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SOURCES OF MOAS AND MOA HUNTERS

Garry Law

Sir Julius von Haast's conclusion that the hunters of moa were both paleolithic and autochthonous (of the earth) was first widely disseminated in the paper, the centenary of which is celebrated by this issue. To his New Zealand readers then this was as extraordinary a proposal as it is to us today. How did he arrive at such a conclusion? I have attempted to answer this question in this article, for I have found to my surprise that Haast's beliefs were for the time he proposed them, and the information he had at his command, not as poorly founded as some have assumed. Dr Duff, who could scarcely be accused of depreciating Haast, was led to conclude that Haast had been "perhaps deliberately rash and provocative" (1956:252) and "wild and irresponsible" (1968:169). His son's massive biography (H.F. von Haast 1948) leaves me with the belief that such behaviour was foreign to the man's character. To explain it we shall have to examine the state of knowledge in 1869-71, and more particularly Haast's knowledge of, and beliefs on: moa, the association of moa with man in New Zealand, the antiquity of man in New Zealand, supposed sea level changes and former trans oceanic links to New Zealand, the antiquity and archaeology of man in the rest of the world, and concepts of the length of geological time.

ON MOA

Haast first came to New Zealand in late 1858 on the NOVARA with Hochstetter. One of the avowed purposes of the latter's exploration was to secure skeletal specimens of moa. This proved remarkably difficult and it was only from Haast's excavation of a cave floor in Nelson that any useful specimens were obtained (Hochstetter 1867:184). Haast's first experience then, of the relative abundance of the remains of moa would incline him to the view that their remains, certainly their surface remains, were rare. Although the first Europeans on the Canterbury plains and elsewhere, were reputed to have found bones in abundance, by the time Haast first arrived in Canterbury (1860) much of the plains had been taken for agriculture and surface finds were of the past. It was not until Haast's investigations of the Glenmark deposits from 1866 on, that he obtained any quantity of bone. Glenmark is interesting for not only were skeletons recovered from below the surface of the still extant swamp, but also from the base up in a thick alluvial deposit which had been exposed by down-cutting streams. Haast describes the oldest contexts he found as "post-Pliocene" (Haast 1868, 1871b:69). Haast's overall experience to that time was that natural moa

remains were essentially geological rather than those of a current fauna. There was little in the literature to 1870 to lead him to any other opinion, and indeed some statements to foster his view. On Haast's side, at least to Haast, was the extinct giant fauna of Europe and North America, by that stage well attested to the Quaternary, and extinct well before written record.

MAN AND MOA TO 1870

Six sites had been mentioned in the literature up to 1870, all of which we now know contained clear evidence of the association of man with moa. Cormack's Midden (Opito, no site number as not relocated, Green 1963:57 - 58) was noted in one of Owen's early articles on moa (Owen 1856) yet strangely Haast never mentions this site. He makes it clear he knew of the Kaikoura burial with the moa egg (Haast 1871b:93) (S49/46) but reading between the lines it is fairly clear that at that stage he had not heard of an adze which was later alleged to have been found with the burial. We might presume Hochstetter's knowledge of this burial came from Haast (Hochstetter 1867:183). Haast quotes G.A. Mantell's (1851) "Petrifications and Their Teachings" (Haast 1871b:78) in which was included a description of Waingongoro (now called Ohawe, N129/77). Haast was aware of the claimed association of evidence for killing of moa with evidence of cannibalism and what he admits were Maori artefacts, (Haast 1871b:78) but rejects the latter as having been mixed in, in the rather trying circumstances in which Mantell carried out his investigations.

Although Waingongoro seems to have been well known through its investigators (Taylor, Mantell and with Taylor, Sir George Grey) as was Awamoia (S136/4) (Mantell), Haast had little in the way of solid documentary evidence he could use to compare the cultural material found in these sites, with that from those he was familiar with. By his own admission (1871b:91) he had overlooked the summary of Mantell's address to the New Zealand Institute in Wellington which summarised his views on moa (Anon. 1868). Haast's 1871 paper, as published in the Transactions of the New Zealand Institute, is a collection of three parts, a modification of the original address given to the Canterbury Philosophical Institute on March the 1st 1871 (Haast 1871a), and the further comments he made at two other meetings later in that year. The two latter contributions reveal that Haast's knowledge of the other sites had increased after the first paper, but it is fairly clear he was sticking to his first conclusions, and in particular that the Moa-hunters had no polished tools. By this third paper he had heard of Murison's site at Maniototo (no site number as not relocated). Haast's argument that the polished tools at this site may have been unconnected losses by later Maoris (1871b:104) is particularly thin.

It is unfortunate in this regard that Haast had not heard much of

this site before. However Hector who had heard of it earlier, appears not to have heard of polished tools, at least initially.

In Hector's 1866-67 report for the Colonial Museum is an accession list with: "Mr W. Murison, Collection of ancient chert knives or flakes found with the bones of the moa and other extinct birds in native cooking ovens. Tree Gully, west side of Maniototo Plains". (Hector 1867:8). Most other accessions of artefacts were described as Maori. This all too brief note suggests that Hector too, was at first struck by the European parallels of extinct fauna and flake knives. As a geologist he must have been familiar with Lyell's "The Geological Evidences of the Antiquity of Man" (Lyell 1863). Hector and Haast were still on good terms in the 1860s so Hector must be regarded as a possible catalyst for Haast's views. Haast knew of a site on the Manuherikia plain (1870:119) which lies parallel to the Maniototo plain. Had he confused his geography ?

The only other site to my knowledge which was known before 1870 was Shag River (S155/5) (Melvin 1868:8) and although Haast visited the site subsequently there is no evidence he knew of it earlier.

Based mainly on the investigations at Waingongoro and Awamoa there were many published statements up to 1870 affirming the contemporaneity of moa and man, G.A. Mantell until his death being the most prolific. (E.g. G.A. Mantell 1848, 1850, 1851, Mantell in Owen 1848:11).

His enthusiasm for his son's often somewhat amateurish research seemed to know few bounds. A newspaper report of one of his lectures in Britain on moa describes it as "animated". From these many statements very few archaeological facts pertinent to the culture of those who had killed and eaten moa can be extracted. To those early Victorians the possibility of archaeology in New Zealand had not occurred, although they certainly were conversant with geology, paleontology, botany and zoology. To these observers then, the assumption came easily that Maoris, much as they were in the 19th century, were responsible for killing moa. Some accounts though did grant some reasonable antiquity.

By 1870 there are some signs that this attitude was changing. Mantell in 1868 could speak of archaeology (Anon. 1868:5) and state that his findings "appeared to indicate a period when many of the implements in common use among the Maoris, and supposed to have been brought with them from Hawaiki, were unknown to these early aborigines" (ibid.:6). The Reverend Richard Taylor in his second edition of "Te Ika a Maui" was aware of "Danish middens and Abbeville gravel pits"

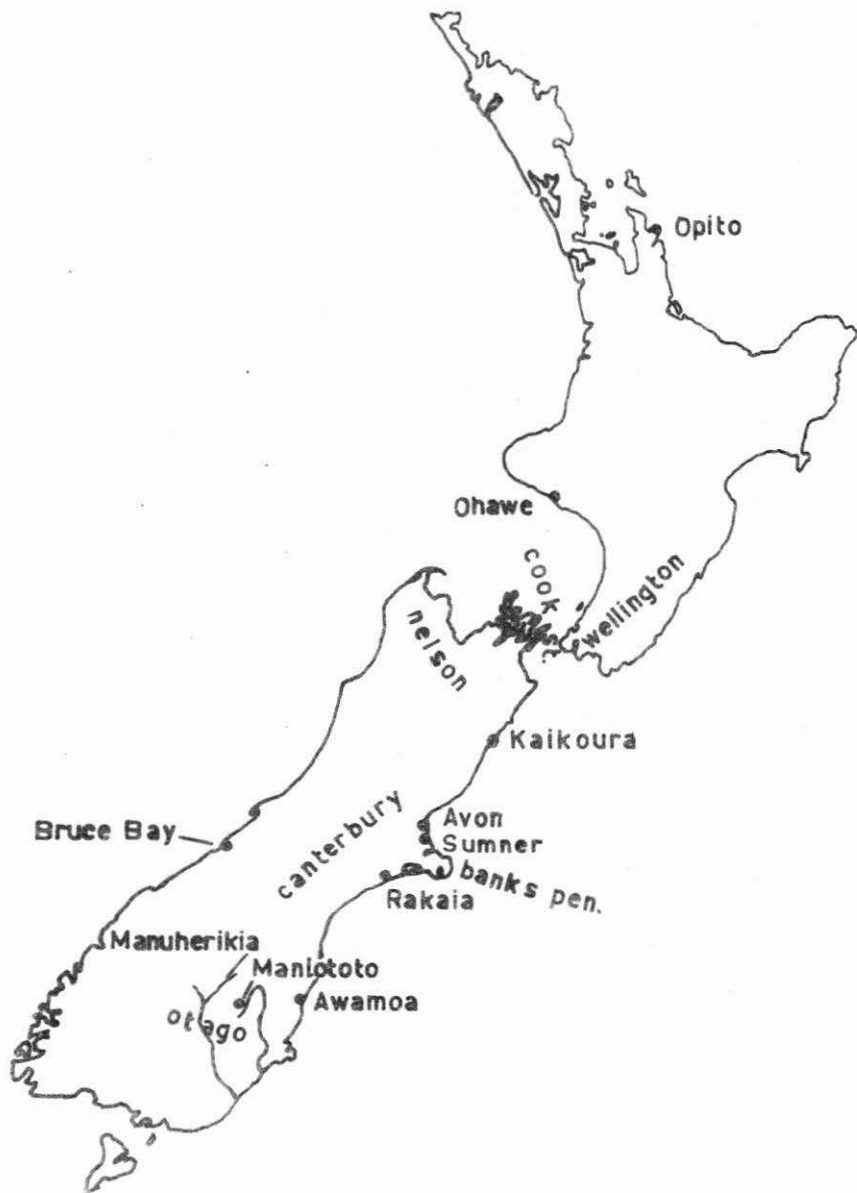
(Taylor 1870:416) and equated Maori culture with the former in level but not in age (ibid.:417). Indeed he states of moa bones, "the recent state of many of them clearly proves that they have lived within the last half century" (ibid.:426). But of the relevance of archaeology he leaves no doubt. "These Maori middens therefore, have their interest, as well as the far older ones of Europe, they are as worthy of our consideration, and their investigation is calculated to throw light on the past state of the Maori" (ibid.:419).

Haast was not the first to propose autochthones for New Zealand, for Colenso had done so in a paper written for the New Zealand Exhibition in 1865 (Colenso 1868). Strangely for Colenso, he offers traditional evidence alone for his proposal (ibid.:394). He excludes the Maoris, regarding them as a recent arrival. At the time Colenso wrote this paper the debate on whether the Maori knew the moa must have started for he writes of moa "never having been seen alive by the present race of New Zealanders. For if it had been seen by them, and by them had been gradually killed and extirpated, as some Europeans have laboured to show, then no surer evidence could be desired as to the great antiquity of the present race in New Zealand." (ibid.:403, emphasis Colenso's). Their labours must have been largely verbal and not in Haast's presence, for in his paper he shows no cognisance of such prior discussion. Colenso was clearly only a step away from attributing the extinction of moa to the autochthones. Here was another catalyst for Haast, but solely with Haast must lie the credit for the coordination that went into "Moas and Moa Hunters".

I have not attempted to cover the investigations of the Maoris' traditional knowledge of the moa. Indeed many of these post-date Haast's paper and were inspired by it. Haast accepted the Maori word moa (see Hansen 1970:6-12) for the bird, but for their knowledge of it invokes a fantastic oceanic legend of giant birds. The coincidence that Maoris should call bird bones, and bird bones far beyond their previous experience, by a bird's name does not seem to have struck Haast. We may safely say that Haast was strongly influenced by the views of Colenso and Stack in this matter.

THE ANTIQUITY OF MAN IN NEW ZEALAND

Haast's evidence was not gathered solely from Rakaia Mouth (S93/20). He mentions in passing that he had noted other sites of a similar nature at Sumner (now called Redcliffs Flat, S84/7) and by the Avon (no number as not accurately relocated) (Haast 1871b:89). More important was the material he had observed at Bruce Bay in Westland (ibid.:79 Haast 1870a:110). Here he had inspected the find site of two stones which he took to be a polished artefact and a polisher. Haast had arrived some days after their discovery in 1868. The circumstances



of the discovery were such that if they were artefacts, there could be no doubt of their considerable age. They were deeply buried in an auriferous beach deposit well behind the present beach, but formed when the sea was essentially at its present level. Haast's account of this find, read by the secretary to the Ethnological Society in London in January 1870 is an article of considerable historical interest to New Zealand archaeologists.

In it Haast defines the end of prehistory for New Zealand, the term prehistory only recently having come into common use in Europe (Daniel 1967:24). He uses an argument based on rates of population growth and a population estimate for 18th century New Zealand, and proposes the term Moa-hunters (Haast 1870a:112). Haast contrasts the polished tool at Bruce Bay with the unpolished tools of the Moa-hunters, and offers them as neolithic and paleolithic respectively (ibid.:113) but he does not offer the term autochthones. He does not mention the Rakaia site, but does mention the site noted before on the Manuherikia valley, and "moa-ovens in this and Otago Province" (ibid.:120), this meaning Canterbury. He mentions again artefacts found around Wellington in swamps several feet below the surface, and under roots of trees of enormous size, artefacts similar to those from Bruce Bay. Interestingly this same evidence was offered by Haast in 1862 in his founding address to the Canterbury Philosophical Institute (H.F. von Haast 1948:228).

Surprisingly it seems, Haast arrived at his Moa-hunters before he studied the Rakaia site. He failed to mention the site in his paper on the Bruce Bay find, which he must have written at latest in the first half of 1869, while he gave a talk on "moa-hunters" and the Rakaia site to the Canterbury Institute on July 7th 1869 (Anon. 1869:420) and it is clear he had been to the area of the site some years previously (Haast 1864:21). In the letter he wrote to Owen on October 26 1869, he states he first investigated Rakaia after he had sent another paper to Lyell on prehistoric New Zealand (Haast 1870b:53). This paper can only be that on the Bruce Bay find. The Redcliffs Flat site must now be regarded as the initial stimulus to Haast's studies.

SEA LEVEL CHANGES AND FORMER SEA LINKS

Haast as a modern uniformitarian geologist accepted the evidence for continuing change in the level of the land. It is fairly clear that he regarded this essentially as a movement of the earth, and was dubious of the then theory of the sea level altering as water was stored in glaciers. Again his view of the magnitude of the changes relative to the sea was not as radical as Lyell's, as shown in his correspondence with Lyell (H.F. von Haast 1948:516). Thus he could regard evidence for former sea levels independently for various parts of New Zealand.

By 1869 he believed that sea level relative to Banks Peninsula had remained essentially constant for the whole of post Pliocene time, and thus the Rakaia site could relate to a coast line of considerable age, limited only in that the Canterbury plains were outwash from the glacial period.

The Bruce Bay finds, related as they were to sea level, were also restricted in age by this. Here Haast accepted the well known raised beaches of the West Coast, and indeed attributed the glacial period to the elevation of the Southern Alps. But he did not regard their elevation as a current process. The deposits in Bruce Bay were limited only by the retreat of the piedmont glacier which had formerly covered the shore at this point.

On the grounds that the species of moa were nearly identical in the North and South Islands, Haast (1871b:84) proposed that Cook Strait was relatively recent, with the additional support that his Moa-hunters - too primitive to have canoes - had obtained obsidian from the North Island. If Haast rejected canoes, and presumably rejected independent creation of man in New Zealand, which as a follower of Darwin he must have done, he could only have brought man to New Zealand on foot. There is a strong hint in "Moas and Moa Hunters" that Haast would introduce moa into New Zealand at the same time as man. In 1869 bones of a struthious bird found in New South Wales were pronounced Dinornis australis. Haast quoted this in the first version of the paper, (Haast 1871a:7). This was removed from the second version of the paper, no doubt as a result of Owen reassigning it to Dromornis, and thus to a species much more distant from New Zealand moa.

To the Victorians the study of the distribution of species involved, as the anti-religious epigram puts it, believing a new impossible thought every week. The hypothesis of former land links was well established by the late 1860s and to explain the presence of some of the flightless fauna of New Zealand has continued to the present. Only with the dramatic increase in the evidence for continental drift have some of the links become more credible. The hypothesis involved does not appear to have worried Haast greatly.

THE ANTIQUITY OF MAN

Haast in the 1860s contributed to many European journals including the Quarterly Journal of the Geological Society (first issued 1845), the Journal of the Ethnological Society (1852), and Natural History Review (1861). These, with the Annual Magazine of Natural History (1828) gave massive space, through the 1860s, to archaeology with its vastly expanded horizons in the wake of the discoveries of stone tools in undoubtedly old

contexts, and Darwin's "Origin of Species" (1858) in its obvious applicability to man. Haast could not have failed to have seen much of this and to adopt much of its attitudes. Lyell (1863) provides an encapsulation of much of this fervour. By the time Haast wrote the antiquity of man could not be doubted, the principal evidence coming from unpolished stone tools associated with extinct fauna and/or in stratigraphically sealed deposits of considerable age. A progression of culture could be demonstrated from these through the Danish kitchen middens, with polished stone tools but no evidence of domestication appears, and later metal tools. Lyell describes the Neanderthal skull (ibid.:64) and provides what were for the time surprisingly accurate estimates of the greatest age of polished tools (ibid.:21-23).

Haast adopted the terms paleolithic and neolithic which were proposed by Lubbock (1865). Haast may have borrowed them direct, or via the tenth edition of "Principles of Geology" (Vol. 2, Lyell 1868) in which they were first adopted (Daniel 1964:48). We know that Haast received a copy of this edition in late 1868 (H.F. von Haast 1948:545). Of equal interest is that the same edition incorporated a map and some discussion of the Wallace Line, (Lyell 1868:349-354) dividing the zoological province of Australia New Guinea, with the Celebes from that of mainland Asia and Sumatra Borneo and the Philippines. Haast could not fail but be struck by the racial division which was claimed to follow almost the same line. Within the province defined by Wallace the origin of the boatless aborigines of Tasmania had long since been called to notice (Mulvaney 1969:133) and the former non-existence of Bass Strait had been proposed to explain it.

Thus to propose a geological antiquity for man in the south-west Pacific was not entirely unilateral. We may see some evidence that Haast considered the above in his eagerness to see the M^oa-hunters' dog as not domesticated (Haast 1871b:88), a position which would bring it into concordance with the Australian dingo (Canis familiaris dingo) well known by 1870 as a singular placental species in a marsupial continent. At least two precedents for this eagerness can be found (G.A. Mantell 1848:234, Lartet in Lubbock 1862:345).

THE LENGTH OF GEOLOGICAL TIME

Although not entirely relevant it is of interest to note that since 1870 there has been a radical alteration in concepts of the length of the Pleistocene. In 1863 Lyell disposed of the glacial age in not less than 220,000 years (Lyell 1863:226) and although we might suspect Haast would not accept the argument on which this estimate was based, we have a not dissimilar estimate of his made in 1868. He argued that since the Banks Peninsula volcanic system was Tertiary in age it had been extinct without doubt for thousands if not hundreds of thousands of years (H.F. von Haast

1948:546). Haast was giving here a lower limit, but when he was prepared to speak in thousands of years he would hardly propose an upper limit of millions such as would be acceptable today. Haast seems to have regarded his Moa-hunters and moa as belonging to the Quaternary in New Zealand. This gives us an interesting order of magnitude estimate of Haast's concept of their age, an estimate he refused to give subsequent to his 1871 paper.

Our knowledge of rates of geological movement, and rates of evolution of species has increased rapidly since 1870, and we must be careful to exclude this knowledge when judging Haast's autochthones.

CONCLUSION

Haast in 1869 and 70 had to arrive at a position of "minimum strain" in relation to the data he had, and what we today would term the conceptual framework of archaeology he had derived from Europe. That he chose to: reject the evidence of polished tools from Rakaia; reject the Maori traditional and linguistic evidence, the former admittedly tenuous; accept that the Moa-hunters cooked food the same way as the Maoris; propose radical alterations in the form of the land with little other evidence; and speculate about the Moa-hunters' racial form in an almost complete evidential vacuum, demonstrates the lengths to which he was prepared to go. Above I have presented some material mostly favourable to Haast, material which it is fairly clear Haast considered, and material relevant to Haast's scientific concepts which are not always entirely clear in "Moas and Moa Hunters".

I find it very difficult to say his position was ill considered, or for its day unsound. We can be sure he was better informed than most of his critics. Unfortunately he was wildly wrong. Much of the blame for this we must place with him in his wholesale and uncritical adoption of an analogous European scheme. Even when we admit that Mantell and his ilk were nearer to being right (i.e. "right" 1972 version) Haast towers above them as a systematic recorder and a theoretical thinker. He and he alone can claim to be the father of New Zealand archaeology. If an apologist he needs I can only offer that in 1869 and 70 he also fathered the Canterbury Museum. No mean creations for two years — New Zealand archaeology, and the Canterbury Museum.

APPENDIX

The sequence of events below is somewhat ironical. In 1858 W. Mantell sent to the Museum d'Histoire Naturelle in France some material from Waingongoro and "Ruamoā" (Awamoā). Edouard Lartet, who was a pioneer of French stone age archaeology (Daniel 1967:78) wrote to Lubbock

in Britain describing this material, which included bones of moa, dog, seal and artefacts including oven stones, a large flint flake tool, obsidian flakes and a fragment of rock with a polished face, the latter from "Ruamoa". John Lubbock, who must claim a large place in the founding of prehistory as a discipline (Daniel 1964:48-65) published the letter together with one from W. Mantell, and in a commentary accepts the association of man and moa (Lubbock 1862:343).

When Lubbock published his "Pre-Historic Times" in 1865, as we noted, he proposed the terms paleolithic and neolithic. If Lubbock had gone back to the letter he had published he would no doubt have included the hunters of the moa in his neolithic. Haast adopted Lubbock's terminology but came to the opposite conclusion, and to complete the cycle Mantell was one of his strongest critics.

(Hochstetter (1867:65) also uses Ruamoa, but the site locality he gives shows the site to be Awamoa. Mantell appears to have changed the name twice rather than the once previously assumed).

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