending will be from Australian and overseas, with a strong New Zealand contingent. The workshop will consist of field work, laboratory work, and evening talks.

The workshop is being organized by Heinar Streimann (06-250-9464) and Judith Curnow (06-250-9461) of the ANBG's Cryptogamic Herbarium, who can be contacted for more details.

The Australian National Parks & Wildlife Service Endangered Species Unit is proposing a two-day crytogamic conservation workshop in Canberra later this year, to discuss the status of cryptogams.

It is expected that this workshop will be on the two days before the Bryophyte Workshop, or alternatively the two days following it.

The crytogamic conservation workshop is being organized by John Hicks (06-250-0281), who can be contacted for more details.

Judith Curnow
Australian National Botanic Gardens

Request for material

We are in the process of re-assessing the Australian material of *Strychnos* (Loganiaceae). Many herbarium specimens do not have any information on habit, i.e. vine, shrub, tree, producing long cones, presence or absence of tendrils, etc. We would be extremely interested to hear from anyone who can supply information, even anecdotal, on the various taxa and how they develop through time. We would also be interested in obtaining more specimens (live and pressed), cuttings and seeds for study.

B.J. Conn and E.A. Brown
National Herbarium of New South Wales

Society of Systematic Biologists

In the largest vote in the their history, the members of the Society of Systematic Zoology (in the U.S.A.) recently voted in favour (8:1) of changes to their constitution, involving the name of their society and of their journal (*Systematic Zoology*). As of 1st January this year, the society is the Society of Systematic Biologists, and as of next year (Volume 41) the journal will become *Systematic Biology*. These changes affect neither the aims of the society nor the content of the journal; they are merely nomenclatural changes.

The American Society of Plant Taxonomists, and their journal *Systematic Botany*, remain unchanged. This means that, instead of having both zoological and botanical systematic societies, the U.S.A. now has one for botanists and one for biologists - a very pretty distinction. Similarly, instead of journals for botany and zoology, they have journals for botany and biology. It is unclear whether the general public and the politicians will understand these distinctions.

Software Licences

For those of you who have never read the fine print in computer software licence agreements, the following licence is part of a flow-charting package for programmers called "Interactive EasyFlow", from the U.S. company Haventree:-

"We don't claim Interactive EasyFlow is good for anything — if you think it is, great, but it's up to you to decide. If Interactive EasyFlow doesn't work, tough. If you lose a million because Interactive EasyFlow messes up, it's you that's out a million, not us. If you don't like this disclaimer, tough. We reserve the right to do the absolute minimum provided by law, up to and including nothing. This is basically the same disclaimer that comes with all software packages, but ours is in English and theirs is in legalese."

Bertil Nordenstam:- Clearly, to be superior to already existing classifications, a new taxonomy has to be based on a wealth of information from as many sources as possible. The accumulated data have been processed in my mind, and whenever necessary, checked, supplemented and integrated with cytological and phytogeographical information — a taxonomic procedure cataylzed by experience and intuition, and in my opinion sometimes superior to the rigid treatment of data by a computer.

Telephone and Fax Numbers for Major Australian Herbaria

International dialling sequence from outside Australia:- add the Australian country code 61 and omit the leading zero of the area code.

AD	BRI
Ph: 08 2282311	Ph: 07 8779325
Fax: 08 2231809	Fax: 07 3716655
CANB	CBG and Flora of Australia
Ph: 06 2465113	Ph: 06 2509450
Fax: 06 2465249	Fax: 06 2509599
DNA	FRI
Ph: 089 894516	Ph: 06 2818211
Fax: 089 323849	Fax: 06 2818312
HO	MBA
Ph: 002 202635	Ph: 070 921555
Fax: 002 202186	Fax: 070 923593
MEL	NSW
Ph: 03 6552300	Ph: 02 2318111
Fax: 03 6552350	Fax: 02 2514403
PERTH	QRS
Ph: 09 3670500	Ph: 070 911755
Fax: 09 3670515	Fax: 070 913245

This list will be kept up to date, and will be published periodically. Please inform us of any changes or additions.

The Society

The Australian Systematic Botany Society is an incorporated association of over 300 people with professional or amateur interest in botany. The aim of the Society is to promote the study of plant systematics.

Membership

Membership is open to all those interested in plant systematics. Membership entitles the member to attend general meetings and chapter meetings, and to receive the *Newsletter*. Any person may become a member by forwarding the annual subscription to the treasurer. Subscriptions become due on January 1 each year.

The Newsletter

The *Newsletter* appears quarterly, and keeps members informed of Society events and news, and provides a vehicle for debate and discussion. In addition, original articles, notes and letters (not exceeding ten published pages in length) will be considered.

Contributions should be sent to one of the editors at the address given below. They should preferably be submitted as an unformatted word-processor or ASCII file on an MS-DOS or Macintosh diskette accompanied by a printed copy, or as two typed copies with double-spacing.

The deadline for contributions is the last day of February, May, August, and November.

All items incorporated in the *Newsletter* will be duly acknowledged. Authors alone are responsible for the views expressed, and statements made by the authors do not necessarily represent the views of the Australian Systematic Botany Society Inc. *Newsletter* items should not be reproduced without the permission of the author of the material.

Notes

ASBS annual membership is $22 (Aust); full-time students $12. Please make cheques out to ASBS Inc., and remit to the treasurer. All changes of address should be sent directly to the treasurer, as well.

Advertising space is available for products or services of interest to ASBS members. Current rate is $100 per full page, $50 per half-page or less. Contact one of the *Newsletter* editors for further information.

Editors

Ms B.M. Wiecek

National Herbarium of NSW
Royal Botanic Gardens
Mrs Macquaries Road
SYDNEY NSW 2000
Tel (02) 231 8150
Fax (02) 251 4403

Dr D.A. Morrison

Department of Applied Biology
University of Technology, Sydney
PO Box 123
BROADWAY NSW 2007
Tel (02) 3304159
Fax (02) 3304003

Cover

David Mackay

Austral. Syst. Bot. Soc. Newsletter 67 (June 1991)

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