AUSTRALIAN SYSTEMATIC BOTANY SOCIETY
NEWSLETTER NUMBER 28 (1981)

AUSTRALIAN SYSTEMATIC BOTANY SOCIETY

NEWSLETTER

Newsletter No. 28 August, 1981.

The Newsletter is the official publication of the Australian Systematic Botany Society.

A.S.B.S. COUNCIL

President	:	Dr Trevor Clifford, Department of Botany, St Lucia, Queensland, 4067.
Vice-President	:	Dr Bryan Barlow, Herbarium Australiense, C.S.I.R.O., P.O. Box 1600, Canberra City, A.C.T. 2601.
Secretary	:	Ms Judy West, Herbarium Australiense, C.S.I.R.O., P.O. Box 1600, Canberra City, A.C.T. 2601.
Treasurer	:	Mr Barry Conn, Department of Botany, University of Adelaide, Box 498, G.P.O., Adelaide, S.A. 5001.
Councillors	:	Mr Laurie Haegi, National Herbarium of N.S.W., Royal Botanic Gardens, Sydney, N.S.W. 2000.
		Mr Rod Henderson, Queensland Herbarium, Meiers Rd, Indooroopilly, Queensland, 4068.

CONVENERS OF LOCAL CHAPTERS.

ADELAIDE	:	Dr Hellmut Tölken
ALICE SPRINGS	:	Mr John Maconochie
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CANBERRA	:	Dr Helen Hewsen
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SYDNEY	:	Dr Jocelyn Powell
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Affiliated Society : Papua New Guinea Botanical Society.

INFORMATION FOR CONTRIBUTORS

The Newsletter is produced four times each year and deadlines for copy are the last day of February, May, August and November.

Please send contributions, preferably typed in duplicate and double-spaced, to the Editor, at the address below. Items from any source and of interest to members are acceptable. All items incorporated in the Newsletter will be duly acknowledged.

Please note: Next deadline for articles is 31 November, 1981.

Editor
Barry Conn
Department of Botany
University of Adelaide
Box 498, G.P.O.
Adelaide, S.A. 5001.

SUBSCRIPTIONS

Subscriptions for 1981 were due on the 1st January. Both Australian and overseas members:

Aust. $8.00 if paid by 31st March
Aust.$10.00 thereafter.

Barry Conn
Treasurer.

MINUTES OF THE 7TH GENERAL MEETING OF THE AUSTRALIAN SYSTEMATIC BOTANY SOCIETY, SYDNEY, 24TH AUGUST 1981.

The 7th General meeting of the Australian Systematic Botany Society was held in the Carslaw Building, University of Sydney with the Vice President in the chair and about 50 members present.

Bureau of Flora and Fauna / Flora of Australia

(This item was placed first on the agenda as Dr McCusker had to leave the meeting early).

The Acting Director of the Bureau, Alison McCusker presented a brief report on the activities of B.F.F. During the year B.F.F. was transferred from the Commonwealth Department of Science and Environment to Home Affairs and Environment, Alex George was appointed Executive Editor of the 'Flora of Australia', Helen Hewson as Flora Writer and Roger Hnatiuk as the Scientific Officer in charge of the Biotaxonomic Information Section. Due to current staff ceiling restrictions the second Flora Writer will not be appointed in the near future. However, the Bureau has "significant staff to enable us to get on with the Flora".

The Australian Biological Resources Study has received an increase of 32% in funding in the recent budget allocations.

The first volume of the 'Flora of Australia' is now published and Volume 29 on the Solanaceae is expected to be published at the end of this year.

Jim Armstrong requested that more news and information on the activities of B.F.F. be included in the A.S.B.S. Newsletter in future. Alex George said the Bureau intends to publish its own newsletter soon.

Minutes of the 6th General Meeting accepted as published in the Aust. Syst. Bot. Soc. Newsletter 23 (1980): 2-7.

President's Report.

(This Report was read by the Vice President, Roger Carolin).

I must apologise both for my not attending this Meeting and for probable omissions from this report. I write it in bed with flu and without access to scattered notes which no one but I could possibly find.

It has not been an innovative year, but it has been a successful one in several ways.

The Newsletter has flourished under two editors — Alex George and Barry Conn. While there are many moments when an editor wonders how he will fill the next issue, somehow something good always has turned up — or has been made to turn up!

Philippa Nikolinsky's poster financed by the Northern Territory Government as a result of Andrew Mitchell's efforts attracted much interest and favourable comment.

The Flora of Central Australia, for all its admitted shortcomings, actually appeared on time. The more than seventy contributors deserve a special thanks for their tolerance and co-operation. The editorial committee's hard work is not as prominently acknowledged as they deserve. I must particularly single out Roger Carolin, who took over the majority of the proofs which arrived while I was absent overseas, and Alex George who organized the index. The Commonwealth, Western Australian, Northern Territory, Queensland, New South Wales and South Australian Governments combined to provide a $20,000 subsidy. They and Reeds must also be remembered for helping to make it possible, especially at so low a price. The last report I heard was that it was selling well and might have to be reprinted very shortly. The Society will receive royalties on the reprint but not on the first printing. I shall be prepared to receive and collate notification of errors and omissions, if the Society so wishes, for future editions.

I have a suspicion that some of the once active regional sections have been holding fewer and/or less well-attended meetings. I have been impressed by the length of time over which the early activity has been maintained. There is a limit to the number of speakers and topics likely to attract good attendances, but I hope regular, if not necessarily so frequent, meetings will continue to be held. The conveners task is not an easy one and their contribution is very greatly appreciated. Without these meetings, even if only a few each year, the Society would be greatly weakened.

There is only one short-term project, but an important one currently in progress, and that is the published form of the "Arid Flora and Fauna" Symposium of our last meeting. Bill Barker has succeeded in obtaining revised manuscripts — all but one of which was being typed when I last heard from him. Problems with finding the most appropriate publisher should be overcome shortly.

I must end up by thanking the members of Council and others whose efforts have continued to ensure the success of our Society. In particular I want to pay special tribute to Judy and Barry. In wishing my successor a productive new year, let me assure him that he could not ask for more support than he will get from these two.

A vote of thanks was proposed to John Jessop for editing and completing the 'Flora of Central Australia'.

Treasurer's Report

Barry Conn presented the Treasurer's Report for the period January 1st, 1981 - August 18th, 1981.

Credit		Payment	
Balance brought forward	2577.00	Newsletter 25 (December 1980 issue)	413.86
Subscriptions	1752.71	Newsletter 26 (March 1981 issue)	608.96
		Newsletter 27[2] (May 1981 issue)	288.38
Donations to N.T. Burbidge Memorial Lecture	15.00	A.H. & A.W. Reed	19242.00[1]
Donations to the "Central Australian Flora" Project	21000.00		
A.S.B.S./I.B.C. Congress Dinner	1525.00		
		Total Expenditure:	$20553.20
		Bank Balance (18.viii.1981)	$ 6316.51[3]
Total Income:	$26869.71	Total Balance	$26869.71

Comments.

1. For Typesetting, bookcraft, for the "Flora of Central Australia" Project.
2. Newsletters numbers 25, 26 and 27, average cost $437.06 per issue. Total of 110 pages produced.
   Newsletters numbers 22, 23 and 24 average cost $191.83 per issue. Total of 55 pages produced.
3. $2500.00 of this bank balance has been deposited in an Interest Bearing Deposit Account at 12% rate of interest. This account matures on the 23rd December, 1981. Therefore, two accounts are operating at present.
4. Number of Members = 327
   Financial Members = 185 (142 unfinancial)
   $1658 outstanding.

5. In the December (1980) issue of the Australian Systematic Botany Society Newsletter I invited opinions on the need for financial support for local chapters. I did not receive any comments. Subsequently I have discussed this issue with a few Conveners and all of those contacted felt that there was no need for financial support, except in special circumstances. On these occasions, such requests for assistance are referred to council for consideration.

The report was accepted (moved Arthur Chapman, seconded David Frodin) with little discussion.

Newsletter Editor's Report.

Since the last Annual General Meeting (the 6th A.G.N. held in Adelaide, 14th May, 1980) there have been five issues of the Australian Syst. Bot. Soc. Newsletter (numbers 23-27). A total of 149 pages were produced.

Dr. A.S. George retired as Editor of the Newsletter after issue number 25 (December 1980). Alex had been editor of the Newsletter since 1978 (issue number 15). On behalf of the Society, I wish to thank him for the time and effort which he freely gave to maintain and promote the Newsletter as a regular form of communication between members.

I have only made some minor changes to the format of the Newsletter (Numbers 26 and 27).

The Newsletter is still produced in Perth (W.A.). There are certain problems with this arrangement, especially when more complex formating is required (e.g. issue number 26). However, these problems are relatively trivial and probably will resolve themselves in due course. As pointed out in the Treasurer's Report, production costs have remained stable.

The Newsletter is an extremely useful means of communication between all members whether they be in cities or small country centres. For this communication to be maintained and developed, the Newsletter must strive to present a range of material which is topical, relevant and academically stimulating.

It must be remembered that for some members the Newsletter is the only form of contact with the taxonomic community of Australia (and even, overseas). On occasions, I feel that we have not met the needs of these people.

I hope that all members will seriously consider the role that they can play to further develop the Newsletter. The Newsletter does reflect the amount of interaction between members and the overall activity of the Society. Since we are a group of professional scientists, I personally would like to read articles which present some of the excitement and controversy of our various fields of study.

Unfortunately, far too many members have not received their copies of the Newsletter. This is a problem which has proved difficult to resolve. The address list was updated, corrected and retyped last month. However, in the last three weeks, there have been many changes. Therefore, it is out of date already! Could I suggest that members choose the most stable address for the receipt of the Newsletter? This will relieve some pressure off an obviously inefficient system. May I assure those members who have not received their copies of the Newsletter that the changes to the addresses are always sent to the Printer with every issue.

Finally, I wish to thank all those who have continued to contribute to the Newsletter.

Barry Conn
24th August, 1981.

(The Report was accepted (moved David Symon, seconded Rosemary Purdie).

Flora of Central Australia.

Roger Carolin thanked all contributors for their efforts in production of the Flora. It was proposed that the Society write to John Jessop thanking him for his very significant role in the initiation of, his contributions to, and editing of the Flora.

Bob Johnson complimented the Society on the publication of such a flora.

Future A.S.B.S. Meetings.

1) Brisbane 1982.

Laurie Jessup reported that the Brisbane Chapter is planning a one-day symposium (probably a Saturday) with the suggested topic of "The origins and evolution of the flora of northern Australia". It is proposed to hold the meeting in May 1982 and possibly to combine it with a Sunday field trip.

2) Perth 1983.

Greg Keighery reported that Neville Marchant is acting as convener for a meeting to be held in Perth at the same time as ANZAAS in 1983.

Academy Flora Committee.

Jim Armstrong outlined the origin of this committee and reported on its lack of activity. Despite the opportunity of most members being in Sydney for the International Botanical Congress no meeting was organised at that time.

As the Society's representative on this committee Jim requested some guidance on the role that it should be playing. Discussion included such suggestions as monitoring the production of the 'Flora of Australia', and encouraging the revisional aspects of taxonomic research.

Thesis List

The Council is investigating the feasibility of producing a combined thesis list with the Ecological Society of Australia. The discussions at present suggest publication in microfiche form with availability on request.

Next General Meeting and Incoming Council

The next General Meeting will be held in Brisbane in May 1982.

The Council for 1981-82 (the executive elected unopposed).

President	:	Trevor Clifford
Vice President	:	Bryan Barlow
Treasurer	:	Barry Conn
Secretary	:	Judy West
Councillors	:	Laurie Haegi; Rod Henderson.

A vote of thanks to the outgoing Council was proposed by Arthur Chapman and seconded by Laurie Haegi.

MINUTES OF AUSTRALIAN SYSTEMATIC BOTANY SOCIETY COUNCIL MEETING, SYDNEY, 25TH AUGUST, 1981.

The following report of the above Council Meeting only includes those issues which were not discussed at the 7th General Meeting of the Society.

— Editor, B.J. Conn.

Present:

B. Barlow, T. Clifford, B. Conn, L. Haegi, R. Henderson, J. West.

Flora of Central Australia:

Bryan Barlow suggested that some of the remaining posters be sent to local SGAP Chapters. It was assumed that all museums and relevant institutions received copies earlier.

Trevor Clifford is to write to John Jessop on behalf of the Society to thank him for the time and effort he put into the production of the 'Flora of Central Australia'.

Newsletter:

Advertising in the Newsletter — Advertisements for relevant books will be included in the Newsletter with the approval of the Council. The charges to be decided in each situation.

Bureau of Flora and Fauna Newsletter — If the proposed B.F.F. Newsletter (see A.G.M. Minutes) is published, then approval should be sought to include relevant items from their newsletter in the A.S.B.S. Newsletter.

Thesis List:

The Ecological Society of Australia is keen to produce a combined thesis list with A.S.B.S.

Judy West presented figures and approximate costs involved with production of this larger thesis list in printed form. Estimating conservatively a list of 1,000 thesis titles (A.S.B.S. already has approximately 450) would cost about $2,000 to publish. Judy and Dr Andy Gillison (E.S.A. President) are investigating putting the entries on computer and producing the list in microfiche form in cooperation with the B.F.F. It is likely at this stage that the list would be available on request (with a small charge) rather than being sent to all members.

I.B.C. Posters:

Roger Carolin had suggested that the posters from the I.B.C. Symposium "Origins and diversification of the Australian flora" be circulated to A.S.B.S. Chapters. After discussion Council decided instead to prepare an audiovisual kit of the posters. Judy is to ask all participants to take slides of their poster and to prepare a short tape to guide an audience through their contribution.

A.W.R.C. REVIEW OF WATER RESEARCH IN AUSTRALIA.

The Australian Water Resources Council has established a working group to review water research in Australia. Its terms of reference are to report on:

.. The adequacy, effectiveness and overall balance of the national effort in water research;

.. the assessment of gaps and overlaps in water research in Australia;

.. the need for concentrated research efforts in those areas having particular national significance;

.. the means of improving efficiency in the use of resources involved in water research; and

.. development of appropriate institutional arrangements.

The working group is seeking from interested parties, submissions which should be forwarded to:

Ms Penny Le Couteur, Department of National Development and Energy,
P.O. Box 5, Canberra, A.C.T. 2600 (Phone 062 458634)

by 30 October 1981. Ms Le Couteur is also able to provide further information if required.

CHAPTER NEWS - CANBERRA

INFLORESCENCE WORKSHOP

At the October 1980 Meeting of the Canberra Chapter a workshop entitled "Plant Taxonomy - the users point of view" was held in place of the usual one Speaker. Four botanists (David Coates, Ian Noble, Bruce Wellington and Nigel Wace) who are not taxonomists, but who use floras and keys to identify plants as part of their work, were the speakers. Each spoke briefly about the adequacy of modern taxonomic treatments for their purposes as users.

A fruitful and lively discussion followed and amongst other aspects the question "What is a species?" was asked. This workshop was regarded as successful and the concept of workshopping thought to be worth attempting once or twice a year.

In the past year or so several of us found that independently we were having trouble with inflorescence analysis and terminology, and so an "Inflorescence Workshop" was organised for the July 1981 Meeting. Lawrie Johnson and Barbara Briggs managed to skip between raindrops and industrial trouble and came to Canberra to join in. They each provided an especially valuable contribution to the Workshop because of their recent monumental works on inflorescence structure in the Proteaceae (Johnson and Briggs, 1975) and the Myrtaceae (Briggs and Johnson, 1979). All of us had had trouble with interpretation of Troll (1964, 1969) and several of us had attempted to do battle with Briggs and Johnson, but were not confident that we had done so successfully.

The discussion was introduced by Barbara, who led us into contemplation of inflorescence structure as exemplified by the Myrtaceae. Helen Hewson discussed the inflorescence of *Lepidium* (Cruciferae) in New Guinea - the inflorescence is terminated by a spine; Alex George illustrated some of the variations in the inflorescence of *Banksia* (Proteaceae) with special emphasis on phyllotaxy and anthesis; Andrew Kanis discussed inflorescence structure of Ochnaceae - Andrew had had the benefit of having worked with Weberling; a student of Troll; (Weberling, 1965, and more recently, Weberling, 1981); Michael Crisp discussed and illustrated variation in the inflorescence structure of *Brachysema* (Leguminosae); Judy West discussed inflorescence structure in *Dodonaea* (Sapindaceae) - Judy (West, 1980), applied the "Briggs and Johnson approach" to inflorescences with spiral phyllotaxy (in contrast to Myrtaceae which has decussate phyllotaxy); Bryan Barlow discussed and illustrated inflorescence structure in Loranthaceae - Bryan presented plenty of evidence of phylogeny by reduction; Ian Telford discussed and illustrated some recent observations which he has made on Cucurbitaceae - this inflorescence work is indicating some taxonomic problems which need resolution; and finally Lawrie Johnson synthesised the variety of observations made by the speakers. Emphasis was laid upon the necessity for utmost care in establishing whether the inflorescence axis terminates in a bud or not; and upon the fact that although the typological basis of the Troll system was rejected by Briggs & Johnson, much of his descriptive work is retained.

Significant observations made included: — that there is a measure of flexibility in many examples. Conn (1980) reached similar conclusions. He proposed that the primitive inflorescence type of the Loganiaceae was flexible in both position and type, with a subsequent stabilisation of both these aspects; — the more reduced the inflorescence the lower the level of flexibility; — phylogenetic trends most frequently moved from the complex to the simple (most speakers had evidence of reduction series). A discussion of the application of descriptive terminology for use in floras and revisions ensued. No conclusions were drawn but it was made abundantly clear that ambiguity leads to ineffective communication and where there is a possible doubt — define.

Helen Hewson,
Canberra.

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* References added by Editor.

SYSTEMS OF CLASSIFICATION OF FLOWERING PLANTS*

by A. Cronquist.

The New York Botanical Gardens, Bronx, New York, 10458, U.S.A.

In talking to you tonight about systems of classification of flowering plants, I think it is only fair to start out by explaining my view of the function of taxonomy, and how that function may most effectively be performed. If any of you disagree with me, as some well might, we should at least be able to determine how fundamental the disagreement is, and at what point it begins.

In my view the function of taxonomy is to permit us to understand and remember what we know about organisms, to communicate with others about that knowledge and understanding, and to provide some guidance in our search for new data and interpretations. To this end, we try to establish a scheme that best reflects the totality of similarities and differences among organisms. We put together into a species or infraspecific taxon the things that are the most alike in all respects, and we group these basic taxa into progressively larger groups according to the similarities and differences we perceive.

As I have said in another place, it is perfectly clear that diversity among organisms is not completely helterskelter. Patterns do exist. Some combinations of characters occur repeatedly, with minor variations on a major theme. Many other combinations of characters that are theoretically possible simply do not exist. I never saw a photosynthetic dog. There are gaps of all sizes, and cluster patterns of all sizes and degrees of density and complexity, in the distribution of character-combinations. The job of the taxonomist is to recognize these cluster-patterns and organize them into a formal hierarchy.

Without taking the time just now to develop the reasons, I will state as dicta that taxonomy is properly based on multiple correlations, that the value of characters should be determined *a posteriori* rather than *a priori*, that the presence of a character is more likely to be important than its absence, and that a proper taxonomic system must reflect evolutionary relationships. I will be happy to discuss with any of you at whatever length may be necessary the reasons for these principles that I here present as dicta, but the only one I want to explore as part of this speech is the one about evolution.

The reasons why an evolutionary classification is preferred to one that cuts across evolutionary relationships are simple. Only if our taxa represent truly evolutionary groups will new information, from characters as yet unstudied, fall into the pattern that has been established on a relatively limited amount of information. If the system is to have predictive value, if

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* Talk presented to the Australian Systematic Botany Society Dinner (with Section 8 of the XIII International Botanical Congress), Sydney, 27.viii.81.

it is to reflect the totality of similarities and differences in addition to the formal critical taxonomic characters, it must have an evolutionary foundation. Artificial classifications, using a few arbitrarily selected characters, are easily devised, but they do not have the predictive value of a natural (i.e. evolutionary) classification; new information will not tend to fall into line.

A closely related, complementary reason why a sound taxonomic system must be evolutionary is that the gaps in the distribution of diversity reflect evolutionary history. In a taxonomic system an effort is made to draw the lines between groups through the gaps in the pattern of diversity. The detection of these gaps, the unravelling of evolutionary history, and the establishment of a taxonomic system are closely interrelated processes.

An evolutionary approach is in my opinion fundamental to the development of a proper taxonomic scheme, but the depiction of phylogeny is properly a means to an end in taxonomy, not the end in itself. I am not a cladist in the sense in which the word is now being used. I consider the amount of divergence more important taxonomically than the time it began. There is some value in current cladistic theory in forcing taxonomists to put their cards on the table and explain how they arrive at phylogenetic and taxonomic concepts, but much of what is being written is in my opinion arrant nonsense. Hennigian cladism is a prime example of confusing means with ends, and it is destructive to what I consider the proper goals of taxonomy. A fuller exposition of the perils of cladism must await some other paper, but you are entitled to know my outlook on it in your evaluation of what I have to say about systems of classification of angiosperms.

One of the interesting anomalies about present-day plant taxonomy is that although most of us are agreed in most respects about the course of evolution in particular characters, and agreed on how to go about making a system of classification, we get such different results when we try to put the principles into practice. Dr. Thorne and I, for example, have virtually no difference in principle, although he tends to lump a little more than I do. Yet once we get past the Magnoliidae (or Annoniflorae, as he calls them), our schemes for the dicotyledons are so different that I scarcely know how to begin making a comparison.

Probably the two most similar present-day systems are those of Takhtajan (as expressed in 1980) and myself. I have many times heard people refer to them in one breath as if they collectively constituted a single system with a hyphenated attribution. Yet differences abound. He refers the Austrobaileyaceae, Lactoridaceae, and Chloranthaceae to the Laurales, whereas I refer the first two of these to the Magnoliales, and the third to the Piperales. He includes the Rafflesiales in the Magnoliidae, near the Aristolochiales, whereas I include this group of bizarre parasites in the Rosidae, near the Santalales. He puts the Euphorbiales in the Dilleniidae, whereas I put them in the Rosidae. I include the Thymelaeaceae in the Myrtales (Rosidae), whereas he has them in a unifamilial order

Thymelaeales following on the Euphorbiaceae. I group the Sarraceniaceae, Nepenthaceae, and Droseraceae into an order Nepenthales, whereas he puts these three families into three different orders in two subclasses. Many more such differences could be listed.

Why should these differences persist? Because of the pervasive evolutionary parallelism in the angiosperms, the weakness of the morphological integration between different organs or organ-systems in plants as compared to animals, the muted operation of competitive exclusion at all taxonomic levels in plants, as compared to animals, and the inadequacy of the fossil record. Other reasons could also be adduced, but those are enough for the moment. The first three of these four listed reasons are in fact merely different aspects of the general pattern of evolution in plants as contrasted to animals. This pattern derives in turn from the facts that plants make their own food, are not motile, and have an open growth system. Again, each of these items could be developed at some length, but not if we are going to get home at a reasonable hour tonight.

One result of the evolutionary pattern in angiosperms is that it is impossible to prepare a synoptical key to families and orders and subclasses that is routinely useful for identification. Any key designed for identification of taxa at these levels on a worldwide basis must be to a considerable extent artificial, and many of the taxa must appear more than once in the key.

The close correlation of structure, function, way of making a living, and taxonomic affinity that we are accustomed to seeing in animals is so attenuated in angiosperms at the level of families, orders, and subclasses that most makers of systems pay it little heed. Angiosperm taxonomists pay lip-service to the neo-Darwinian concept of the mechanism of evolution, but it does not seem to influence their thinking very much. My good friend Armen Takhtajan, for example, is more suspicious of the major taxonomic significance of characters that can readily be explained in terms of survival value than of those that cannot.

In spite of the difficulties, we do make progress. A few years ago the limits of the order Caryophyllales, or Centrospermae, were highly controversial. The controversy now appears to have been settled to the satisfaction of all concerned. The taxonomic distribution of betalain pigments as opposed to flavonoids at first sparked the debate and later contributed to the resolution of the problem. The decisive evidence, in the minds of many of us, came from the seemingly unlikely source of the nature of the sieve-tube plastids. The peripheral ring of proteinaceous fibrils in these plastids now appears to be an absolute marker for the order. Of course we cannot predict what may happen tomorrow, but I now feel comfortable with this part of the general system.

We may be on the way to a similar resolution of the limits of the Myrtales. At a symposium presented at this Congress, the difference of opinion appears to have been reduced to whether the Thymelaeaceae are doubtfully in, or doubtfully out. We cannot be sure that taxonomists not present at the Congress will all agree, but the most notable system-maker

who is not here, Academician Takhtajan, is already close. His suborder Myrtineae is identical to the Myrtales as discussed here (without the Thymelaeaceae), but he also has three other suborders, each with only a single family.

So we come to a discussion of systems themselves. I want to concentrate my attention tonight on seven systems, three historical, four modern. The historical systems are those of Engler, Bessey, and Hutchinson. The modern ones are those of Takhtajan, Dahlgren, Thorne, and Cronquist. I pass over the system of Bentham and Hooker tonight, because it was pre-evolutionary in concept, even though the authors became convinced evolutionists. I pass over some other recent systems because they attracted relatively little attention and have not had much influence on the thinking of the taxonomic community.

The avowedly evolutionary Engler system was the most influential general system of classification of plants for most of the last hundred years, and is only now becoming moribund. It was worked out in great detail, with what we might call Germanic thoroughness. It was widely accepted throughout most of the world, with the exception that botanists in the British empire tended to stay with Bentham and Hooker. It has even worked its way into the International Code of Botanical Nomenclature, in that the conserved list of generic names among the seed plants is in the sequence of Dalla Torre and Harms - a sequence based on the Engler system.

The Engler system has only one major - eventually fatal - flaw. It tends to equate simple with primitive, largely ignoring the significance of reduction. Virtually everyone who is concerned with such things now believes that much (by no means all) of the floral evolution in angiosperms reflects reduction. Flowers that Engler considered to be primitively simple, such as those of *Typha* and the "Amentiferae" are now considered to be simplified by reduction, rather than primitively simple. By 1926 Engler had realized that the flowers of the Amentiferae are simplified rather than primitively simple, and he argued that their extreme reduction indicated the great antiquity of the group. Such an argument misses the whole point of a phylogenetic system. An essential requirement of any phylogenetic system is that one start with the groups which are least modified from the ancestral prototype, rather than with those that have undergone the most change. All groups are of equal age, if one takes in all the ancestors as well as the members of the group. It is only if one bases concepts of age on the members that would actually be referred to a particular group that groups differ in age and a phylogenetic system becomes possible.

We should note at this point that the now widespread dissatisfaction with the Englerian system does not relate primarily to the arrangement of genera into families. Some disagreement on the limits of families is inevitable, and the problem of lumping or splitting will be always with us, but no one wants to reshuffle the genera into a basically different set of families. The dissatisfaction relates instead to the arrangement of families into orders, and to the concepts of relationships among the orders, including how

these may best be arranged in a linear sequence. Such arrangements necessarily depend to a large extent on one's concepts of the nature of primitive angiosperms and the evolutionary trends that have affected the structure and chemistry of their descendents.

The current (1964) Engler Syllabus represents a sort of half-way house along the road to transforming the original Engler system into a really new system based on current thinking. The treatment of the monocotyledons is completely reworked and begins with the Alismataceae rather than *Typha*. The treatment of the dicotyledons retains the major outlines of the original, and still begins with the Amentiferae, but it is studded with comments that one or another group might have to be moved to a new position.

Thus the Engler system, for all its virtues, and for all its dominance over so many years, is now of mainly historical significance. It may be a long time before major herbaria are rearranged, but textbooks and new floras are consigning Engler to the past. The question now is what to use in its place.

Doubts were felt almost from the beginning as to the theoretical correctness of the Engler principles and scheme. The dissent was organized into a major challenge by Bessey in 1915. Bessey's famous "dicta" take their origin in considerable degree from de Candolle's pre-evolutionary proposal that the basic (read primitive) type of flower has more or less numerous free and distinct parts of all kinds, and that many other types of floral structure can be explained as differing (read derived) from the basic type through aggregation, fusion, reduction, and loss of parts. I suspect that de Candolle was a secret evolutionist who in the intellectual climate of the time did not quite dare come out of the closet.

Bessey's 28 dicta attracted immediate attention as a coherent and defensible approach to the nature and direction of evolution in flowering plants. They are still very widely accepted today, except for the one that would have opposite leaves more primitive than alternate. All of the present-day systems that have attracted any considerable support are essentially Besseyan in outlook.

In spite of the wide acceptance of Bessey's principles, his actual system was not so well received and was never widely adopted. His evolutionary tree, familiarly known as Bessey's cactus, was reprinted in many places, and probably most of us who are here tonight have seen it. For many years, however, most botanists continued to follow the Englerian system, in spite of a growing conviction that it was theoretically unsound.

Bessey's system, though sound in principle, was faulty in execution. He did not have a major herbarium at his disposal, and his acquaintance with plants from outside the United States was probably rather limited. He underestimated the amount of evolutionary parallelism, especially with regard to hypogyny-perigyny-epigyny, and he gave only the briefest of descriptions for his families and orders. Anyone wanting to crib a description of a family for a flora still had to go to Engler or to a source derived from Engler.

The first avowedly Besseyan system, after Bessey's own, was that of John Hutchinson, first published in two volumes in 1926 and 1934. A second edition was published in 1959, and a third in 1973. At the time of his death, in 1972, he was hard at work on a new Genera Plantarum designed to replace that of Bentham and Hooker. Unlike Bessey, Hutchinson provided good descriptions, and also a considerable number of illustrations.

In terms of general acceptance, Hutchinson's system was a near miss. Everyone knew about it, and descriptions of families for floras were frequently cribbed from it, but very few writers of floras or textbooks actually accepted it.

In the view of many of us, Hutchinson's system was ruined by a single pervasive error. He believed that there was an early and fundamental dichotomy among the dicotyledons into a basically woody and a derived, basically herbaceous phylad. Most of us, on the contrary, believe that herbaceous dicotyledons have originated from woody ones many times, and that on a lesser number of occasions herbs have reverted to the woody habit. At the time of Hutchinson's first edition, taxonomists were not so disillusioned with the Engler system that they were ready to give it up for another system that was also perceived to be pervaded with error. In his first edition, Hutchinson did not in fact carry his views on the woody-herbaceous dichotomy to their logical taxonomic conclusion, in a number of instances. In the second and third editions he came progressively closer to doing so, thus making his system progressively worse in the eyes of his contemporaries, rather than better.

Hutchinson was also determinedly classical, in that he did not like to consider characters that could not be seen with the naked eye or a hand-lens. What was good enough for Bentham and Hooker was good enough for him. I am reminded of a hymn still popular in fundamental churches in the United States, which starts out, "Give me the old-time religion", and has the persistent refrain, "It was good enough for Moses, and it's good enough for me". In this case, what was good enough for Bentham and Hooker may have been still passable in the 1920's, but it became progressively less so under the flood of micromorphological and chemical data that followed. Hutchinson would, on occasion cite micromorphological data in support of his views, but I have seen no indication that he gave such things any consideration in making up his mind in the first place.

I remember speaking to him at Kew in late 1951 or early 1952. There was some matter, I do not now remember what, in which a recent anatomical study came to conclusions contrary to his own. I asked him about it, not in a contrary spirit, but to find out what he thought. He told me clearly and bluntly, although I do not recall the exact phraseology, that any time the anatomists start looking into things they get them all confused.

Now we come to systems produced by currently active taxonomists. I want to say something about my own scheme first, and then compare the others with it. Naturally I think my own scheme is the best. If I did not, I would have no business writing a book about it. I must, of course, concede the same right to my colleagues about their schemes. People are notoriously not the best judges of their own work, and it is up to the botanical community to determine which if any of the current schemes is good enough to replace the traditional Englerian one. About the moribund condition of the venerable Englerian scheme I have no doubts. The question is what will replace it, and when. Replacement is of course to some degree already under way, as shown by the fact that Al Smith's flora of Fiji is following the Takhtajan scheme, and the new flora of Australia will follow mine.

Perhaps the most important thing that sets my system apart from others is that I have consistently tried to prepare synoptical arrangements, in key form, of the families within the orders, the orders within the subclasses, etc. This procedure has profoundly influenced my thinking. As we have already noted, the pervasive parallelism within the angiosperms confounds all efforts to prepare a natural key that is consistently useful for the identification of families and higher groups, but if one cannot prepare a synoptical arrangement that will provide for most members of each taxon in a large group, it is time to stop and reconsider. Sometimes, after such reconsideration, we must still settle for phenetically ill-defined affinity-groups, such as the conceptually useful but poorly characterized subclasses Rosidae and Dilleniidae. Often, however, it is possible to choose among phyletically valid alternatives in such a way as to promote the relative homogeneity and phenetic definability of the taxa to be recognized.

Most makers of systems of angiosperms give no evidence of considering the selective significance of the characters they use, or the ecologic significance of the groups they recognize. I can readily understand why, because it is so often difficult or impossible to come up with a plausible answer. In the dicotyledons, in particular, families and orders are apt to be ecologically very diverse. On the other hand, recognition of major groups of monocots by their aspect is not so consistently difficult and frustrating as a comparable effort among the dicots. Such recognition therefore becomes a feasible addition to the other objectives of a system of monocots. A taxonomic system should always be as simple and easy to use as is consistent with naturalness. My scheme for the monocots reflects my attention to these matters.

The system most nearly comparable to my own is that of Academician Takhtajan. This similarity reflects a community of interest and outlook, bolstered by frequent correspondence and personal conversation over a period of more than 20 years. We believe that the similarity reflects the requirements of the present state of knowledge and taxonomic theory.

Then why do the differences persist? There are several reasons. For one thing, he does not prepare the sort of synopses, in key form, that I have referred to. Thus his scheme is weighted more toward the recognition of affinity-groups, and less toward phenetic definability, than my own. It is a difference in emphasis, rather than a diametric opposition of views, but the difference can swing the balance in close decisions. He is more likely than I to recognize small families and orders, in the pursuit of closely knit groups. This may seem to be in opposition to what I just said about affinity groups as opposed to phenetically definable groups, but in practice it is not. I may decline to recognize a small satellite group if its inclusion in the larger group does not compromise the distinction of that group from others, where he may choose to recognize the satellite because it would be aberrant in the larger group. On the other hand, I may find it necessary to recognize a family or an order to facilitate the preparation of a synoptical arrangement, whereas he can put it with a larger group to which it is obviously allied. Furthermore, Takhtajan places a little more reliance on serology, and a little less on other chemical characters, than I. He is more suspicious than I of the major significance of characters that can readily be interpreted in terms of survival value. Even with access to similar herbaria and the same body of literature, no two people work with exactly the same set of data, or are exposed to exactly the same influences from their colleagues. Although we agree on the main outlines of angiosperm evolution, there remain many doubtful cases and close decisions at the present state of knowledge, and any two people will inevitably make some of these decisions differently.

That brings us to the systems of Dahlgren and Thorne. Each of these authors has presented several rather different versions during the past few years, and I find it a little difficult to keep up with what they are doing.

I believe that the fundamental reason for most of the differences between Dahlgren's system and my own is that he places a great deal more weight on chemical characters, especially secondary metabolites, than I do. Thus he has tried in the past to put everything that produces iridoid compounds into the same major group. I understand that he has retreated a little from that stand recently, but the melody lingers on.

I do not disparage the taxonomic significance of secondary metabolites, many of which evidently serve to protect the plant from possible predators. Indeed I believe that the substitution of new groups of secondary metabolites for old has played a major role in the diversification of angiosperms, and in the rise and subsequent decline of a number of major taxa. I even wrote a paper about that several years ago, and in my descriptions I consistently try to say something about the secondary metabolites. At the same time, the secondary metabolites constitute only one relatively limited set of characters, and they are just as subject to parallelism and convergence as other characters.

The most consistent unifying feature of the order Capparales, with nearly 4000 species, is the production of mustard oil, but there is also a scattering of about 300 species, in 6 other orders as I see it, that produce mustard oil. Dahlgren would include a number of these other taxa also in the Capparales, and I have lost track of just which ones remain there in his latest scheme. I believe he has never transferred *Drypetes* (a euphorbiaceous genus of some 150 species) to the Capparales, however, even though it produces mustard oils.

Tropaeolum, in the order Geraniales, produces not only mustard oil but also erucic acid, a substance found mainly in the Capparales. Yet the cytochrome *c* of *Tropaeolum* is very different from that of *Brassica*. They differ in 9 amino acid positions. There are only 10 sequence differences between *Brassica* and *Triticum*. Even the most ardent proponents of the taxonomic and evolutionary significance of cytochrome *c* now admit that it does not by itself provide an adequate basis of the construction of a phylogenetic tree, but I think it must be conceded that genera with as many sequence differences as *Tropaeolum* and *Brassica* are not likely to be closely related.

I get the impression that Dahlgren is being progressively more reasonable, from my standpoint, in the evaluation of chemical characters for his system, but many differences remain. I still have not grasped the essential features of his system in a way that will permit me to feel that I know what holds the orders and higher groups together, and thus what most members of these groups can be expected to be like. Of course this may well be a sin of omission on my part, rather than a sin of commission on his.

I understand that a major reconsideration of the monocotyledons, by Dahlgren and Clifford, will soon appear. From conversations with Dahlgren, I think that our views on the system of monocots may be less divergent than on the system of dicots, but I shall have to wait until I see the paper.

Finally, I must say that I do not understand the underlying reasons for the differences between Thorne's system and my own. His paper on the Annoniflorae, which I call Magnoliidae, is excellent. He lumps a little more than I do, but I really have nothing to complain about. Furthermore, our views on the system of monocotyledons are reasonably compatible. The remainder of the dicotyledons are another matter, on which we have many fundamental differences. The nearest I can come to an explanation is to say that we evaluate overlapping sets of similarities and differences differently in many instances. It is of course a standard kind of problem for angiosperm phylogenists that if the similarities between A and B in one set of characters are interpreted to indicate close relationship, then another set of similarities between B and C must be dismissed as a reflection of parallelism or convergence. The fact that Thorne and I so often come to different conclusions, even though our principles are similar and we have access to essentially the same set of data, should serve as a warning that we should not take any one system as being necessarily correct.

When I was in graduate school I was exposed to a comment that I am now unable to trace to a source. Even without a source, I think it is worth passing on. Phylogenetic trees, having no roots, are easily blown over.

BOTANICAL BUCCANEERING.*

by R.C. Carolin.

School of Biological Sciences, University of Sydney, N.S.W. 2006, Australia.

My mother was a sort of suburban pirate. Moreover, she had no compunction about using her family as accomplices in her buccaneering activities. When I was a small boy, and it may come as a surprise to some of you that such was the case, she used to take me on a 4d bus ride to Hampton Court Palace. She wasn't particularly interested in the long oak panelled corridors, the beautiful tapestries or fine dark paintings in the Palace itself; it was the plants in the gardens which excited her imagination. On many occasions I was forced to stand in such a way that the fact that she was digging up one of the King's plants or snipping a branch off another could not be seen by the passers by, rangers, policemen or MI5 men who might be around. My childhood nightmares were not about two-headed monsters. They involved King George VI sentencing me to be decapitated for being an accessory after the fact of the theft of one branch off his George Dickinson Rosebush. However, like all really good buccaneers my mother and I were never caught. She always had a garden that was admired by respectable people. She became a member of the Royal Horticultural Society and when she died the local vicar planted a rose garden in her memory which probably contained a few purloined roses.

I tell you this little personal story to remind you that buccaneering is often thought of as a respectable pastime and also of its rather close connection with botany. The first professor of botany in Australia, here at Sydney, took great pride in referring to his ancestory of Scottish pirates and I gather many of his colleagues thought he carried on the profession tolerably well himself. But, of course, long before him, the first botanist who collected plants on the continent held a privateer's license altho' as far as I can gather he was considerably less successful as a pirate than my mother was and he committed that awful sin of field botanists in "getting his specimens mixed". Some years ago I was searching for a suitable logo or house symbol for the John Ray Herbarium. I thought it would be rather satisfactory if we used an illustration of one of Dampier's specimens — or at least a formalized representation. I selected a drawing which looked like a *Scaevola* and was duly labelled "Hab. in Nov. Holl." Imagine my dismay when subsequently I discovered it was a *Centropogon* from Brazil transferred to the wrong paper during a frantic rush to roll the canon out. Nevertheless we stuck with it.

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* Talk presented to the Australian Systematic Botany Society Dinner (with Section 8 of the XIII International Botanical Congress), Sydney, 27.viii.1981.

Not only has Australia this tradition of piracy it also has a considerable tradition in the botanical sciences right from the early days of European settlement. Indeed a newcomer to our shores might believe we have a fixation on plants when he finds that a number of our ethnic nicknames are plant oriented. The affectionate name for the english is a "pom" which might be derived from "pommes" referring to the englishmen's rosy cheeks like an apple at least that's the story to tell the English. It does lend itself to abuse of course and I remember going into a hotel in the Blue Mountains to find scrawled across the dart score-board "Grow your own dope, plant a pom". There was a charming barmaid there and I pointed out that I had once been a pom and in different circumstances I could have taken exception. Well she said she was sorry for me and was glad I'd got better and gave me a free middy of beer. It doesn't always follow that plant names are used for ethnic groups and I still treasure the bemused expression on a French friend's face when I introduced him to some Australian who started off the subsequent conversation "Oh so you're a frog are you"?

We really are wandering a bit from botany aren't we?

Botany at Sydney was started by a clutch of Bower's pupils from Glasgow before and during the first world war. Botany was taught to and remembered by hundreds of students in good Scots language and when you bear in mind that the Scots aren't quite English neither are they quite Irish, Lawson and his colleagues did a remarkably good job. They produced some pioneering work on plant ecology, particularly in the Snowy Mountains area. One has a sneaking suspicion that the excellent trout fishing there might have something to do with the selection of the site but be that as it may, the results were there. They researched the life-histories of the peculiar gymnosperms of this continent, the pollination mechanisms of its flowers, some unique fungal-plant associations and they even found time to enquire into nutrient-plant relationships. None were particularly eminent in the international sphere except Lawson himself, all however were decent tradesmen with a view of botany as a whole. And that is the crux of their success, for they were successful - their interest in botany rather than a small section of plant science.

It might be a case of sheer egotism, but I think that taxonomists have never lost this interest in botany as a whole and of course one of the results of this embracing interest is the compilation of improved systematic arrangements. (I must confess I was a little dismayed when I heard that one of these new arrangements had been accepted as the basis for the arrangement in the New Flora of Australia, being an Englerman myself. Dismay followed dismay when I found Arthur was actually coming to Australia. I thought we'd have to change the shape of the doors to a kind of broad-obovate outline. However it proved quite unnecessary, he has retained the elliptical outline that I know so well.

It is, I guess, appropriate that Arthur has given us the address tonight. He has, after all, left his imprint on Australian botany in the New Flora for future generations to view. But let us not lose sight of the fact that this imprint could only have been produced by someone who is not just a taxonomist, it required a botanist, and since I am now no longer a councillor of the Society, I can only suggest from the floor that we might acknowledge that we all aspire to be botanists and become the Botanical Society of Australia.

Allow me to thank all those people who have contributed to this and all our sessions from Arthur for his address to us this night right back to the guy who started it all when he strode across the sands of Roebuck Bay almost three centuries ago!

NEW PUBLICATION FROM CONSERVATION COMMISSION, NORTHERN TERRITORY.

"Eucalypts of Central Australia"

by A. Mitchell.

A new technical bulletin detailing Eucalypt species found in Central Australia has been prepared by the Conservation Commission.

This bulletin will be of interest to the informed layman, and to botanists or scientists involved in research work.

The booklet details descriptions, habitats and distribution of 26 species of Eucalypt, and also relates some interesting notes on some of the species, including use by Aborigines. Editorial on each variety is supplemented by line-drawings of the leaves and seeds.

The bulletin was compiled and written by Andrew Mitchell, a planner with the Conservation Commission in Alice Springs. It is the third in a series of similar publications designed to compile information on flora and fauna of the Territory. The two previous bulletins were "Fire Management in Top End Conservation Reserves" and "Deciduous Vine Thickets of the Darwin Area and Effects of Cyclone 'Tracy' 25 December 1974", both by Bob Fox.

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SUCCESS OF HORTICULTURE COURSE

The first horticulture course to be offered in the Northern Territory has been an outstanding success with five students in Alice Springs being awarded the Certificate in Amenity Horticulture from the S.A. Department of Further Education.

The three-year part-time certificate course was started in 1978 to provide a trade-level course of study for people who are working in, or are about to enter, the private or public sectors of ornamental or amenity horticulture, or allied industries. This includes parks and gardens, plant nurseries, landscape design and construction, maintenance of sports grounds, and the like.

The course was run in Alice Springs with the support of the Community College of Central Australia, using two lecturers, John Maconochie and Andrew Mitchell, with assistance from other qualified staff in Alice Springs.

A wide range of subjects was covered including soils, botany, nursery practice and landscaping. Students also had field trips, projects, and laboratory and practical work to complete. Landscaping designs for Alice Springs which students prepared, were on display at the Community College and at the Alice Springs Show last year.

The Certificate is the first horticulture course to be run in the Territory, and the first in arid zone horticulture in Australia.

SPECIATION IN THE TROPICAL RAIN FOREST : WHERE DO WE STAND NOW?*

by P.S. Ashton.

The Arnold Arboretum, Harvard University, 22 Divinity Avenue, Cambridge, Massachusetts 02138, U.S.A.

We have heard today some of the sources of evidence for patterns of speciation among tropical rain forest trees. It will be my job to attempt to bring these reviews together in a summary statement of our knowledge to date and, in the spirit of the Congress Symposia, to indicate promising avenues for future research.

The reason for our interest in this subject stems, on the one hand, from the extraordinary species richness of some, though not all, rain forest tree communities, and equally extraordinary seasonal and geological uniformity of the moist warm climate experienced by many, and especially those, it seems, with outstanding species richness. How can so many species, sharing a common uniform habitat and, superficially, habit, possibly each persist through niche specialization? This species richness is a manifestation of the exceptionally large number of species with very low population densities: How can outbreeding persist in these populations? How are numbers maintained in equilibrium over evolutionary time, or are the majority of species on the way in or out?

To answer these questions we need a biosystematic approach. In particular, we need prolonged study of phenology, seedling establishment, growth, demography and the causes of mortality in selected species, and especially groups of closely allied species sharing the same community type. We also need information on the patterns of genetic variation within and between populations and, especially, the size and area of the evolutionary unit of population (Ehrlich and Raven, 1969; Levin, 1979). In the meantime a pattern is beginning to emerge from the largely empirical evidence so far available.

Systematic and biogeographic studies suggest that speciation is generally allopatric. Sympatric distribution patterns of putative infraspecific taxa are occasionally found though, for instance in Dipterocarpaceae. These have been shown to be ecotypic in some cases (Ashton, 1969), while in others the absence of morphological clines suggests that barriers to hybridization already exist. This notwithstanding, systematic analysis of the series of congeneric tree species sharing a common habitat, that characterize in particular the West Malesian forests (e.g. Fedorov, 1966) suggests that the individual species components are either relatively isolated systematically, or have allies in other habitats that are closer than are the other members of their sympatric series. Differentiation appears for the most part to be exceptionally gradual, except where segregates have come to occupy dramatically different habitats, such as savanna, or river beds below flood level (e.g. van Steenis, 1981). Even

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* Talk presented to XIII International Botanical Congress, Sydney, 24.viii.1981.

here, evidence is lacking as to whether the process of speciation is sudden, that is "salatory" (Goldschmidt, 1933), or gradual. Polyploidy, or manifestation of sudden evolutionary change, does occur though in certain rain forest taxa (Kaur et al., 1978).

Within these series of sympatric species, differentials in habit at maturity (Ashton, 1969), light requirements (Wyatt-Smith, 1963), flowering and fruiting times (e.g. Snow, 1966; Start, 1914; Chan and Appanah, 1980), as well as pollen vectors (Gentry, 1974) have been observed, though Janzen's suggestion that species richness may be maintained, through the influence of host-specific seed predators on the population densities of individual species, has not so far been supported with evidence of predator specificity among sympatric congeneric tree species. It is one thing to observe these various differences in potentially adaptive characteristics, and quite another to demonstrate that they are being favored or maintained through selective mortality. Biotic interactions in tropical rain forests are exceedingly complex. There are always multiple explanations for correlations, and hence the pervasive danger of teleology. In this respect, Chan and Appanah's (1980) meticulous observations of the flowering phenology of population samples of six sympatric emergent tree species in *Shorea* section Muticae (Dipterocarpaceae) would particularly merit further extension. At Pasoh Forest, Peninsular Malaysia, they found that each species flowered in sequence, with some overlap but with separate flowering peaks. All species shared the same thysanopteran pollinators. Further observation of flowering phenology within this section in neighboring forests, where the species array may differ, could reveal evidence of localized differentiation in the flowering sequence. Comparison of mortality among buds that open when one, or more than one, species is in bloom could yield direct evidence for selection, while spatial discontinuities in the flowering sequence could indicate the area of individual breeding populations.

Once again, the evidence suggests that evolution is gradual, though still far from inactive, within the rain forest. The frequency of mass fruiting among many Far Eastern families, the general low level of fruit production in species of the mature phase, and the absence of dormancy (as opposed to delayed germination) could imply adaptation to an extraordinarily predictable physical environment. Alternatively, though, it could imply that most selection takes place within established individuals, or yet again that survival is largely random though that is hardly supported by other evidence.

An alternative, albeit indirect, approach is through analysis of reproductive biology and breeding systems, which can provide evidence of the existence, and degree, of gene exchange within populations. The high level of dioecy, which is concentrated among species of the understorey, is now well-known (Ashton, 1969; Bawa and Opler, 1975). Recent work in Central America and Malaysia indicates that most species of hermaphrodite-flowered trees have high levels of self-incompatibility.

Studies of pollinator behaviour (e.g. Stiles, 1975; Start, 1974; Appanah, 1979) are indicating that the means do exist, often of incredible subtlety, to transfer pollen between flowers of distant individual trees. It is now known that pollen dispersal varies greatly between different taxa. Thus the nectariforous bat *Eonycteris* may commute 50 km nightly to its principal feeding ground (Start, 1974), visiting flowering trees *en route*; while one understorey tree species is now known to be wind-pollinated (Bawa and Crisp, 1980), presumably only at exceptionally short distances. Short-range dispersal appears to prevail. It will prove very difficult under rain forest conditions, though, to establish proportional pollen dispersal distances in the manner of Colwell's (1951) classic study, and thus provide further evidence of the area of the breeding population.

Much the same range of patterns are known to occur in fruit, as with pollen, dispersal vectors, and the two appear frequently to be correlated. As wide a range of breeding population seems to occur therefore in rain forest trees as, say, in butterflies. Preliminary results from isozyme electrophoresis, coupled with analysis of morphological variation within populations, also suggest that most gene flow takes place over short distances (Yap et al., 1977).

Claims of self-compatibility without direct evidence of zygote formation must be discounted in view of the increasing evidence that apomixis, through adventive embryony, occurs in many families of rain forest trees. The evidence for apomixis has been derived directly from embryological studies, and inferentially from demonstration of consistent triploidy in seedlings sharing a common parent. In Dipterocarpaceae there is a loose correlation between adventive embryony and polyploidy, while in some genera of other families, e.g. *Garcinia* (Clusiaceae), adventive embryony is associated with polyploid series (Kaur et al., 1978).

Much further work is needed, to establish whether adventive embryony is widespread among rain forest trees. So far, it has been demonstrated or inferred in a surprising number of those series of congeneric taxa which are of so much importance to our understanding of speciation in the tropical rainforest, including *Citrus*, *Eugenia*, several genera in Clusiaceae and Dipterocarpaceae, and *Mangifera*. The little evidence available suggests that it is generally facultative, with variable levels in different individuals within a population. There is little doubt now that apomixis plays some part in speciation among rain forest trees, but its importance will prove difficult to assess. We can only speculate on the adaptive significance, if any, of this form of apomixis among rain forest trees, though it cannot be doubted that it reduces the level of genetic variability within populations, and hence their capacity to adapt to change. It has been suggested (e.g. Ashton, 1969) that high tree species richness in some tropical forests reflects the geological age of the community type. Apomixis may accelerate rates of speciation though, and there is some phytogeographic evidence to support this (Ashton, 1979). It will be interesting to ascertain whether there is a proportionate increase in the representation of apomictic species with increase in species richness in closed rainforest communities.

In tropical American forests, it seems, species richness is concentrated in the mature phase understorey shrubs and treelets, while (Gentry, in litt.) in montane forests it is among the epiphytes. So far no detailed study of patterns of diversification has been made in either.

Most autecological research to date has concentrated, not surprisingly, on species with relatively high population densities. We still know little about the reproductive biology of the rare species that comprise the majority. Their ecology and breeding systems alike will be exceedingly hard to study. Once again, the answer must lie in long-term studies, which are best done by residents in the tropics. There remains a critical need for large permanent plots in strictly conserved forests, where all individuals of selected populations can be tagged, mapped and monitored.

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SOUR GRAPES

(painted by Bauer, Parkinson, etc.)

The museums of London and Europe hold hundreds if not thousands of original drawings of plants and animals discovered in Australasia in the great days of exploration. Most of this has never been published, due in part to the cost of good colour reproduction in earlier times. While there may be relatively few iconotypes in the collections, the material is of great interest to Australian biologists for many reasons. It is a record of the landscapes and people at the time of white discovery. It records the plants seen at that time with considerable accuracy. It is certainly important scientific and historical data in any study of the discovery of Australasia.

It has been a longstanding tragedy, due to a colonial mentality and philistine values on the part of Australians, that greater efforts have not been made here to have this treasury made more accessible. Good quality colour reproduction is now infinitely cheaper than it used to be and methods of printing are changing so rapidly that even higher standards can be expected but we don't need extravagant 10 colour separations on handmade paper.

The possibility of even getting reasonably priced prints of this heritage is being filched by elitist publishers catering for an investment market at prices out of the range of most institutions, let alone individuals. The worst aspect is that a 50 year copyright ban goes on to the future use of any of these paintings preventing their reproduction at more reasonable rates. First we had the Basilisk Press "The Australian Flower Paintings of Ferdinand Bauer" and now the Banks Florilegium. I don't query the exceptionally high quality of either of these works. It is a printing coup to be able to use the original copper plate of Banks and the whole exercise is remarkable.

However, I must register my protest at the attitude of the B.M. in particular conniving with publishers in accepting the long-term copyright blanket.

Let the investors have their elephant folios by all means, but the people who are most likely to look at, use and benefit from these collections will find them as unavailable as ever.

I hope that the A.S.B.S. representative on the Australian Academy Flora Committee will raise the matter.

D.E. Symon.

(Adelaide).

EVOLUTIONARY PATTERNS IN *DICERANDRA* (LABIATAE).*

By Robin B. Huck,

University of North Carolina, Chapel Hill N.C. U.S.A.

Dicerandra Bentham (Labiatae) - endemic to the southeastern coastal plain of the U.S., exhibits considerable variation throughout its range. Of special interest diagnostically as well as evolutionarily, are the spurs which extend from the anthers in the flowers of this group.

Currently seven species are recognized within this genus and they themselves are endemics, restricted to four states in the southern United States. From the southernmost species to the northernmost is a distance of approximately 975 km.

The flowers of *Dicerandra* are characterized by spur-like appendages which extend from nectariferous anthers. At the base of the spur is the stomium of the pollen sac. The spurs are triggered by entomophilous vectors for pollen release. As the spur is pressed forward by the insect, pollen squirts from the stomium below the spur. It is believed that characters relative to spur dimension and shape are critical in this taxon and under strong selective pressure.

Ontogenetically the spur in *Dicerandra* develops as a tiny protrusion of tissue on one side of the pollen sac. The pseudo-transverse dehiscence pattern of the anthers in *Dicerandra* represents a slight shift from the usual longitudinal dehiscence pattern in the Labiatae, the spurs and pollen sacs having rotated so that the stomium now opens transversely at anthesis.

Dicerandra is psammophilous, specific to the soils of xeric dune systems of the southeastern coastal plain. It grows on the edge of oak-dominated plant associations in those xeric areas. The remnants of these dune systems laid down since the Miocene stretch in a north - south direction from the states of Florida to South Carolina, providing habitats for the soil specific flora. Often these remnants are island-like. Edaphic exclusivity is seen as the isolating factor separating populations of *Dicerandra*, promoting genetic isolation and eventual speciation.

Indeed, the ranges of the three southernmost species, *D. frutescens*, *D. immaculata* and *D. cornutissima*, grow as if on marooned islands, or remnants of Pleistocene dune lines. These southernmost species are woody suffruticose Chamaeophytes - and allopatric in distribution.

These three species have infundibular corollas, exerted stamens and are protandrous. Data indicate that they are self-incompatible and obligate outcrossers, apparently totally reliant upon their entomophilous vectors for pollen transfer. Pollen is deposited sternotribically.

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* Talk presented to the XIII International Botanical Congress, Sydney, 22.viii.1981.

Spur length can range to 2.5 mm as seen in *D. cornutissima* or spurs can be absent as in *D. densiflora*. The loss of spurs and the brush type blossom in the latter species suggests that that flower is more of a generalist attracting many different pollinators.

To the north are the two pioneer annual species with the widest distribution, *D. linearifolia* with an infundibular corolla and *D. odoratissima* with a bilabiate corolla. These two are sympatric in a small area in the State of Georgia. *D. radfordiana* is a localized endemic there. Both *D. odoratissima* and *D. linearifolia* have a small capacity to self. However, they are predominantly outcrossers.

Considerable variability exists in both these complexes of *D. odoratissima* and *D. linearifolia*. To explain this variation Mayr's founders principle is invoked: each seed is a founder, carrying its own limited gene pool, each forming the basis for a new evolutionary line diverging from the ancestral population.

An apparent shift to a higher ploidy level as well as radiation into a mesic habitat has occurred with *D. radfordiana*. As well, phenological displacement of colour has taken place: *D. odoratissima* usually with a pink corolla blooms simultaneously with the white corolla members of the *D. linearifolia* complex in September; the pink corolla members of *D. linearifolia* bloom in October.

In the bilabiate construction of the *D. odoratissima* complex the stamens are inserted and pressed against the upper lobe of the corolla with spurs pointing downward into the corolla. Nototribic deposition of pollen occurs, the pollen being deposited on the back of the insect.

Two major morphological patterns relating to pollinating strategies are present in *Dicerandra*: the bilabiate corolla and inserted stamens are linked to nototribic deposition of pollen and the infundibular corolla and exerted stamens are linked to sternotribic deposition of pollen. No intermediates to these patterns have been observed in the field or in the examination of herbarium specimens.

The spur mechanism of anther dehiscence exhibited by *Dicerandra* is viewed as one of great specificity and parsimony, of adaptive significance in the hot xeric areas for efficient transfer of pollen. A similar adaptation may be present in the spurred members of the Prostanthereae of Australia.

THE BOTANIST'S NIGHTMARE.

by Rick Burchall.

To the tune of "The Floral Dance"

******

As I slept tight on a summer night
I thought I saw a field in sight
And I was slowly walking there
Carrying all my field trip fare
And it weighed a hundred pound.

Sweat poured down from my fevered brow
My feet were covered with a gift from a cow
Searing pain in my aching back
Optic strain, then I lost the track
And my pack now weighed two hundred pound.

And then I could hear a curious roar
Of a likeness that I never heard before
Yelling, screaming, highpitched laugh
Perfumed all with the smell of grass.
Close at hand, but I knew not ought,
I heard the sound of the floral rort.

And then I stood on the edge of a space
A sign said "Welcome to Xanthosia's place"
A purple Veronica slapped my back
Thomasia took my pack
Tho' it weighed three hundred pound.

Symphionema played Velleia
Hypocalymma waltzed Tristania
Passiflora stripped it's vestige
Actinotus got the message
And they Prostanthera'd, smilaxing there.

Stackhousia grapped little Viola
With a look that said that she had gone too far.
August, Cordi, Folium twins
Took turns with Cassinians
Cryptostyles erecta was fraught
For his chance to join the floral rort.

And then there came another sound
A revving roar, I looked around,
There in black on Harley Dees,
Fumes and oil, rode miseries,
And they weighed four hundred pound.

Led by a Fungi, a rust in tow,
A Canker I saw, and a small yellow.
A leaf spot rode with a scab behind.
All spilling their filth, of a human kind.
Oh horror! All me! When they turned around.

Twas then I awoke with a horrible start
With a thick tongue, headache and a beating heart.
Shaking, quaking, bed all wet,
The sun, not risen fully yet.
Then I saw sitting on my sill
My plant box of Little Daffodil.

Waiting for, I cannot say
Then they felt the first morning ray.
Out they came, in a Wondrous show

Hurray!
Mother Nature's Mightier still.

Mr L.G. Adams
Herbarium Australiense
C.S.I.R.O.
P.O. Box 1600
CANBERRA CITY
A.C.T. 2601