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DEFINING THE PERIOD OF MOA EXTINCTION

Atholl Anderson
Australian National University
Canberra
Australia

Few aspects of New Zealand's prehistory have engaged scientific and public attention so consistently as two interlinked questions of moa extinction; when did moas become extinct and why? Answers offered over the last 160 years have run the gamut from geological antiquity by evolutionary senescence, to within the 19th century by Maori and European disturbance (Anderson 1989a). During the last thirty years the consensus has settled on extinction of all taxa within the period of pre-European habitation, caused by direct and indirect cultural agencies. The latest perspectives on this subject are offered by Holdaway and Jacomb (2000), and Diamond (2000) in a review essay. In bringing a more sophisticated approach to the modelling of extinction than those which have been attempted earlier (e.g. Anderson 1989b), they make a welcome contribution to the subject, but to what extent does it differ from previous hypotheses, and how convincing are its conclusions? I discuss briefly here two issues about moa extinction: the difficulty of extinction modelling and the archaeological evidence of extinction age. Since both depend substantially on arguments about radiocarbon chronology it is important, first, to clarify the basis of comparison.

The span of moa-hunting

Holdaway and Jacomb (2000: 2250) describe the orthodox model of moa-hunting as having "lasted some 600 years, peaking 650-700 years before present and ending about 400 yr BP". This, of course, is the original 1980s version of the model (Anderson 1989a, 1989b), based on the full, untested, dataset of uncalibrated radiocarbon determinations from moa-hunting sites. Subsequent chronological research, beginning in 1991 (Anderson 1991) reduced the

orthodox moahunting chronology to approximately 800-400 BP and some recent data, including those from Wairau Bar (Higham *et al.*, 1999) suggest a range of about 350 radiocarbon years (750-400 BP).

The Holdaway and Jacomb model, in contrast, is constructed from calibrated radiocarbon ages. A proper basis of comparison with the orthodox model requires, therefore, that its chronology be calibrated as well. Holdaway and Jacomb (2000: Figure 4) attempted this by collating some calibrated ages on moa-hunting sites from Anderson (1991) to create an "orthodox" model of 220 years of moa-hunting, extending up to about 400 years. A better approach in the present context is to take the actual estimated span (400-750 BP) of the current orthodox model and calibrate its marginal ages. Since 400 BP calibrates to about AD 1450, and 750 BP to about AD 1280 in the Stuiver *et al.* (1998) calibration curve (leaving aside for the sake of exposition the necessary probability distributions), the orthodox span of moa-hunting is reduced to 170 years. This can be compared to the new model in which it lasts up to 160 calibrated years (simulation A in Holdaway and Jacomb 2000: 2251), although its authors prefer a time to extinction of only 50 years. Given the error margins of radiocarbon determinations upon which all the chronological propositions of extinction are erected, differences of this degree are so difficult to discriminate that they are hardly worth debating. Nevertheless, taking the figures at face value is still worth while for considering the issues they raise.

Modelling moa extinction

The advantage of simulation is that it enables some parameters of the extinction process to be examined more precisely than otherwise by exploring the effect of changing variables in a simple model. In this case, Holdaway and Jacomb (2000) assume a certain moa population size (158,000 birds throughout New Zealand), with given rates of survivorship and fecundity, and an initial human population of 100 people which is increasing at low to medium rates (1% and 2.2% natural increase), and which is cropping moas at rates derived from Anderson (1989b) – although it is not clear which of those are actually used. Regional variation in moa population density and also levels of habitat destruction are taken into account in additional simulations.

Structured in this way, the model can examine the business end of the engagement between people and moas – the consumption, in this case over-consumption, of a prey item (considering moas as a single target). However, what it cannot examine might be just as important, the prior and contingent selection of that subsistence item. Prey selection is a much-discussed zoological and zooarchaeological issue in which the economics of consumer choice

mediate between predator preference and selection behaviour. For example, as a preferred prey item becomes progressively scarce and the effort to find it correspondingly greater, there is an economic incentive to switch towards prey items that might be gathered more efficiently (perhaps seals and fish in New Zealand). Lowered consumption of preferred taxa may then allow some recovery of population density leading to further exploitation in a series of predator-prey cycles. In other words, even where taxa go extinct through over-exploitation, it is unlikely that depopulation will follow a simple, steep, downward curve into oblivion unless in exceptionally constrained circumstances of a kind which did not exist in New Zealand.

Additional complications are added when the predator is human. Whereas many predators will be spread fairly evenly through the territory of their prey, and enabled thereby to maintain a broadly even and constant cropping pressure, people seldom distribute themselves in that way. The flexibility inherent in an unusually broad human omnivory, coupled with uniquely-diverse requirements of social behaviour, remove human subsistence arrangements beyond the simple construction of a predator-prey relationship, however useful that can be in explaining limited instances (e.g. Anderson 1981). In the New Zealand case, it is apparent that early settlement sites in the main areas of moa-hunting were located primarily along the coast, rather than inland. Their contents show that they were places at which various economic, technological and social activities were carried out. Coastal settlement probably reflected, *inter alia*, the importance of canoe transport as an essential factor in exchange and social relationships between communities.

Consequently, potential human contact with moas was more continuous in coastal districts than inland, and locally-rapid extirpation probably occurred in the areas where access was easiest and habitat destruction by burning largely unimpeded. The Canterbury Plains and some of the more accessible and open inland basins would fall into that category. For that reason, the evidence from Monck's Cave (Holdaway and Jacomb 2000) is unconvincing and substantially irrelevant to the larger issue of moa extinction. The site is located beside precisely the kind of dry-country plains where moas were most vulnerable to human impact and probably disappeared earliest. In addition, the evidence of only one site cannot assure us that moa remains were not deposited contemporaneously somewhere else in the general area. Human subsistence scheduling seldom involves the mere scooping up of representative quantities of all resource types in the site catchment – that is a zoological rather than an anthropological concept.

Testing by archaeological evidence

A much better test of the Holdaway and Jacomb model than the Monck's Cave case would be to look at evidence from archaeological and natural sites in districts that were more difficult of access, since the question is not when moas were locally extirpated but rather when they disappeared altogether. Parts of the interior with rugged topography were probably only visited occasionally, perhaps seasonally in some districts (as is suggested by the ethnohistorical data, Anderson 1998), resulting in comparatively low human-moa contact frequency, even in areas where, on ecological grounds, one might have expected relatively high moa population densities. Areas of broken hill country, with scrubby vegetation and deep gorges were probably very attractive to some moa species, yet difficult to hunt. Inland Otago, for example, contained plenty of good moa habitat of this kind (Anderson 1982), yet it has relatively few moa-hunting sites compared to the coastal districts (Anderson 1989a: Figures 9.1, 10.1).

It is worth looking at one case-study from this area. Neville Ritchie (1982) excavated several sites containing moa remains in the Cromwell Gorge. Four out of eight radiocarbon determinations from three sites have calibrated ages that suggest occupation later than the mid-14th century. One site, at Italian Creek, is especially interesting. It contained large quantities of burnt moa eggshell, from at least two moa species, concentrated around two hearths.

The radiocarbon determinations (Stuiver *et al.*, (1998) calibrated, two sigma range) on these are as follows:

1. From one side of a hearth in square A2, NZ-4714, 399 ± 88 BP. This sample was on short lifespan charcoal of 84% *Hebe* sp. and 16% *Discaria toumatou*. It calibrates as AD 1332-1342 (.007), 1396-1664 (.993).
2. From the other side of the same hearth, NZ-4715, 309 ± 82 BP. This sample was on short lifespan charcoal of 50% *Hebe* sp. and *Discaria toumatou*. It calibrates as AD 1432-1686 (.883), 1731-1809 (.097), 1925-1943 (.020).
3. From another hearth in square A4, NZ-4716, 579 ± 96. This sample was on short lifespan charcoal of *Hebe* sp. 34% and *Discaria toumatou* 66%. It calibrates as AD 1246-1496 (.995), 1604-1611 (.005).

If two occupations are represented, then one of them dates with high probability to the 15th to 17th centuries AD. If a single occupation is represented, then it probably occurred in the late 14th through 15th centuries AD. It should be added here that all charcoal samples include some inbuilt age - and *Discaria toumatou* can live for several hundred years - so those results are maximum ages and

might, in fact, be significantly younger.

Even along the coast, especially where it is backed by broken hill-country, there could also have been some relatively late moa-hunting. As noted by Anderson (1989a, Anderson *et al* 1996, and various other sources), there are numerous radiocarbon ages on charcoal from moa-hunting sites which extend into the 15th century or later. In addition, there are some radiocarbon determinations of similar relatively late age directly on moa bone from coastal sites. There are acknowledged difficulties in dating this material and we need to be cautious in using such results, but Fiona Petchey's (1997) careful analysis and discard of these data shows that at least 14 of the surviving determinations from five archaeological sites extend into the 15th century, and several into the 16th century or later. Examples (calibrated ages) are: Tairua in the Coromandel (NZA-558, AD 1431-1483), Tumbledown Bay on the south side of Banks Peninsula (NZA-825, AD 1487-1945), and Ototara in North Otago (NZ-7739, AD 1434-1624). These results, directly on moa bone collagen, are free of inbuilt age and doubts about association. As they stand, they clearly contradict propositions of moa extinction occurring earlier than AD 1400, and imply that it probably did not occur earlier than AD 1450 as the orthodox model proposes. Holdaway and Jacomb (2000) may disagree with this or other evidence that suggests the relatively late survival of moas, and on quite reasonable grounds, but simply ignoring it does not strengthen their conclusions.

Conclusions

I argue that the nature and course of moa-hunting, which was one amongst various competing demands of early Maori socioeconomic activity, were probably more complex than is allowed for in the Holdaway and Jacomb model (or in the Diamond comments). Zoological models are useful devices for thinking about basic parameters and variables but they have obvious limitations as analogues of cultural behaviour and they need to be tested against relevant evidence. The Holdaway and Jacomb model has not been tested adequately against existing archaeological data which suggest that while moa populations had been severely reduced by the 14th century, some moas were still to be found a century or more later. Consequently, the strongest model remains the current orthodox hypothesis of moa survival for about 170 years of human contact, and possibly longer, with extinction occurring no earlier than the 15th century. This would mean a period of human-moa contact three to four times as long as the preferred results in the Holdaway and Jacomb model, although not much longer than in their most conservative simulation. It would also suggest that the extinction curve was more complex than their model allows.

The test of these competing propositions lies in further chronometric research, particularly on moa bone and eggshell (and perhaps on desiccated soft tissues and feathers on some museum specimens). A programme using material from both archaeological and natural sites might help us to clarify both the most probable age of extinction, and broad regional variations in it, and also whether there were predictable constraints upon levels of direct human predation in the later stages of moa decline. This is a question of general theoretical interest and it is perhaps only in the case of moas that it could be explored empirically with any real hope of a useful result.

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