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Site 820 and the evidence for early human occupation in Australia

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Kershaw *et al*. (1993: 113) have recently claimed that charcoal and pollen evidence from Site 820 of the Ocean Drilling Program "adds substantially to the case for Aboriginal burning and an early date for human arrival in Australia". This claim has been quite widely accepted and reported in the media as fact. 8This note discusses three major aspects of the claim -- the charcoal evidence, the pollen record and the depositional situation -- and suggests that it is insubstantial.

Background

Site 820 consists of two cores, 820A and 820B, drilled at the outer edge of the northeast continental margin, about 60 km ENE of Cairns, near the Grafton Passage and only about 5 km from the 100m undersea contour. Drilled beneath a water depth of 280 m, the combined depth of the cores is some 400m. The base is dated, by correlation with oxygen isotope stages from deep-sea cores, to ca. 1.4 mya (Peerdeman *et al.* 1993: 163). Kershaw *et al.* studied 44 pollen samples taken over the total core depth, with a minimum of 50 identifiable dryland grains being counted for each sample. Five samples come from the upper 37.5 m, covering oxygen isotope stages 1 - 6, i.e. the last 140,000 years (Peerdeman and Davies 1993). The dates attributed to these samples are not disputed here.

The pollen record

Kershaw *et al*. note (1993: 107) that many grains could only be identified to family or genus. They also report (1993:109) that the pollen record may be skewed, both generally in that "most grains were water transported" and particularly, for example against the representation of rainforest angiosperms, by the recovery methods used.

The claim for evidence of human presence rests in part (p.111) on the "dramatic decline" in *Araucaria* and increases in Myrtaceae and Poaceae, which indicate regional replacement of rainforest by open sclerophyll.

The *Araucaria* decline, from consistently high values from samples 44 - 5 to low to non-existent values in samples 4 - 1 (Stages 5c - 1), is certainly dramatic, but it is curious to find that it doesn't seem to be reflecting the same trends as are noted in Lynch's Crater, only 100 km away and in the drainage of a river feeding into the Grafton Passage. There, *Araucaria* pollen disappears when charcoal values soar at about 38,000 BP, but is also low in the last Interglacial levels (25 - 40 m) when charcoal is almost non-existent (Kershaw *et al.* 1993: Fig 4) and no human influence is claimed.

Myrtaceae in Site 820 show a substantial rise on previous values in samples 4 - 1, but also does not show the same trends as at Lynch's Crater, being there strongly represented in the last Interglacial levels.

Poaceae in the Site 820 records show two major periods of high values, one in the top four samples and the other in samples 39 - 40, dated to well over 1 mya.

The charcoal record

A main part of the claim for human presence in this core is "the sharp increase of charcoal at this change, followed by the maintenance of relatively high values" (Kershaw *et al.* 1993: 111), which suggests burning was responsible for initiating and maintaining the vegetation changes discussed above. These changes are visible in pollen sample 4, taken at a depth of 26 mbsf and thus within oxygen isotope stage 5.

Although Kershaw *et al.* claim that the increase in charcoal in sample 4 is continued from then on, this is not supported by the only data provided, that in Fig.2, as the following table shows:

Sample	Depth (approx)	Charcoal as % (approx) of dryland identifiable pollen
1	0	3200
2	9	1550
3	20	1350
4	26	5000
5	36	1350
6	47	1600
7	57	180

In fact, sample 4 seems to be a single, unusually high value rather than part of a trend. Given that the majority of the pollen, and the charcoal, is water transported, a chance influx deriving from a range of possible causes seems more likely.

The unusually high quantity of charcoal in sample 4 is not unique. A similar value occurs in sample 38, which comes from slightly more than 350 m down in the core and thus must date to more than 1.0 mya. It is not clear why, if sample 4 derives from human burning of the landscape, sample 38 is not also claimed to do so. The presence of sample 38 strengthens the suggestion that sample 4 is an example of an occasional occurrence.

The depositional situation

Kershaw *et al*. (1993:107) note that site 820 is situated near the Grafton Passage, which is likely to have carried river flows, including pollen and charcoal, to within a few km of the site at times of lower sea level and may be a current locale for sediment transport. They do not discuss the implications of this, such as the possibility that sediment deposition, including pollen and charcoal, might be intermittent and variable in the short term. Peerdeman and Davies' detailed analysis (1993: Fig 9) of the upper 35 m of core 820A show that sedimentation rates do vary from a high of 1 m/2300 yr in the Holocene to a low of 1 m/ 5800 yr in the early last Interglacial (stage 5e) and changes in these rates are not related directly to climatic regime. They also show that the sediment samples vary in their constituents, and it may be assumed that this is the effect of depositional processes, such as variation in river flow, tides and currents. Similar processes may well have affected the quantity and nature of pollen and charcoal deposited. In the light of these factors, Kershaw *et al*.'s assumptions (1993:109) that "the influx of this component [identifiable dryland pollen] was relatively constant through time" and that charcoal values therefore "present a realistic impression of fire activity" must be reconsidered.

Discussion

Kershaw *et al.* claim that the increase in charcoal and change towards a more fire-promoting vegetation at Site 820 are similar to changes seen by Singh and Geissler (1985) at Lake George and are "remarkably similar" to them in date. It is unfortunate that they do not discuss Wright's proposed re-dating of Zone F at Lake George (1986), which put the changes with which they claim correlation at only 60 kya and perhaps within an interstadial rather than an interglacial. This date for Lake George is very close to recent thermoluminescence dates for deposits clearly containing archaeological materials (Roberts *et al.*1990) and may suggest that the Lake George data do indeed reflect some human impact on the landscape. Whether this temporal correlation should lend strength to the Lake George data needs discussion elsewhere.

Kirch and Ellison (1994) have recently argued that palaeoenvironmental evidence is a better guide than archaeological in determining the age of human settlement of Remote Pacific islands. They draw attention to several requirements of this evidence, including the need for a carefully designed research program using a range of data to reach a conclusion. They note in particular that a *sustained* (their emphasis) and considerable increase in charcoal in palynological cores would be a strong pointer to human presence in situations where natural fires were previously extremely rare. I believe that none of these criteria are satisfied by the claim for human presence in Site 820 and suggest that this claim can be set aside until considerably stronger evidence is presented.

Acknowledgments

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Site 820 and the evidence for early human occupation in Australia - a response

A. Peter Kershaw

I am pleased that Peter White has taken the time to examine the basis of inferences made from an examination of pollen and charcoal in the Ocean Drilling Program Site 820 (Kershaw *et al.* 1993) and bring up questions that may be bothering a number of people. I attach a summary of the pollen record (Fig. 1) to help readers follow my responses to the questions raised.

The major feature of the pollen diagram is a dramatic decline in *Araucaria,* a present dominant of moist rainforest vegetation in southeastern Queensland, extending in patches to the northeast of the state, including islands on the continental shelf. In the study area, which includes the core of the humid tropics between Cairns and Innisfail and extends inland to the Atherton Tableland, *Araucaria* exists only as a few individuals in widely separated stands. The decline of this taxon is significant in that it has been recorded as a major and consistent component of the vegetation of the region back to about 1.5 my in this core and to at least the Late Miocene in Site 823 in the continental trough to the east of Site 820 (Martin and McMinn 1993). Possible explanations for this decline include a change in sediment transport and depositional processes, climate change, the tail end of a long term replacement process of fire sensitive by fire promoting vegetation and burning by Aboriginal people and, of course, a combination of the above.

Peter White examines the changing nature of sediment type and sedimentation rates and their possible effects on pollen deposition. These influences have not been addressed in interpretation of the pollen record as the data were not available to us when the paper was written. However, despite variations, it is clear that *Araucaria* is well represented in all phases of climatic cycles prior to about 140ky and poorly represented in both glacials and interglacials since this time. It is also likely that some changes in sedimentation rate and type are a result rather than a cause of the vegetation change. The record shows a sustained increase in mangrove pollen corresponding with the *Araucaria* decline which has been interpreted as an expansion of mangroves on sediments eroded from the catchment due to the destruction of rainforest vegetation, previously relatively stable for the past few million years. This erosion would have had some influence on sediments crossing the continental shelf. There is little doubt that erosion rates would have increased as massive soil movement has been demonstrated for much of the region with the replacement of rainforest by sclerophyll vegetation during the height of the last glacial period (Hopkins *et al.* 1993). Another factor that could have influenced the pollen record is a change in stream channelling across the continental shelf. It has been hypothesised that most pollen is, and would have been, carried by the major rivers, the Barron and the Mulgrave/Russell, and then channelled through the Grafton Passage to the site, but this has yet to be proved. However, if the pollen source was to have changed, this would most likely have occurred with the formation of the Great Barrier Reef, about 400 k.y. (Peerdeman *et al.* 1993).

Concern is expressed about the different patterns of *Araucaria* and Myrtaceae representation in the Atherton Tableland and Site 820 records. Differences can be largely explained by a consideration of the depositional settings. At Lynch's Crater, pollen is derived largely from the immediate catchment and the fluctuations in representation of *Araucaria,* until about 40 k.y., sensitively reflect the movement of vegetation belts across the Tableland in response to changing moisture levels. The marine core, by contrast, is likely to be reflecting the vegetation of the whole region and, despite changes in distribution and abundance of communities with climate change, all would have been present through most of the recorded period. With the Myrtaceae, the high values during the Last Interglacial at Lynch's Crater are from rainforest taxa which have limited pollen dispersal. The local occurrence of one taxon, *Xanthomyrtus,* which makes up a substantial component of the Myrtaceae pollen, has been demonstrated by the presence of wood in the marginal swamp sediments (Ann Bohte personal communication). It is interesting to note that this genus is no longer present in Australia. Almost all these Myrtaceae grains, unlike *Eucalyptus,* are small, and their poor representation in the marine core sediment samples is probably due to their selective elimination in the sieving process during pollen preparation as well as to their limited dispersal.

The difference in age between the sustained declines in *Araucaria* in the terrestrial and marine records is significant. The declines are real in that there is no major source for araucarian pollen within the region today.

The known regional extirpation of two genera, *Dacrydium* and *Xanthomyrtus,* which are no longer present anywhere on the Australian continent, within the last glacial cycle adds weight to the proposed extent of the vegetation changes. Although the coarse sampling interval in the marine core has prevented accurate determination of the time of the *Araucaria* decline, it is dated from the oxygen isotope record by Peerdeman *et al.* (1993) to the period embracing the height of the penultimate glacial to the height of the Last Interglacial. The penultimate glacial would be the prefered time of change as this corresponds with conditions during the last glacial period when *Araucaria* declined around Lynch's Crater and also around Strenekoff's Crater (Kershaw 1994) on the Atherton Tableland. At these times driest conditions during the glacial cycles may have placed these forests under greatest stress, facilitating replacement by open sclerophyll woodland.

The cause of this replacement may have been purely climatic though, for the Lynch's Crater record, it was argued by Kershaw (1976), before any charcoal counts had been undertaken, that fire would have been the critical factor as a drying of climate alone would most likely have led to the replacement of araucarian forest by other fire sensitive rainforest types such as deciduous vine thicket on the high nutrient status basaltic soils, rather than by sclerophyll vegetation. The subsequent production of a charcoal curve appeared to provide a convincing justification of this interpretation. However, concern was expressed by Singh *et al.* (1981a) that the substantial increase in charcoal was more a reflection of the development of a vegetated swamp surface that was capable of carrying fire than an accurate indication of the increase in regional fire activity although an increase in burning undoubtedly occurred. Furthermore, it was hypothesised by Kershaw (1976) that a decrease in *Callitris,* which is recorded prior to that of *Araucaria* in the Lynch's Crater record (an estimated 50-55 k.y), was most likely to have been the result of an earlier replacement of drier, fire sensitive 'rainforest' by sclerophyll vegetation on lower nutrient status soils surrounding the Tableland. Again, fire was invoked as the cause and this explanation is still favoured despite an absence of charcoal in this part of the core. Although a great deal of research has been undertaken on fossil charcoal, the actual relationships between vegetation, fire and charcoal representation in sedimentary records have proved to be very complex (see for example Head 1989)

Against this background, the interpretation of the *Araucaria* decline at site 820 as a result of burning is based primarily on ecological and biogeographical grounds, rather than on the charcoal curve. However, some response to questions raised about the charcoal curve is warranted. There are surprisingly high values for charcoal throughout the core suggesting either that fire has been a regular event or that the charcoal has been augmented by 'charcoal-like' particles which may have been chemically precipitated in this marine environment. Support from ARC has been requested to examine more closely the nature of this material. Concerning the proposed increase in burning in the top part of the record, Peter White questions the significance of the high 'charcoal' value associated with the decline in *Araucaria* and the statement that high charcoal levels are then sustained. I argue that it is more than coincidence that the highest 'charcoal' value coincides with the *Araucaria* decline and that, on average, 'charcoal' values are higher since this event than before with two of the three highest values for the whole sequence being recorded during this limited period.

The final question is can these vegetation changes, that are almost certainly the result of increased burning, be attributed to the activities of people? There has been little arguement (with the notable exception of Horton 1982) against the interpretation of the Lynch's Crater event and the same reasoning has been applied to the interpretation of Site 820. The Lake George record has been used to support the possibility of an early date for the arrival of people and their impact on the continent. White challenges this support on the grounds that there is debate over the age of the charcoal increase at Lake George and criticises us for not discussing the possiblity that this increase occurred only at 60 k.y. as suggested by Wright (1986) rather than at the original date of c.125 k.y. proposed by Singh *et al.* (1981a) and Singh and Geissler (1985). The reason for this ommission is that the issue has been addressed previously (Kershaw *et al.* 1991) and, although the proposed date of 60 k.y. is currently very fortuitous in light of the existing Australian archaeological record, it was considered incompatible with what was regarded as the more reliable pollen and sedimentological evidence. It was felt that there was no reason to assume a constant rate of accumulation in sediments which show substantial variation (Singh *et al.* 1981b) and that the extension of the linear timescale to the base of the core would not allow correspondence with the established date of the Brunhes/Matuyama boundary. Furthermore, the change clearly predates the last interstadial marked by relatively high *Casuarina* percentages that has been related to oxygen isotope stage 3 throughout eastern Australia (Kershaw and Nanson 1993). Consequently it has to be older than about 80 k.y. and, until further dating has been carried out, Singh's estimate of the age appears to be the most realistic.

White employs the conclusions of Kirch and Ellison (1994) to counter the claim for a human involvement in the change within the marine core. They consider that it is necessary to have a major and sustained increase in charcoal from a previously low level in order to invoke a human presence and impact. This may be the case for remote oceanic islands where natural ignitions are likely to be rare and have limited affect on the composition of vegetation and where settlement is confined to the relatively stable climatic conditions of the Holocene. However, the scenario hardly applies to Australia where fire is known to have been an important variable since at least the Early Miocene (Kershaw *et al.* 1994) and where the relationships between climate, fire and people have inevitably varied substantially over the period of human occupation. At Lynch's Crater, little charcoal is recorded between about 7 and 5 k.y., when the site was surrounded by fire-resistant rainforest existing under very high rainfall, but there is little doubt that people were firing the surrounding, more open forests. As previously mentioned, it is the sustained nature of vegetation change in the landscape as a whole, including the destruction of fire sensitive vegetation and plant extinctions, upon which the burning hypothesis is largely based.

With respect to White's concern over the different timing of araucarian forest destruction indicated in the marine core and at Lynch's Crater, I believe that these differences, and the inferred time of change at Lake George, can be accomodated by a likely pattern of human colonisation and impact. The height of the penultimate glacial period would have been an appropriate time for human entry into the continent as sea level was low and seaways between Australia and islands to the north at their narrowest. As suggested in the 'coastal colonisation hypothesis' of Bowdler (1977), the first people probably had a coastal economy, so that impact would first be on the northern continental shelves. Northeastern Australia would also have been less arid and inhospitable than much of the continent at this time. It has been demonstrated that the continental shelf was relatively dry during glacial periods (John Grindrod personal communication) and probably supported extensive araucarian forests that would have been vulnerable to an increase in burning. The much later replacement of araucarian forests on the Atherton Tableland could be explained by their relatively protected position. Fires would have had difficulty in penetrating wetter rainforest communities that presumably survived during the glacial period on the intervening ranges. During the subsequent interglacial, the change to sclerophyll vegetation would have slowed or even reversed due to increased precipitation and it may not have been until the last glacial period that the transition process continued. Although the pattern of rainforest replacement may relate to the process of human colonization, there is no reason to believe that impact was immediate, and there may have a long period between the two events in any one region. Around Lynch's Crater, the process of vegetation replacement appears to have taken some 12 k.y., very different to the almost immediate response of vegetation to major climate changes, and the survival of patches of araucarian and other fire sensitive forests to the present day suggests that the process may still be continuing. It has been argued previously (Singh *et al.* 1981a) that the date of 125 k.y. for the impact of fire around Lake George is related to the onset of interglacial conditions rather than the first arrival of people as, in a drier environment than northeastern Australia, the vegetation cover during drier glacials would have been insufficient to carry fire.

A gradual replacement of fire-sensitve by fire-promoting vegetation may also have occurred in response to increased burning associated with a trend towards drier or more variable conditons since the Early Miocene (Kershaw *et al.* 1994). Within the latter part of the Quaternary, there are certainly indications of an acceleration in this trend (eg. Bowler 1982) that may have allowed some impact on the more humid coastal margins of the continent. However, both the Site 820 and Lake George records suggest that patterns of vegetation in relation to glacial climatic oscillations have been relatively stable through many hundreds of thousands to millions of years. White questions why Kershaw *et al.* do not postulate human impact for a peak in charcoal, equivalent to that around the decline of *Araucaria,* dated to at least 1 m.y. ago in the marine core. This peak also corresponds with a peak in mangrove pollen and follows a low value for *Araucaria* and a peak in Poaceae. The pattern certainly shows some similarities with the *Araucaria* decline, and suggests that conditions may have been climatically suitable for a replacement by sclerophyll vegetation. However, the pattern suggests that fires may have been a response to the vegetation changes rather than an initiatior of them and this is very different to the later changes for which a human cause has been invoked. The fact that the change was not sustained and is not accompanied by plant extinctions also suggests a different cause. A similar increase in charcoal and vegetation to that evident in zone F at Lake George is noted by Kershaw *et al.* (1991) in zone O within that record. They did suggest that this event may reduce the Aboriginal burning argument but again it was of a 'temporary' nature. The significance of these events in both records could be that natural conditions have been condusive to a change in vegetation for a long period but that such a change could not be sustained until there was an additional and more constant ignition source.

In conclusion, I feel that the weight of available evidence points towards Aboriginal burning as the most likely cause of vegetation changes indicated in all longer pollen records examined, and this implies that people have been present on the Australian continent for at least 140 k.y. Regardless of the cause of the vegetation changes, these are important to an understanding of the development of the Australian landscape and are not insubstantial. Samples have been obtained from the Ocean Drilling Program to detail the upper, most critical part of the record from Site 820 and it is hoped that in the near future a more detailed reconstruction will be available.

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Comment on J. Peter White's paper "Site 820 and the evidence for early human occupation of Australia" submitted to *Quaternary Australasia*.

Atholl Anderson

I agree with White's comments on the Kershaw *et al.* (1993) argument for anthropogenic vegetation changes beginning 100-150 k.y.a. However, I think that there is a more fundamental problem for the hypothesis than the substantive issues which White addresses. This is the matter of sampling.

The period in question extends over approximately 140,000 years (the top 37.5 m of the core), but our information about it comes from only five 5-cm^3 samples (and on pollen and spore counts ranging from 98 to 243 per sample which are fairly low (Birks and Birks 1980: 165) as well), taken about 7.5 m apart on the core. This represents a mean sampling interval of about 28,000 years. To put it another way, if each sample was a 1 cm thick slice of the core then on average it describes about 37 years of accumulation and the total sample of 187 years would be only 0.13 % of the record. These figures can hardly be precise, but they illustrate the main point which is that the sample size is so minute that drawing any conclusions about the representativeness of the data or perceiving patterns in their distribution can hardly be more than an exercise in conjecture.

Nevertheless, there are questions about the internal consistency of the argument which can be taken up independently of the sampling problem, and it is these which White discusses. I would add several points. Firstly, while Kershaw *et al.* (1993) plausibly relate the extinction of *Dacrydium guillauminii* to an increase in burning frequency, the proposition is not relevant to their hypothesis because that taxon seems to have persisted until somewhere between samples 2 and 3 (Kershaw *et al.* Figure 3), so its demise could have occurred during the known span of human occupation.

Secondly, not only do the charcoal abundance data fail to demonstrate that there were *sustained* high levels of burning after sample 5 - with only one sample prior to the last providing a value higher than the general background level of 1000-2000 particles (% identifiable dryland pollen) - but the period represented by samples 1- 7 overall exhibits the greatest *variability* of charcoal abundance in the entire core.

Anthropogenesis

White suggests that research on Mangaia, in the Cook Islands might provide useful criteria for perceiving anthropogenic influences in Australian cases. However, while Kirch and Ellison (1994) note some obvious principles, the particularities and conclusions of their case are wide open to dispute (Anderson 1994, n.d.a.), and both it and the Kershaw *et al.* (1993) argument beg broader questions about the palynological approach to culture history. The rationale of this is that the production of archaeological remains was so slow (for demographic reasons, themselves eminently questionable), for so long after initial human colonisation and the rate of their destruction by natural agencies so rapid, that alternative means of documenting early settlement are required, and palynological and related evidence can form a proxy record of cultural activity. Amongst the many objections to such an argument, I note several here.

Firstly, the initial assumption is dubious. Prehistoric hunters were not tidy campers. They scattered refuse liberally over the landscape, much of it in distinctive forms or configurations of highly durable materials. It is true that the means to date sites much more than 60,000 years old were not readily available until the advent of luminescence dating and especially its recent offshoot of optically-stimulated luminescence dating. However, if people were in Australia during the preceding 100,000 years they must have created *millions* of individual sites during that period, a significant proportion of which are likely in some degree to have survived in demonstrably early sedimentological contexts or which contain artefactual, animal or human remains that are suspiciously different from later counterparts. It is highly unlikely that all such sites have been missed, or their evidence misunderstood, during several centuries of casual collecting and more recent systematic investigation by archaeologists and natural scientists.

Secondly, palynology and related interests, at best, provide only indirect evidence of a former human presence, except in very rare cases where remains of cultigens or similar plants are involved. Archaeology, however, works with evidence which is usually direct and unequivocal. Worldwide, archaeological evidence is consistently older than or contemporaneous with palynological indications of human colonisation - at least partly because the main event discerned in this way is substantial deforestation, which is predominantly a Holocene phenomenon (Walker and Singh 1993: 107). Where it is asserted that palynological data document pre-archaeological evidence of occupation the argument loses plausibility in proportion to its scale; extensive proxy suggestions of human activity, without any direct evidence, indicate rather the operation of natural agencies.

Thirdly, while some general rules for defining anthropogenic effects have been enunciated usefully by Walker and Singh (1993: 105), the difficulties of putting them into effect should not be underestimated. Perhaps the most important of them is that inferred processes should appear to have operated at levels and rates that are unprecedented under natural conditions. However, in the case of charcoal abundance, which is the usual criterion at issue, the ideal case of a long record of little or no charcoal, followed by high and sustained levels which would constitute a strong proposition of anthropogenesis, is hardly ever attained, including in both the Mangaian and site 820 cases. Consequently, the argument depends on personal judgement as to where a threshold of potential cultural impact might lie, and this varies considerably. It is most difficult to establish where charcoal abundance is thought to increase during periods when relative aridification would tend to produce much the same signature in the palaeoenvironmental record, as during the late Pleistocene in Australia.

Consequently, I agree with White's implicit view that, in the absence of any other evidence in its favour, anthropogenesis is an explanation for palaeoenvironmental indications which should first defer to comprehensive exploration of the probability that phenomena fall within the amplitude of natural variability.

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