

TWELFTH BIRBAL SAHNI MEMORIAL LECTURE

WHAT IS PALAEOBOTANY FOR?

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I WAS deeply honoured to be invited to give this year's Sahni lecture and touched to find my room in this building labelled "T. M. Harris, Birbal Sahni Professor"; I am one of the diminishing number of his friends; he was born not many years earlier than I. I feel he might have been still alive if he had worked less hard. Well he was inspiring and a man of courage and I must give a cheerful lecture. But I am an old man and cannot tell you about new things as a young man should. Instead I look back and think and I ask myself the question "What is Palaeobotany for?". It is a question I have asked myself many times; when young I was an ardent collector of all sorts of things, beetles, shells, plants as well as fossils and I knew I would soon turn to something else. So when I asked 'why am I doing it' I thought a minute and went on, not at all discouraged by my failure as a philosopher.

I shall talk about a few pieces of work by others; they are as I consider worthwhile because they were done with courage and should give courage. But that and the tiresome state of fossil plant material is all they have in common. The state is important. It gives men who work on fossils sympathy with one another.

I begin with some fossils barely 10,000 years old from a marshy field in Yorkshire, the field is called Star Carr. All the plant species still live in Britain and 10,000 years would seem a moment at an earlier period, not more than one Ammonite zone. But it was at a time of rapid change, especially of climate for only 3000 years earlier the last glaciation was at its maximum and in Britain most of the land north of London was covered by ice. South of London we had permafrost, the land was deeply frozen in winter but the surface thawed to give mud in summer. I have lived in permafrost in Greenland for a year, life is indeed possible

and there are bright flowers but trees cannot survive on land that flows down hill each year. Along with glaciation there were notable changes in sea level and Britain was widely connected to N. Europe.

The other great change was in human technology. As I understand our species long existed as a rare animal with bones indistinguishable from those of modern man and then at first very slowly and afterwards with increasing rapidity developed new tricks and remembered them, and as he added new ones became more numerous. Thus ten thousand years ago, while these animals and plants preserved at Star Carr are all essentially modern, the way the people lived had changed and would change again. This is Archaeology and is of wide interest. It was appropriate that the main work was done by the archaeologist Clark of Cambridge but he made use of Botanists, Zoologists, Geographers and others, a happy collaboration which should be normal but is rare. And there was early help from two men in jobs where you would hardly expect scholarly treatment of scientific evidence; the skipper of a trawler and a road engineer who dug ditches. And finally there was a strong smell of scientific scandal; to save disappointment I say at once the scandal proved baseless, everyone had been honest but it undoubtedly roused interest in Star Carr.

I will tell the story as a history. I had only faint concern with archaeology and I thought of Englishmen before the last glaciation, dimly seen creatures, living as hunters and called Palaeolithic man. After the glaciation I called them 'Neolithic' herdsmen but this was too simple and for a good 3000 years after the maximum they were still hunters and are called Mesolithic. The 'lithic' part of the word refers to the fact that almost the only things which survived decay were their stone tools and

even these are rare and scattered. Archaeologists recognize types of stone tools and name them but stones tell very little about the people who used them, particularly as a flint, that is a chert knife, picked up in a ploughed field has no attached date.

The first relevant fossil found in England was a barbed spearhead, made not of stone but of deer antler and dug up in 1903 not far from Star Carr. It was sold to a private collector who just kept it for twenty years. Also in 1903 an able archaeologist excavated a bog at Maglemose in Denmark and he got numerous barbed spearheads and also various stone tools. He did his work well and his find became known as the Maglemose culture, now known to extend from Spain to Siberia and limited to a period of a few thousand years after the glacial maximum, the Late Glacial. When the Yorkshire collector showed his specimen in 1922 it was denounced as a forgery. Archaeology is only too open to forged evidence. In 1932 the trawler skipper brought up a great lump of peat off the Yorkshire coast (and the occurrence of peat was well known round there). Instead of just throwing it back he cut it with a spade and found a beautifully preserved barbed spearhead. He recorded the exact place where he got it and handed it to the nearest museum. Harry and Margaret Godwin got a peat core from the sea bed just there and showed that the pollen indicated early Post glacial; tree growth was at its beginning. Then in 1947 the road engineer saw a flint knife in a newly cut ditch at Star Carr, noted the place and also handed it in. The finds were duly reported and Clark realized that Star Carr might give him what he wanted. He worked there for several years with volunteer labour of undergraduates. He was digging in wet peat formed by a reed swamp at the margin of a great lake which existed there in the Late Glacial. The lake had been dammed by ice and moraine and later was to vanish. What they uncovered was the site of a tiny settlement only a few metres wide and built of tree branches laid roughly on reed swamp. Towards the lake there was a track of wood going through a water lily belt to the open water. The platform was made casually of irregularly laid birch branches and lumps of clay and stones but they seem to have had thoughts of comfort as there are local piles of moss. The

platform could not have had more than about four small skin tents, a tiny settlement indeed and not a village but so was the Danish original Maglemose.

I show you a plan of the platform in relation to the lake and a picture of the platform as excavated. It seems that the site was used for quite a long time and many thousands of flints, the local form of chert, were collected and hammered there to make useful tools. The hammer and anvil stones were found. Most of the flint tools were spoilt. We know they were made there because the flakes can be fitted together. Some hundreds of finished flint tools were found and they are of a dozen kinds, little sharp arrow points, skin scrapers, augers for boring holes, saws and special tools for working deer antlers and some of unknown function. I show you a group of skin scrapers and some large mattock heads made from the antlers of the extinct giant deer, the Elk. Plenty of spoilt deer antler spearheads are available, so we know how they were made. The antler was first scratched deeply with flints, then the surface between the scratches was split off and then the splinter was filed till barbed. I show you one group of spear points. These are sometimes notches suitable for attachment to sticks.

Now for some inferences. The little platform about 20 m across was occupied only as a winter camp. We know this from the deer antlers. Deer shed their antlers every year and as the new ones grow they change and the state can be recognized even when fossil. Huge numbers of deer were slain and smaller numbers of other wild animals, cattle, horses and small ones also. The bones suggest that they collected every kind of creature for food. But unfortunately there are no human bones. Several of the Star Carr plants are eaten by people in Europe when hungry but there is no fossil evidence that they were eaten in the winter camp. It seems that the camp was used over a long period, Clark estimates more than 100 years and changes in style of tool occurred. Then though life was doubtless precarious, and Clark suggests that the wild animals of Britain could scarcely have supported more than a very few thousand people in tiny groups of families, we need not suppose they were unhappy. They did things for

pleasure, they bored holes through teeth and bits of amber for necklaces (you can find amber today on the coast) and they made themselves hats out of the tops of deer heads. I show you a picture of a modern hunter from Arctic Siberia wearing just such a hat for a ceremonial dance and he plainly enjoys his life. But I must warn you, Clark was a pioneer worker and a man a century hence would find other things when he knew what to look for. We still know little about the lives of my Mesolithic ancestors who lived at Star Carr.

I have barely mentioned Godwin's work on the pollen at Star Carr and the work on the undersea peat where the earlier spearhead was found and I will not give any details. It was standard work; first the pollen of certain trees *Betula* and *Pinus* is considered, *Betula* being by far the commoner. We know that all over Britain in the late Glacial this was true. But then *Pinus* became the commoner. And after considering these trees, made to add to 100%, other kinds of pollen are considered in relation to them; the shrub *Corylus* and this briefly becomes enormously abundant just at the time of switch from *Betula* to *Pinus*. And then the herbaceous plants, mainly grasses and Cyperaceae are taken and they are more abundant than the trees at Star Carr but later on when *Pinus* established itself as forest they become few. Later still warmth loving trees *Ulmus*, *Quercus* and *Tilia* replace *Pinus* and *Betula* but not at Star Carr; that belongs to a brief phase of the late Glacial. It is a phase dated by radiocarbon to just under 10,000 years ago.

I now take a research in the early Tertiary, a thing I would not have done twenty years ago when the Tertiary gave me bad feelings; I was unconvinced but knew I had no right to judge. But I did recognize that the fossils were worthy of study and should illuminate flowering plant taxonomy. So I remained silent, but things have changed.

Tertiary plant fossils are mostly bits of dicot trees, the separate leaves, fruits or seeds, pollen grains and pieces of petrified wood and each organ is determined by comparison with organs of modern plants. But not exactly as in the Quaternary because we are becoming sure that most of the plants have changed a good deal, at least since the early Tertiary so the comparison

is a general and vague one with several living species or genera and when you decide that agreement of a leaf with *Ficus* say, is impressive you cannot be sure the fossil is of this genus. I think about every known genus of Eocene mammals has vanished and most without modern descendants. Plants may be more stable and secure; but perhaps the fossil mammals are merely better understood.

The new Tertiary work I enjoy starts like the old style work that I do not. It takes the organ, say a leaf, and compares its shape and veins with various living leaves. The old style work stops there but the new goes on. It uses a second kind of evidence unrelated to leaf shape; the pattern of epidermal cells. In a family or in a genus we can recognize a basic leaf shape and veins but there are also widely divergent species. So it is with the epidermis. Then a peculiar seed may be noted in association with the leaf. Association means that the leaves and seeds were produced near one another at the same time but when they are found together repeatedly in different localities of rather different age, it is impressive and when there is no rival organ in those localities, I am deeply impressed and would say so.

The paper I select out of many is by the Americans Crepet and Daghluin who early this year described what they called 'Euphorbioid inflorescences from the Middle Eocene Clairborne Formation'. Note their restrained wording, it continues through their paper. When men of science feel they have a strong case they often write modestly. The inflorescences are branchlets bearing tiny male flowers they name *Hippomanoidea* and compare with various members of the Hippomaneae a tribe of Euphorbiaceae trees of tropical America: I had never heard of them. In particular they compare with *Gymnanthes*. They give pictures of *Gymnanthes* inflorescences and their fossil, they plainly look similar but are the tiny and very simple flowers really similar? The fossil is imperfectly seen. But the fossil gave excellent pollen grains and these are compared with various Hippomaneae, some are strikingly similar, some not and this is usual. Pollen grains evolved like every other plant organ. If I had been sent two separate papers to referee; one on the inflorescence and one on separate pollen I

would have been unimpressed by either, always remembering that I am ill informed about pollen and about tropical American plants. But when I take the two together and the authors' statement that there is nothing else in the world which shows this combination of characters I am deeply impressed. I recognize that we do not have certainty, Palaeobotany is not for those who require certainty, but there is good probability on which I could build. The authors do build a little; they point out that it is odd that the Hippomaneae, regarded as an advanced tribe should occur so early.

It must be sixty years since I considered whether I would give my life to Tertiary plants and decided — no. I would have poor respect for my own determinations. But I could work happily with determination based on two different kinds of evidence. You may well say, the second kind of evidence commonly is not there; what then? I would do something else.

I next give you Krassilov's paper of 1975 on *Dirhopalostachys*, a cone from the Upper Jurassic and Lower Cretaceous of eastern Siberia. I say at once, he was not responsible for this long word and I also say that such words are one reason why lay men dislike science.

In the Mesozoic most of the pteridophytes fit into living families but most of the gymnosperms do not, or not easily, so their study is one of bringing a strange plant to light, first as separate organs and then assembling them. Krassilov attributes the leaf *Nilssonia schmidtii* to the same plant and he has a strong case; repeated association, some agreement in structure and no rival associate.

The cone is about 12 cm long and 2 cm wide. It has a slender axis bearing short horizontal stalks in a loose spiral and these fork into two capsules. The capsules are closed and each ends in a pointed beak, eventually they split open along one side and expose and then drop a single seed; careful dissection sometimes demonstrates the seed in a closed capsule. The substance of the fossil is fragile and its cuticles are delicate and Krassilov had great trouble in making out its structure though he had plenty of specimens.

A *Dirhopalostachys* cone looks like some sort of Angiosperm fructification, Krassilov

compares it in particular with the Upper Cretaceous *Trochodendrocarpus* but we do not yet know anything about pollination. Surely the beak played a part, either as a stigma or opening sufficiently to admit pollen grains. The only grains found were on the capsule surface. Krassilov sees it as one of several Jurassic and Lower Cretaceous gymnosperms which were advancing on a broad front towards angiosperms, two others are *Caytonia* and *Czekanowskia* and he calls the whole varied assemblage 'Proangiosperms' in the way Devonian plants of partly gymnosperm character are called Progymnosperms. He favours the idea that angiosperms had several gymnosperm starting points. I will not follow this idea but a comparison he makes with the Jurassic cone *Beania*, also linked with *Nilssonia* leaves. *Beania* is a more robust and easily studied fossil. It is similar, but instead of the lateral branches bearing two capsules, they form a broad somewhat lobed scale with two naked ovules on the upper side. To Krassilov this is the primitive condition of *Dirhopalostachys*; the halves of a *Beania* scale have wrapped themselves round the ovules; an idea I can accept. We have more or less good knowledge of several kinds of *Beania*, always in association with a *Nilssonia* leaf, the first known and perhaps the oldest from the Rhaetic of Sweden was provided by Nathorst in 1909 though not under that name. There was one in the Greenland Lower Liassic, there are three in Yorkshire, all rather similar and I only described the two better known by name. I found them rather discouraging, they did not apparently help with the appalling problem of taxonomy of *Nilssonia* though the two better known are to be counted as among the more completely known of Jurassic gymnosperms having also their male cones and scale leaves. There are no stems but Kimura in Japan found *Nilssonia* leaves attached to a slender stem, I imagine that this stem had been torn off by violence before the time for leaf abscission.

*Nilssonia* is one of the most tiresome genera of N. Hemisphere Mesozoic leaves but not as far as I know in Gondwanaland; India is lucky in this. Specimens are uncommonly hard to group in species. There is great variation in each species, and a huge number of species are distinguished by

differences less than between the right and left sides of a single leaf. But I do not say the appalling pile of binomials is nonsense; some of them do correspond to something real. When as often happens you get hundreds of leaves in a bed varying around a central type you feel sure they represent one taxon and this is reinforced when you can prepare the cuticles: they also range around a mean. But most *Nilssonia* leaves have delicate cuticles and they are often ill-preserved. Occasionally the cuticle gives a clear answer, Krassilov's *Nilssonia schmidtii* looks like *Nilssonia kendalliae* from Yorkshire but their cuticles do distinguish them; it seems the plants were very different, one had *Dirhopalostachys*, the other is associated with an unnamed *Beania*.

So we have a pile of *Nilssonia* names almost without stratigraphic value because similar looking leaves occur widely and over a long range. My own work in Greenland and Yorkshire has contributed about twenty species that have been of no use to anyone so far and people add more every year. Even more than with *Beania* I felt discouraged, I felt I was dealing with endless local variants of a nearly uniform plant. But Krassilov has changed this: he has demonstrated variety in the plants producing *Nilssonia* that is at least of generic value (he gives it family value) and if these are fruits of two genera why not more? If I were young I might go back to Yorkshire or Greenland and work hard in a bed rich in a *Nilssonia* and search for other organs. And in my experience with *Beania* they should turn up, occasionally, perhaps once for every thousand leaves. I would work in a bed where there were leaves in thousands. Quite apart from anything about Pro-angiosperms, Krassilov has made the Botany of the *Nilssonia* plants worth pursuing; he has roused courage.

My last exercise deals with plants of the morning of life on land, or if as some hold plants lived on land far earlier, they are too little known to speak about. Those I shall take are Lower Devonian. Certain Devonian plants are aristocrats among fossil plants, excellently preserved, excellently studied and described and their descriptions are written up again in reviews and textbooks. But far more are miserable specimens of what seem very simple organisms and more than with most fossils what

interest they have come from the writer's mind.

I deal with some problematic gametophytes. Many feel that the simpler Devonian plants should have similar looking generations and *Rhynia gwynne-vaughani* was thought likely as the gametophyte of *R. major*, an undoubted sporophyte. *R. gwynne-vaughani* lacked sporangia but there are many little pits on the aerial axes. They were first considered to be wounds, but some in section do look like certain pteridophyte antheridia and archegonia, dead ones surrounded by brown cells. Three authors independently offered them as antheridia and archegonia but their different interpretations of the whole plant were confusing and the recent find of sporangia by Edwards seems to end the matter. But almost at once and in the last two years there were two attempts; by Schweitzer of Bonn and by W. & R. Remy of Münster and they deal with the little Rhine-land fossil *Sciadophyton*, known since 1930. *Sciadophyton* is gregarious and forms rosettes of radiating branches. Hepatics growing on mud often look like this but those who have studied *Sciadophyton* conclude that the branches were round in section and the plant was attached by a central disc from which the branches grew upwards and outwards. A peculiar feature is that some branches end in a sort of flat umbrella, rather like the antheridiophore of *Marchantia* and the umbrella certainly shows small black spots which could be antheridia but are they? Schweitzer and the Remy's think they may be, but for different reasons.

The Remy's rely on a plant they found in the Rhynie chert called *Lyonophyton*. I will refer to it soon. Schweitzer considers he has later stages in the umbrellas. He considers that the small marginal spots are antheridia, the larger inner ones archegonia and that after fertilization the umbrella flops onto the mud and several sporophyte axes grow out, looking like young *Sciadophyton* branches, from the central disc. When rather larger they are the fossil called *Drepanophycus langi* and when much larger and tall they become *Zosterophyllum rhenianum* and bear sporangia. Schweitzer who is bold and cheerful reckons that he can see the early sporophyte stages among the figures that the Remy's offer as gametophytes. He offers a life cycle diagram.

The Remys' *Lyonyphyton* is like the umbrella heads of *Sciadophyton* but excellently preserved by silica impregnation. It gave me much pleasure that the old Rhynie chert, first studied just before I became a student, should still be providing new things. The heads are on rather long stalks a mm thick and their margins are lobed and raised. Again there are black spots on the upper surface but this time there is more. The best do show the characteristic structure of antheridia containing spermatocytes, and even it seems nearly mature spermatozoids. Do not worry about the preservation of nuclei in this chert, other Rhynie plants show them, even chromosomes. An unusual feature is that the wall is two cells thick.

The archegonia are less satisfactory but are held to form a small radiating group in the centre but all I can say of the few sections showing one is that while it can well be a section of an archegonium, it is not the section cut in the best possible plane. There is no young sporophyte and the fossil though remarkable leaves us asking for more. But it does fit the idea that the *Sciadophyton* umbrellas may also be bisexual gametangiophores. All this in the last two years, is too recent to have been controverted.

If the young men of Palaeobotany are like what they used to be they will be dissatisfied and will seek for new evidence and destroy or, I rather hope, confirm the ideas of these three Germans. Either way there is progress.

I now face my opening question, 'what is palaeobotany for?' using these four exercises and must not shirk answering it.

All four are, in modern phrase 'on coming', they should cause further work and the oldest, Star Carr certainly has. The palaeobotany of Star Carr stands apart in that it was usefully applied to Archaeology. It is easy to see what applied science is for. When a spore man works for an oil firm, his employers think his results worth his wages. But the other three in pure palaeobotany only lead, as far as I can see, to more pure palaeobotany. I judge that each of the four was good: vigorous, to me inspiring and palaeobotany is doing well when such work appears but still I have misgivings about our future. I think of palaeobotany as a living plant species, I do

not know what *that* is for but all plants act as though *they* know what they are for, their job is to live as well as they can, and to propagate their kind so to be replaced when they die. They do this in an environment partly physical and partly biological, mostly of other plant species in competition.

We know that nearly all species of one era have vanished by the next, and I imagine that the cause is in the changing environment and the danger I fear for palaeobotany is in its environment. I limit myself to Britain where our Universities are the sole environment for recruitment and the main one for research. Just now our Universities are in financial straits, they had over expanded and there is national stringency. Staff are leaving without replacement. A plant analogy is a miserable period of widespread cold, you cannot know when it will end, may be next year or as some hold it may be the start of the next glaciation. Palaeobotany found its optimum climate in Britain early this century. The discovery of pteridosperms was still exciting and a Botany lecturer felt that in palaeobotany he had a fair chance of reward from his research labour: what he taught was what his department needed, descriptive work on various living plants, fossils would be a small fraction.

Fossil material for research is still available and with the advances in technique perhaps more promising than formerly but I fear failing demand for lecturers in general and descriptive Botany. You will find very little palaeobotany in the respected old journals today and none of the kind of descriptive morphological Botany that there used to be. I suppose there is less exciting material from living plants in this largely explored world. I cannot think of the discovery of any new genus of living gymnosperm since *Metasequoia* but only new genera by dreary splits, former sections raised to generic rank. When departments can make new appointments it is likely to be men in fields fashionable today.

If a young lecturer who proposes to work on fossils is appointed it will be because he is already inspired and the committee can see possible interest. Both will come from recent papers of the right kind, ones read by those outside palaeobotany, inspiration is all.

I tell you my history which began when palaeobotany was declining in Britain. It was not Cambridge or Seward who inspired me; Seward gave me research opportunity and help. I had taken an external degree of London University at the small college at Nottingham and there Harry Holden, of whom you may not

have heard, inspired me. Holden was trained at Manchester where Weiss and Marie Stopes were working; earlier there was the great W. C. Williamson their first professor. He had been inspired as a child by his father, a gardener with interest in natural history. The thread is thin.

