PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY B BIOLOGICAL SCIENCES

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Phil. Trans. R. Soc. Lond. B 1985 **311**, 139-141
doi: 10.1098/rstb.1985.0145

Phil. Trans. R. Soc. Lond. B **311**, 139–141 (1985) [139]
Printed in Great Britain

Problems with permineralization of peat

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I must begin by congratulating these three authors on their comprehensive and lucid reviews of the processes of plant permineralization which are both complicated and still, in certain respects, perplexing. There is little in what they say that I am competent to dispute. None the less, I assume it is my prerogative, indeed my obligation, to seek out some areas of contention, lest our discussion should devolve into mere amiable acquiescence.

It is perhaps appropriate to start by remarking on the rather obvious fact that the preservation of truly cellular detail in animal fossils is exceedingly rare (setting aside unicells, such as forams, and bone tissue); whereas in plants, even in prokaryotes, preservation of cellular structure in silicate, carbonate or pyrite is not all that uncommon. Such preservation is, inevitably, in some degree concomitant with the possession of cell walls. It is also, for palaeobotanists, one of the huge compensations for the relative rarity of plant fossils, compared with animals, throughout the geological record. However, although permineralized plants have now been studied for over 150 years, we still know remarkably little about the processes resulting in this permineralization. There are two main respects in which we remain ignorant of the permineralization process. First, even now, there is no general agreement about the source, either of the silicon in silicification, or of the cations (calcium, magnesium) in coal balls. There is continuing debate about whether the mineral matter came in each of these rather different cases, from 'above' or 'below', and indeed whether in each instance a single common process is involved. Secondly, we still have no present-day environment that offers a model for either silicification or calcification resulting in cellular preservation of higher plant tissue in a swamp environment.

A general point that I would make, which applies both to Knoll's and to Scott & Rex's papers, is my (probably minority) view that they take too broad a concept of what constitutes peat. I would, for example, be reluctant to accept that any Precambrian occurrence (for example, a silicified assemblage of prokaryotes) constitutes 'permineralized peat'. Nor would I acknowledge that the concept of peat must embrace all accumulations of plant-derived material. I would not, for example, refer to dry seaweed lodged on a strand-line as (even, incipient) peat. A fairly generalist definition, by Whitten & Brooks (1972) gives peat as a 'partly decomposed mass of vegetation which has grown in a shallow lake or marsh'. James Schopf (1966) offers a more technical but broader definition of peat, including the stipulation of its being 'partially fragmented and decomposed', but without the implied *in situ* formation of Whitten & Brooks. My plea is merely that we should perhaps refine our terminology to avoid lumping together such diverse plant accumulations as the algal and bacterial scum of an intertidal mud surface, or undegraded higher plants debris, with the diagenetically altered fuel long dug from Northern European peat bogs.

There are two items in Knoll's presentation that I would mildly query. The first, arising from the point just made, is his emphasis on the implied common process of formation of, on the one hand, Precambrian cherts rich in microorganisms (Gunflint, Bitter Springs and many others) with terrestrial deposits rich in vascular plant material, such as the Devonian Rhynie Chert, the Tertiary Intertrappean Chert in India and the Clarno Chert of Oregon. The latter all occur in suggestive association with volcanic series – lavas, pyroclastic, or both – which seem in varying degree to be implicated in the silicification process. In the former, there is not even circumstantial evidence for a direct volcanic source of the silica. While these diverse instances of silicification may have mineralogical features of similarity, the source, process and timing of their permineralization, in relation to the growth of the plants, seems to have been significantly different.

I also have some concern over the concept that *all* these silicified *in situ* plant accumulations fall in the category of 'evolutionary museums', as distinct from 'cradles of evolution'. (I construe the 'museum' metaphor here, I hope correctly, as a fixed collection of biological objects, no longer contributing to the mainstream of evolution.) I freely concede that marsh and swamp environments do present certain constraints on the plants living in them. The niches offered may be narrow, and those species that occupy them no doubt fit the adaptive specification tightly. That is to say, plants from other habitats are unlikely to challenge their hold, while the swamp plants are unlikely to have much capacity to colonize other, significantly different, environments. Does this specialization make them innately 'conservative' in an evolutionary sense? While the late Carboniferous swamp flora may have contained few of the 'seeds' of the ensuing Mesozoic ('Mesophytic') flora, it can hardly be said to represent a Devonian 'relict' flora, with respect to other contemporaneous plant communities. I am hesitant to accept the idea that *in general* plants of lowland, swamp vegetation have a relictual character, indicating some innate evolutionary slow-down in that particular type of environment. Modern bog communities include, for example, a number of flowering plants (*Dionaea*, *Drosera*, *Utricularia*) with elaborate and complicated structural adaptations to insectivory. This is one of the distinctive strategies for dealing with the nitrogen deficiency common in swamp and bog habitats. The status of these insectivorous angiosperms cannot be said on any basis to be 'primitive'. There is strong circumstantial (and doubtless sound) evidence for believing that major plant innovations (the Palaeozoic rise of conifers, the Mesozoic rise of the angiosperms) were recruited from small populations away from the lowland habitats that 'favoured' fossilization. But I think we should be guarded in relegating all members of plant populations of lowland peat-forming communities to 'evolutionary museum' status.

Turning to Scott & Rex's treatment of coal ball formation, I believe it is fair to say that certain aspects of the process seem even more enigmatic now than they did when Stopes & Watson first proposed their hypothesis of marine control. The absence of coal balls (but not of silica permineralization) in all the European limnic basins, as well as in the Gondwana Permian coals, certainly adds credence to the plausibility of a marine source for the permineralizing cations. But whether coal balls formed by means of one, two or all three of the processes summarized in Scott & Rex's figure 1, brings us no closer to an explanation as to why calcification has not occurred in any subsequent bituminous coal, lignite or peat-forming environment. The extension of our knowledge since Stopes & Watson's time has emphasized a further perplexing dimension in coal ball distribution, as Scott & Rex acknowledge: while all European coal balls (outside of Russia) occur before the end of the Westphalian B, coal ball formation in North America was only starting at about that time. We are still without a single unifying hypothesis which explains all known aspects of coal ball distribution. Our most pressing need is perhaps for a more directly experimental approach to test the embarrassing wealth of hypothesis based on field and laboratory observation of the fossil material.

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