IOP NEWSLETTER 31

INTERNATIONAL ORGANIZATION OF PALAEOBOTANY

INTERNATIONAL UNION OF BIOLOGICAL SCIENCES
-SECTION FOR PALAEOBOTANY
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IOP NEWS

ELECTION OF OFFICERS TO IOP EXECUTIVE COMMITTEE

Nominations for the posts of President, three Vice-Presidents and Secretary continue to be received at the IOP office in London. THE CONSTITUTIONAL DEADLINE FOR THE RECEIPT OF NOMINATIONS IS DECEMBER 31st 1986.

Once the secretary has written confirmation that those nominated are prepared to serve, names of all the candidates will be presented in the next Newsletter. If an election is needed for any of the posts, ballot papers will be distributed to members with the Newsletter and votes will be counted at the IOP General Assembly in Berlin at the International Botanical Congress. Postal votes will be accepted from those unable to attend the Berlin meeting.

The incumbent IOP Secretary announced a year ago in Newsletter 28 that he intended not to stand for a further term of office. There has been strong pressure for him to change his mind, which he has

done. Of course, others can still be nominated for election to that job.

AGENDA FOR THE IOP GENERAL ASSEMBLY, BERLIN, 1987

This is the only formal business meeting of the IOP Executive Committee and members that is provided by our Constitution. (Since our Constitution was written the newly instigated IOP Conferences, which take place about half way between Botanical Congresses, also have a Business Meeting; but this is unconstitutional and cannot have foral responsibility for such things as changing the Constitution.)
Will those members wishing to make Proposals for the 1987 Berlin General Assembly please send details to the IOP Secretary.
The IOP Constitution was attached to IOP Newsletter 3. Copies are available on request from the Secretary.

NEWS OF FORTHCOMING MEETINGS

PALAEONTOLOGICAL ASSOCIATION, London, February 11th, 1987.
This is a review seminar on 'Major events in the evolution of land vegetation' and is being convened by Dr M.E. Collinson at the Department of Biology, King's College London (KQC), Kensington Campus, Campden Hill Road, London W8 7AH.
Events will be reviewed by Chaloner, Collinson, Dianne Edwards, Scott and Spicer.

LINNEAN SOCIETY PALAEOBOTANY GROUP, Cardiff, April 6 - 9th, 1987.
The meeting intends to celebrate the 50th anniversary of Cooksonia, and so the theme is 'Pteridophyte Diversity'. There will be two days of field trips and two of papers, both invited and contributed ones.
Write to Dr D. Edwards, Department of Plant Sciences, University College, P.O. Box 78, Cardiff CF1 1XL, Wales.

3RD IOP CONFERENCE, Melbourne, 24 - 26th August, 1988.
The Second Circular should be distributed early in 1987 to all who sent in 'Notification of Interest' replies from the First Circular, and those others who request it. It will have full details on registration, accommodation and excursion costs, and some elaboration of the proposed programme.
It will contain slips for payment which should be despatched before February 29th 1988.
There will not be a Third Circular, but registrants will receive a receipt and supplementary notes with progress information.
On 1st October 1986 195 'Notifications of Interest', including 93 accompanying members, had been received.
An incentive to early registration and other payments is the present exchange rate, which is very favourable to overseas visitors.
Contact Dr J. Douglas, Department of Industry, Technology & Resources, PO Box 173, East Melbourne, 3002, Victoria, Australia.

7TH INTERNATIONAL PALYNOLOGICAL CONGRESS, Brisbane, August 28 - September 2nd, 1988.
The First Circular is now available (European Members of IOP should receive one with this newsletter) and can be obtained from the Conventions Department, PO Box 489 GPO, Sydney, N.S.W. 2001, Australia.

NEWS OF RECENT MEETINGS

III CONGRESO LATINAMERICANO DE PALEONTOLOGIA, 1984.
This congress included a symposium on Late Triassic floras, and the proceedings have recently been published. There are plans to review the book in a forthcoming newsletter.
Send USD 23.00 to Simposio sobre floras del Triasico Tardio, su fitogeografia y paleoecologia, Memoria, Instituto de Geologia, UNAM, Mexico, DF.

L'EVOLUTION DES GYMNOSPERMES: APPROCHE BIOLOGIQUE ET PALEOBIOLOGIQUE. Montpellier, September 1986.
This meeting was held under the auspices of the Fondation Louis Emberger - Charles Sauvage in collaboration with the Societe Botanique de France and the Laboratoire de Palaobotanique (Institut des Sciences de l'Evolution, CNRS). The local organization was handled with characteristic good humour and efficiency by Jean Galtier and his colleagues in the paleobotany laboratory in Montpellier. Although paleobotanical papers predominated (24 to 9) the meeting was particularly successful in its aim of bringing together both neobotanists and paleobotanists interested in the common problem of gymnosperm evolution. The program allowed ample time for discussion and this helped to generate the pleasantly relaxed atmosphere as well as some interesting commentaries and frank exchanges of views. The papers covered a wide variety of topics ranging from detailed studies of pollen and spores, including their ultrastructure and ontogeny (Allen, Audran, Chesnoy, Lugardon, Pedersen & Friis, Smoot & Taylor) to topics concerned with the structure, growth and ecology of extant conifers and cycads (Edelin, Medus, Pena et al., Woltz, Wang). Many of the contributions will be published in a special volume of the Bulletin de la Societe Botanique de France.
Several papers dealt with early seed plants from the late Devonian and early Carboniferous (Beck & Stein, Bertram, Galtier, Matten, Meyer-Berthaud, Rothwell, Rowe) and demonstrated the intensity and excitement of current research in this area. A good deal of new information was presented and in particular Muriel Fairon-Demaret's careful and thorough presentation of her work with Steve Scheckler on the seeds and cupules of Moresnetia from the Famennian of Belgium was one of the highlights of the meeting. Among other details revealed was the deeply divided Genomosperma - like integument, which fits very conveniently with the usual textbook interpretation of the origin of this structure. Moresnetia is now one of the most completely understood Devonian seed plants and if details of vegetative parts and pollen organs can be added from this promising material

then it will become the standard against which information on all other early seed plants will have to be judged.

In addition to papers dealing with specific early seed plants, other contributions provided overviews of the current data and interpretations of early seed plant phylogeny. One recurring theme that ran through most of these presentations (e.g. Galtier, Matten, Rothwell) was that there are strong similarities between aneurophytalean progymnosperms and the earliest seed plants. Furthermore, although by no means all early seed plants are identical, there are sufficient similarities in anatomy, branching reproductive structures and presumed reproductive biology (termed "hydrospermal reproduction" by Rothwell) that it is hard to envisage anything other than a common (monophyletic) origin of the group. The major outstanding question that then arises is: where does all this leave Archaeopteris ? Is it really possible that this plant, so central to our concept of the progymnosperms, so conifer-like in many aspects of its anatomy and with a level of heterospory seemingly so well "poised" to make the transition to the seed habit (as Chaloner & Pettitt emphasized in their discussion of Archaeopteris latifolia) - is really just a complete evolutionary blind alley? One suggested escape from this dilemma is to emphasize the possibility of a close relationship between Archaeopteris and coniferophytes (sensu stricto), but this seems to create as many problems as it solves. Not only are we then forced to accept at least a biphyletic origin of seed plants (dual acquisition of seeds and associated characters) - but we also confront a substantial stratigraphic hiatus between Archaeopteris (Upper Devonian) and the earliest cordaites (Namurian B) and conifers (leaves Westphalian B, cones Westphalian D). It will need some dramatic new discoveries to restore Archaeopteris to its former central position in seed plant evolution. Optimism about the likelihood of such discoveries clearly hinges on the completeness of our current sample of early seed plants.

Papers dealing with late Paleozoic and Mesozoic material covered a greater variety of taxa than those on middle Paleozoic plants. Many of the contributions focused on evolutionary problems within specific groups, such as conifers (Barale, Reymanowna, Stockey) or glossopterids (Pant), while others dealt with some interesting fossil woods, several of which showed highly unusual structure (Leistikow & Du, Vozenin-Serra & Broutin, Vozenin-Serra & Pons, Wilde & Goth). In addition, the thorough analyses of dispersed Allicospermum seeds from the Jurassic of Poland (Woislo-Luraniec & Ichas) provided a good demonstration of how much information can be obtained from well preserved compressions with appropriately careful technique.

Relatively few papers considered the problems of attempting to resolve relationships between major groups of gymnosperms. Rothwell presented the case for a close relationship between conifers and cordaites contrasting them with the earliest seed plants from which they were presumably derived, and I discussed some of the common features in the different cladistic analyses of seed plants that have recently been attempted. Meyen presented an overview of his group the Ginkgoopsida, concentrating on late Paleozoic and early Mesozoic representatives and appearing to

deemphasize some of the other taxa that had been previously assigned to this group (e.g. Calamopitys, Ephedra,Pentoxylon). He reiterated his view that the similarities between Ginkgo and cordaites plus conifers are convergent, demonstrating an almost bewildering variety of predominantly Permian and Triassic taxa that may be intermediate between Permian callipterids and Jurassic Ginkgo, Czekanowskia and corystosperms. Meyen's presentation and the fascinating plants described by Grauvogel-Stamm highlighted the extraordinary diversity and interest of Triassic seed plants. Many of these taxa may be critical to clarify the homologies between several Mesozoic groups. In the Permo-Triassic perhaps large pieces of the phylogenetic puzzle still seem to be either missing or at least unrecognised.
At the end of this stimulating meeting I came away with few clear cut answers but a heightened appreciation of the obvious surfeit of interesting questions that we have scarcely even begun to address. For example; what is the ontogenetic or functional basis for the apparently significant change within seed plants from tetrahedral megaspore tetrads to linear megaspore tetrads? What is the basis for the change from proximal to distal pollen germination and how is it linked to the origin of siphonogamy? What are the homologies between the different wall layers of spores/pollen and what is their developmental origin? What is the driving force for the neotenic trend in megagametophyte development outlined so well in Montpellier by Favre-Duchartre? It seems that these and other questions may yield more quickly to a multidisciplinary approach rather than the efforts of neobotanists or paleobotanists alone. In this respect the Fondation Emberger-Sauvage, and especially Jean Galtier and his colleagues, deserve our thanks for bringing paleobotanists and neobotanists together to focus on an important area of common interest.
P.R.CRANE, Chicago, USA.

IS PALAEOBOTANY GOING EXTINCT?

This question was raised in IOP Newsletter 28, December 1985 page 4, and attracted an optimistic response from Australia (IOP Newsletter 30, July 1986, page 8). The contrast of those two items has been repeated in two recent articles which appeared in the first 1986 edition of the Swedish Fauna och flora, published by the Naturhistoriska riksmuseet, 104 05 Stockholm.
One article is by Svenolov Lindgren (the articles are in Swedish with an English summary): 'Palaeobotany seems to be stably declining'. Then, Britta Lundblad has written: 'Paleobotaniken ar mer vital an nagonsin'. This is a sequel to her earlier (1985) article in the same journal 'Palaeobotany is blooming'.
S. Lindgren is at the Stockolms universitet, Geologiska institutionen, 106 91 Stockholm. B. Lundblad is at the Sektionen for paleobotanik, Naturhistoriska riksmuseet, Box 50007, 104 05 Stockholm.

PALAEOBOTANICAL SPECIES - MYTH OR REALITY?

The critique by Stein & Wight (1986; I.O.P. Newsletter 30, 5-7) of Snigirevskaya's suggestion (1985; I.O.P. Newsletter 28, 6) to habitually distinguish between 'living' and 'fossil' types was broad in its scope and, I suspect, intended to prompt further discussion. I would like to rise to the bait.

I accept Stein Wight's argument that it is difficult to provide completely reliable criteria for separating fossil from extant species, but believe that their supplementary arguments for an all-embracing biological approach to palaeobotany gloss over the particular problems faced by palaeobotanists.

To quote, "Snigirevskaya's proposal touches upon the fundamental issue of what palaeobotany is all about: are we entitled ... to use palaeobotanical evidence for biological inference, or not? If we are entitled, then let us be biologists, and like them, subject only to differences in amount of degree of available evidence." And later, "Palaeobotanists should decide for themselves which features constitute reasonable evidence for biological taxa and which taxa, based on all available evidence, are more or less (never totally) artificial."

Perhaps available knowledge of a very small number of fossil plants is more complete than evidence for the least well-defined extant plants. However, it is not available evidence that disinguishes the taxonomic abilities of a palaeobotanist from those of a neobotanist but potential evidence. A live organism is almost infinitely testable, but the limitations to testing a long dead organism are only too obvious. Stein & Wight are searching for "reasonable evidence for biological taxa" (presumably the traditional American substitution of 'taxa' for 'species') in the fossil record. They will not find it. The biological species is "a group of potentially interbreeding natural populations that are reproductively isolated from other such groups" (Mayr 1969; 'Populations, species and evolution'); it is based on genomic and breeding system information that is unattainable for the palaeobotanist, despite recent advances in the study of the chemistry, ecology and geography of fossil plant fragments.

So let us move down a level of taxonomic completeness, and examine whether we can define morphological species in fossil assemblages. The morphological species sensu Bateman& Denholm (1983: Watsonia 14, 347-376, and in prep.) requires knowledge of the intra-population variation of each putative species throughout its geographical range.

Morphometric data are obtained for all organs of each plant, and subjected to multivariate analysis. Species are then defined as groups of plants separated by clear morphological discontinuities. I do not believe that even this degree of evidence has been obtained for any palaeobotanical species.

The best data that the palaeobotanist can hope for (and even this is very difficult to acquire, as I know to my cost) are from a complete plant, painstakingly reconstructed like a jigsaw that has many pieces missing and many others poorly matched. Such a plant will make an exceptionally good type fossil, but it cannot be used to extrapolate to a genuine morphological species, let

alone a biological species; it is a plant, no more and no less, and best described as a typological species.
However, Stein & Wight are correct in one sense; the typological approach still pervades neobotany (especially when practised in herbaria), so the end-products of palaeobotanical and neobotanical taxonomy are often very similar. This is to the shame of the neobotanists, for their subjects are testable and therefore open to the application of more ambitious taxonomic concepts.
Please don't misinterpret my viewpoint. I strongly endorse Stein Wight's thesis that we should attempt biological inferences from fossil plants. I merely feel that they have chosen to illustrate this principle using the discipline where it is of least benefit, namely taxonomy, and its most reprehensible aspect, namely unwarranted obsession with types.
Those of us who study black smears on rocks that the uninitiated would sink into synonymy with bird droppings are somewhat removed from the biological species concept, and will remain so!
RICHARD BATEMAN, London, U.K.

CLADISTICS OF MARATTIALEANS: another view

I agree with Barry Thomas (IOP Newsletter 30, pages 16-17) that Hill and Camus' paper on cladistics of marattialean ferns is not an easy one to read. But unlike Barry I feel that there are various facets of cladistics deserving palaeobotanists' attention. Cladistics clearly exposes how characters and their states are selected and how the polarity of the states is treated in terms of generalized and derived states. It involves all sensible characters and all their states. As a result several competing versions of the taxa internesting are demonstrated. Some technical terms, such as apomorphy, plesiomorphy, sister group, polarity, outgroup comparison, etc. appear useful. The concepts corresponding to these terms existed even in the last century, but the absence of concise terms led to ambiguities in taxonomic discussions.
The paper by Hill and Camus differs importantly from many cladistic exercises in the due attention it gives to the fossil record. It provides a very good review of numerous characters and an interesting discussion of the polarity of the character states. Apart from this it provides a short and qualified summary of the cladistic ideology.
The cladistic fashion has penetrated into palaeobotany after a delay. By now palaeobotanical cladistics has begun to appear here and there. I am not quite happy with this because, apart from the above mentioned advantages, cladistics shows very important deficiencies, of which I will briefly mention only the most significant.
Plant taxa may be roughly divided into monothetic and polythetic (see details and references in: Stevens, 1984. Taxon, 33: 171 ff.) In the latter, no character occurs in all members, although numerous characters allow us to oganize the constituent members of such a taxon into an uninterrupted series (chain or network). Polythetic taxa are treated by cladistics as paraphyletic, hence

unnatural, although practically all larger taxa of living plants are polythetic. Most of monothetic taxa are usually monotypic (or oligotypic). That is why Hill and Camus are forced to treat gymnosperms as a cladistically unnatural taxon, although, in the phylogenetic sense, gymnosperms constitute a monophyletic and quite natural taxon (at least for me).

Terminal groups selected by cladists for cladograms are very often polythetic taxa, i.e. they have not been (and could not be) obtained by cladistic methods. But for those interesting groups into which the terminal taxa are organized, only monothetic nature is allowed. In other words, cladists wind at the polythetic nature of some taxa whereas other taxa are said to be strictly monothetic. To my mind, all taxa should be treated in the same way, if we strictly follow cladistic logic. It means that even individuals should be organized into species with cladistic methods in hand. I suspect that males and females of dioecious species will prove to be members of different species in this case.

Another negative aspect of cladistics is the negation of character weighting. The refutation of weighting is understandable within numerical taxonomy, which merely records hypervolumes of taxa within the hyperspace of characters. But cladistics has a claim on revealing phylogenetic relationships. It implies the allottment of cladistically sound characters with a casual significance. It means that cladistic analysis is a casual analysis. Since various characters are often in conflict, cladists simply calculate all "pro and contra". Such calculations are inadmissible in principle in any casual analysis which requires weighting of characters (as in criminal investigation).

Probably the above mentioned and other deficiencies of cladistics are not so harmful in the case of the Marattiales. Most of the involved characters may well be roughly of the same weight and their sample calculation may be reasonable in this special case. Most of the marattialean genera are monothetic (excepting Marattia). Therefore the internesting of genera presented by Hill and Camus deserves attention. It gives, at least, a very good identification key for the involved taxa.

Frankly, I never take classification schemes proposed by cladists as really objective and hence as having advantages compared to schemes based on character weighting. I consider that the very attribution of equal weight to all characters is a highly subjective decision. Still I confess that cladistic results provide food for thought. In this respect the paper of Hill and Camus is fairly nutritive. I read it with great interest and my copy is spoiled by numerous marginalia.

S.V. MEYEN, Moscow, USSR.

NEW PALAEOZOIC PLANTS FROM CHINA

Professor Cai Chongyang, Head of the Laboratory of Upper Palaeozoic Plants in the Palaeobotany Department at Nanjing Institute of Geology and Palaeontology, Academy Sinica, Chi-Ming-Ssu, Nanjing has recently made some exciting discoveries:

'On returning back from West Germany at the end of 1982, I have been working mainly on the Devonian megafossil plants and non-marine stratigraphic implications. During the last two years I have been to the Jungaria Basin of Xinjiang, in north west China to work on Upper Palaeozoic stratigraphy. I was lucky enough to find for the first time some petrified stems of Leptophloeum rhombicum Daw. with anatomical structure and Callixylon newberryi from the Upper Devonian.

'Additionally, Mr Wang Yi, one of the graduate students of Professor Li and myself, has recently finished his MA thesis entitled 'First discovery of Eviostachya hoegii Stockmans, from the Upper Devonian of Jiangsu, east China.' We are very excited to have found this Famennian plant in China. In our collections there are over 160 specimens with both internal structures and external morphology, which are much better preserved than the original specimens from Belgium. They are good enough to provide in detail reconstructions of different parts of the whole plant, including the rhizome, stem, polymorphological leaves and fructifications. The well preserved anatomy is shown not only in the cone axis but also in the stems. A preliminary study indicates that Leclercq's reconstruction of the cone might not be completely correct. A new emendation was given by the author.'

Callipteris IS ACTUALLY A SYNONYM OF AN EXTANT FERN

It is simply inconceivable that Callipteris, a well-known index fossil plant of the Permian, spreading far and wide in Euramerica, Cathaysia, and especially the Angara Provinces, should be a synonym of a living plant and should be declared invalid accordingly.

Here is the story about this unusual event.

Mr. Sun Fu-sheng, a young Chinese graduate student of Nanjing's Institute of Geology and Paleontology, Academia Sinica, was engaged in the study of the Angara flora from Xinjiang (Sinkiang), NW China during the past three years from 1983 to 1986.

In order to complete his M.A. degree thesis in Mid-July before setting out on a third field trip to Xinjiang, Mr. Sun was working from early morning till midnight in the lab. One night, he felt too tired and lay in bed for a relaxation, with a book: "Atlas of Plants in China" in his hand. While reading it over quickly and absent-mindly, he happended to catch sight of the word "Callixylon", a very familiar term to which he had devoted his study for several months. At first, he did not believe even his eyes, thinking that it was absolutely impossible that this fossil could have come to life and turned to a modern plant once again. After having searched for the references concerned and for help from others as well, he was so excited to find this to be true.

Although the fossil genus Callipteris was first established by Brongniart over 137 years ago (1849) and appeared in all of the palaeobotanical textbooks, it was this very term "Callipteris" that had been erected by the botanist Bory 45 years earlier

(1804) for an extant fern genus Callixylon, belonging to the Athyriaceae.
According to Art. 13 (ICBN, Voss et al., 1983, p. 12 & 13) "Valid publication of names for plants of the different groups is treated as beginning at the following dates:

Non Fossil Plants:

(a) Spermatophyta and Pteridophyta, 1 May, 1753 (Linnaeus, Species Plantarum ed.1)
(b) All groups, 31 Dec.,1820 (Sternberg, Flora der Vorwelt, Versuch 1:1-24, t.1-13). Schlotheim, Petrefactenkunde, 1820, is regarded as published before 31 Dec., 1820.

It is obvious, according to the rules of the ICBN mentioned above, that the two same names are both regarded as having been published. After the priority rule (Art. 11.2 in ICBN, Voss et al. 1983), the fossil genus Callipteris Brongniart 1849 is a synonym of an extant plant, Callipteris Bory 1804.
From this story we can learn something. It tells us that both palaeobotanists and botanists have to pay more attention to the nomenclature, and it also teaches us that some synonyms might still exist in the literature published even more than a century ago.
C. CHONGYANG, Nanjing, China.

HUNTING ANIMALS

The fossil record of terrestrial arthropods is a very patchy and incomplete affair, which is largely a reflection on the low preservation potential of arthropods, compared for example, with that of plant tissues. However, dispersed fragments of arthropods occasionally crop up in palaeobotanical preparations, particularly bulk macerations of coaly shales intended to recover larger microfossil remains such as cuticles, megaspores, sporangia and seeds.
Amongst the earliest terrestrial arthropod faunas, both the Rhynie Chert animals and the arthropods of Givetian age from Gilboa, and New York, were originally accidental finds by palaeobotanists. In each case the presence of arthropods was recognised as a result of more or less complete animals being found. Subsequent work on the Gilboa fauna has proved that dispersed fragments of arthropod cuticle are abundant enough to allow the reconstruction of some animals. The Carboniferous has also provided a number of dispersed cuticle records, most of those examined so far proving to be scorpions, including a good Westphalian B fauna found by Andrew Scott (London), and a Stephanian B locality by Gene Mapes (Ohio).
These fortuitous discoveries are shedding a great deal of light on the initial colonisation of land and the early evolutionary history of the terrestrial arthropods, but the full potenial of dispersed cuticle studies has yet to be explored. This field is still very much in its infancy. The extent to which the study develops depends upon the discovery of new cuticle-bearing localities of all ages. It is here that palaeobotanists have an

important part to play, because from both the point of view of the preservation style and original association, sediments yielding plant cuticles are those most likely to produce animal fossils as well.
Occurences of arthropod fragments are probably greatly under-recorded. They are not anything like as abundant as plant cuticles in floodplain lithologies and so are easily overlooked. Odd scraps are also frequently difficult to recognise as arthropodan, even for specialists. In those cases where animal fragments are recognised the finds are seldom mentioned in the literature the significance of these pieces is seldom realised.
The message is that the animals are there, and that it is the palaeobotanists who are likely to discover them. Every record is valuable at this stage of the game, and if only a single fragment is recovered it indicates that larger samples might be more productive. If this wealth of new information is to be tapped a close liaison between palaeobotanists is highly desirable. I would urge you all to keep a special eye out for beasties in your preparations and let us know when you find them. Whatever you do don't ignore those odd little fragments that don't look quite like plants!
If you would like a little more information on how to recognise arthropod fragments, I can supply a sheet of details on request, and I would be happy to hear of any material that turns up, whether definitely arthropod, or merely suspect. Good hunting!
A. JERAM, Manchester, UK.

OBITUARIES

FRANCOIS STOCKMANS, 1904 - 1986
Francois Stockmans who retired in 1969 as head of the Palaeobotanical Section of the Royal Institute of Natural Sciences in Brussels died on April 7, 1986. He was born in Brussels on September 13, 1904. After completion of a pharmaceutical chemist degree at the Free University of Brussels, he obtained a "Doctorat en Sciences naturelles (botaniques)" from the same University in 1930 and immediately entered the Institute as an assistant naturalist. His long career there allowed him to put his interest and enthusiasm for fossil plants into practice. He published about a hundred papers; his well known contributions on floristic assemblages in the Devonian, Namurian and Westphalian of Belgium are particularly important and widely cited. He studied also Carboniferous specimens from Spain, Carboniferous and Jurassic material from China and was interested in Upper Cretaceous, Eocene and Holocene macrofloras as as well as Cenozoic palynology. With his wife Yvonne Williere (1905 - 1979), who was his research associate and active collaborator for many publications, he started in Belguim the study of Acritarchs, as early as 1959. For 15 years Francois Stockmans also taught the Paleobotany course at the University of Brussels. He was deeply involved in raising interest for palaeobotany outside the limited circle of the scientists; many high level popularising articles, his "Initiation a la Paleobotanique" amongst others, express his constant concern for education.

In 1962 he entered the Royal Academy of Belgium and was elected as Secretary for the National Biography to which he contributed some forty notices. Francois Stockmans was modest and quiet but his infatigable work has endowed the Brussels Institute with an amazing quantity of invaluable specimens which attract many scientists. All those who have visited the collections under his guidance will remember with emotion his affability and dedication. He was a quiet and lovely person, so distressed by the death of his wife that he almost disappeared from active life for many years.
M.FAIRON-DEMART, Liege, Belgium.

G. THANIKAIMONI
G. Thanikaimoni, Head of the Department of Palynology at the French Institute, Pondicherry, India, fell victim to terrorism in Karachi on September 6th 1986.

BOOK REVIEWS

PHYTOSTRATIGRAPHY AND FLORA OF THE JURASSIC AND LOWER CRETACEOUS OF THE LENA BASIN. A.I.Kirichkova, 1985. 223 pp., 76 plates. "Nedra", Leningrad. 1 rouble 80 kopeks.
This is the first comprehensive description of the entire Jurassic and Lower Cretaceous flora coming from numerous localitites along the Lena, Vilyui and Alddan Rivers and their affluents. The first chapter provides a phytostratigaphic summary of the plant-bearing beds. In the second chapter the plant assemblages of the Lena Basin are compared with those of other regions of North Asia, Japan and North America, and relevant stratigraphic correlations of the plant-bearing beds are suggested. The third chapter yields descriptions of lycopods, ferns, cycadophytes, ginkgos, czekanowskias, and conifers. Numerous new species are described.
S.V.MEYEN, Moscow, USSR.

LATE CRETACEOUS FLORA OF KAZAKHSTAN. SYSTEMATIC COMPOSITION, HISTORY OF DEVELOPMENT, STRATIGRAPHIC SIGNIFICANCE. P.V.Shilin, 1986. 200 pp., 60 plates. "Nauka", Alma-Ata. 2 roubles 90 kopeks.
The book provides a short historical sketch of previous works, characteristics of plant megafossil assemblages, their stratigraphic and geographic settings and age assignment. A special chapter provides an analysis of the phytogeographical position of Kazakhstan in the Cretaceous. Main stages in the history of the flora are outlined. A phytogeographic zonation of Eurasia during the Late Cretaceous is suggested. Systematical descriptions are given for new species only. Other taxa are figured on plates. Most of the taxa, both figured and described, belong to angiosperm leaves.
S.V.MEYEN, Moscow, USSR.

FLORAS OF KORYAK-KAMCHATKA AREA AND STRATIGRAPHICAL PROBLEMS OF THE CONTINENTAL CENOZOIC. A.G.Ablaev, 1985. 60 pp. Far-Eastern Centre of the USSR Acad. Sci., Vladivostok.
Materials on the biostratigraphy and palaeofloristics of the area

are discussed in connection with new findings of Palaeogene and Neogene plant remains. Mostly late Eocene plant-bearing beds occur near Shestakovo (north part of the Okhotsk Sea area), and late Miocene ones along the Gusinaya River and in the Korf Bay. Plant assemblages of different landscape zones are recognised. A major part of the work is devoted to the description of plants coming from Shestakovo and Gusinaya River.
(Adapted from the summary published in "Referativny Zhurnal", 1986, No.6-127).
S.V.MEYEN, Moscow, USSR.

VEGETATION AND CLIMATE OF THE LATE CENOZOIC OF THE SOUTH OF EASTERN SIBERIA. V.A.Belova, 1985. 160 pp. "Nauka", Novosibirsk. On the basis of palynological and biostratigraphical studies supported by statistical analysis of data coming from the heterofacial Neogene and Quaternary deposits, the history of the flora is given. The investigated territory extends from the sublatitudinal part of the Tunguska River to the mountainous part of South Siberia. Reconstructions of both vegetation and climate for different time intervals are suggested. The following palaeoclimatic episodes are recognised: a climatic optimum in the first half of the Miocene, a cooling in the Late Miocene, a notable aridization in the middle Pliocene, a cooling at the very end of the Pliocene - beginning of the Pleistocene, and 5 stages of the vegetation evolution within the Holocene.
(Adapted from the summary published in "Referativny Zhurnal", 1986, No. 6-370).
S.V.MEYEN, Moscow, USSR.

JURASSIC CONTINENTAL BIOCOENOSES OF SOUTH SIBERIA AND ADJACENT TERRITORIAS. A.P.Rasnitsyn (ed.), 1985. 199 pp., 8 plates. "Nauka", Moscow. 3 roubles 10 kopeks.
The monograph summarizes main data on geology, palaeobotany and palaeozology of continental deposits of central Kazakhstan, South Siberia and West Mongolia. Reconstructions of the composition, structure, geographical distribution, environment and history of the Jurassic terrestrial and fresh-water biocoenoses are provided. Palaeobotanical chapters are: "Vegetation cover of South Siberia in the Jurassic" (Yu.V.Teslenko), "Jurassic flora of Oshin-Boro-Udzyur-Uly and Dzhargalanta (Mongolia)" (V.A. Krassilov). Concluding chapters (V.V.Zherikhin, N.S.Kalugina) provide reconstructions of the landscapes and communities, with bio-geographic zonation for the Fore-Baikalian and Transbaikalian sub-areas of the Early-Middle Jurassic. Several provinces are also recognised.
S.V.MEYEN, Moscow, USSR.

PLEISTOCENE OF CHUKOTKA. PALYNOSTRATIGRAPHY AND MAIN PALAEOGEOGRAPHICAL EVENTS. N.B.Verkhovskaya, 1986. 112 pp. Far-Eastern Centre of the USSR Acad. Sci.,Vladivostok. 1 rouble 10 kopeks.
New material on the stratigraphy of the region, succession of palynozones and their diagnoses are given. The age of the palynozones has been established on the basis of interrelationships between climatic fluctuations, development of

vegetation, marine transgressive-regressive cycles, and glaciation dynamics correlated with events established in other regions of the Northern Hemisphere. On the background of the increasing cooling, three episodes of relative warming have been recognised. Episodes of increasing glaciation fall on relatively warm epochs, and the decisive role in this process is allotted to higher snowfall above incipient glaciers probably even during summers. A new phytostratigraphic scheme of the Pleistocene of Chukotka is proposed. Key sections and their palynological characteristics are decribed in a supplementary chapter.
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