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WERE MOAS REALLY HUNTED TO EXTINCTION IN LESS THAN 100 YEARS?

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Three months ago New Zealand archaeologists were surprised to read in their daily newspapers that moas had been eaten to extinction by Maori moahunters in less than 100 years.

The claim had been made in the US journal *Science* by Richard Holdaway, formerly with Canterbury University, and Chris Jacomb of Canterbury Museum (Holdaway and Jacomb 2000). The story was picked up by *The New York Times* which headlined it as a "blitzkrieg extinction", and quoted a mammalogist at the American Museum of Natural History, New York, saying Holdaway and Jacomb had "made a really very impressive case" for "the best instance of blitzkrieg, of overkill, on the record." An ecologist at the University of California enthused in *The New York Times* that "Yes, this was a blitzkrieg... Yes, a few people could and did kill every moa". Our news media picked the story up from there. A rather roundabout way for New Zealanders to learn about ideas on their own prehistory originating down in Christchurch.

It seems to me there are a number of weaknesses in the original paper, which should have been thrashed out locally before going for prestigious exposure overseas.

The rapid extinction claim is based first of all on a "Leslie matrix model" of moa population dynamics, and secondly on some recent carbon dates of a single archaeological site, Monck's Cave, near Christchurch.

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I know nothing about population models, but it seems reasonable to presume that the output of a model will be only as good as the assumptions fed into it. Fundamental to their conclusions is an assumption by Holdaway and Jacomb that the total population of moas at the time of Polynesian arrival was only 158,000 birds, based on "current carrying capacities for emus" in Australia.

It seems to me the 158,000 figure is far too low, even though it is twice the number used by Athol Anderson (1989) in Prodigious Birds for "the sake of argument - and archaeological speculation". Anderson, too, based his calculation, less sophisticated than that of Holdaway and Jacomb, on emu biomass densities in Australia.

Why emus have been chosen for these calculations I don't understand, because the only kind of figure they will lead to is one which predicts how many emus might flourish in New Zealand if they were to be liberated here. Back in 1989 a wildlife ecologist who has worked on both emus and moas looked at all the living ratites (large flightless birds including the emu) and concluded that none of them could provide a model of moa ecology. The emu he dismissed because it was a grassland grazer. He suggested several better possibilities, including fallow deer and red deer (Caughley 1989).

Ecologists I have talked to are reluctant to guess how many moas there were, saying that there just isn't enough data, but in general they feel 158,000 is too low.

Are Four Stomachs Better Than One Stomach?

So it looks like this is a speculative field into which archaeologists rush while ecologists fear to tread. At the risk of making a fool of myself I would like to suggest that a look at how many deer and goats there are in the New Zealand bush might be more helpful than looking at how many emus there are in Australia. Emus over there are operating in a very different environment to that in our bush, one with lots more predators (e.g.dingos) and with a lot more competition from other grazers (e.g. kangaroos). Moas, on the other hand, had the browsing/grazing franchise in NZ pretty much to themselves, apart from invertebrates and a few smaller birds such as the takahe and the now-extinct goose, and they had only one predator of note, the giant extinct eagle *Harpagornis* (Holdaway 1989). Moreover, having eleven different species with different stature and habits meant that moas could exploit more ecological niches than could emus, and some of them could browse much higher above the forest floor than could the vertically-challenged emu. So emu carrying capacity

is unlikely to equate to moa carrying capacity, even if environmental factors were equal. A bit like comparing organic moas with motor mowers. One chews twigs, the other chews grass.

It has to be granted, of course, that deer carrying capacity may not be comparable with moa-carrying capacity Deer, being ruminants with four stomachs, may be capable of eating a much wider variety of foliage and getting more nutrition from it, and therefore may be able to thrive at higher biomass densities than moas with only one stomach. But we should not to be too sure of this. Analysis of gut contents in Pyramid valley moas (Burrows 1989) showed that they ate large quantities of quite coarse twigs as well as podocarp foliage not favoured by deer. The grinding gizzard of moas would have been a partial substitute for the teeth and rumen of deer, and, according to Burrows, "the Dinornis moa seemed to have behaved like deer, cattle or goats, in taking a variety of food items.... Presumably their digestive system was similarly specialised to cope with lignin and cellulose."

Even today, with competition from possums and a habitat restricted largely to conservation land, commercial forest and Maori land, there are 250,000 wild deer in New Zealand (NZ Department of Conservation 1997). The numbers are this low because of hunting pressure, particularly from helicopters, not availability of food. Back in the 1950s, before the advent of helicopter shooting, Wildlife Branch cullers were killing 40-60,000 deer per year, and no-one knew how many were still alive.

There are also about 300,000 feral goats, ranging over a mere 11% of New Zealand's land area. Goats, despite popular perceptions to the contrary are quite choosy eaters (Parkes 1993), and thus might also be good for modelling moa numbers.

By contrast, in pre-Polynesian days the moas had access to almost the entire land area of New Zealand, and their habitat was mostly of better quality than that available to deer today. No possums, and only *Harpagornis* circling overhead rather than helicopters.

How Many Moas?

Holdaway & Jacomb built up their 158,000 total population figure from densities of moas which various environments could carry. They were calculated to range from 1.5 birds per square kilometre in the drier podocarp

and beech forests to 1 bird to every 7 square kilometres in the alpine areas. It is worth looking at some of these environments.

Stewart Island was estimated to support only about half a moa per square kilometre. The main deer down on Stewart Island today is the smallish white-tail, which at 54 kg body weight, is about equal to the medium size moa, *Emeus crassus*. There are about 25 white-tails per square kilometre in coastal habitats and ten per square kilometre inland on Stewart Island (Davidson and Challies 1990). In other words Stewart Island supports about 20 times as much deer biomass today as it did moa biomass in pre-Polynesian times - if we believe Holdaway and Jacomb.

The Blue Mountains (Otago) are in a zone estimated to carry 0.9 moa per square kilometre. In 1985 the area carried 6 fallow deer (and see Caughley's comment above) per square kilometre (Nugent 1990), with body weights of about 50 kg.

Inland Marlborough beech forest was estimated to carry a mere 0.15 moas per square kilometre. In 1992 a study area here carried 17 goats per square kilometre (Brennan *et al* 1993). Feral New Zealand goats weigh in within the weight range of the small *Megalapteryx* and *Anomalopteryx* bush moas.

Based on this I would like to suggest - conservatively of course, and for purposes of archaeological speculation only - that the pre-Polynesian moa population of New Zealand was, say, five times that suggested by Holdaway and Jacomb - i.e. a population of 850,000. Those birds were spread unevenly over the country at an average density of 3 per square kilometre.

This is not very different to recent figures from a wildlife biologist who has suggested there were 2-4 smaller moas and about half a larger moa co-existing per square kilometre (Speedy 1998). He derived this from comparison with emus, kiwi and deer (*pers. com.*).

Feels Good

To me this "feels" like a reasonable figure, when one reflects on the vast quantities of moa bones seen on some large South Island moa-hunting sites or those dug out of swamps. At Pyramid valley, 2000 moa skeletons accumulated in a 1.2 ha swamp over 3-4000 years (Duff 1952, 1955), and there were other swamps more crowded than that. Back in the sixties Ron Scarlett and I were mining moa bones in South Island limestone caves and sinkholes for Canterbury

Museum, and it seemed to us that moa bones had been accumulating faster in pre-Pakeha times than deer bones (bush locations)were in the 20th century.

Holdaway and Jacomb's wet podocarp forest zone where there was supposedly less than half a moa per square kilometre, covers all of the North island except Hawkes Bay-Wairarapa. At Waitomo, within that zone, local Maoris were mining moabones in pre-pakeha times for making fish hooks etc, and for selling to tourists right up until the 1940s. Along the King Country coastline, also within the wet podocarp zone, there are amazing numbers of moa gizzard stones to be picked up in sandhills and loess-like soils. The moahunter site at Kaupokonui, in Taranaki, where there was evidence of "an orgy of hunting and eating" moas (Anderson 1989: 116-8) is also located within this zone.

Several of the ecological papers I am quoting come from a seminar on "Moas, mammals and climate in the ecological history of New Zealand," held by the NZ Ecological Society in 1986. One of the main things that came out of that symposium was the surprisingly large impact that moa browsing has had on the vegetation of New Zealand. This is shown in particular by the considerable numbers of plants that have evolved defences against browsing by moas - the divaricating habit of Coprosmas and matagouri, and the unpalatable juvenile foliage of lancewood, for example. There are quite a few plants with palatable foliage which are deciduous so as to deprive moas of their food just when they need it most, and there are quite a few poisonous plants (poroporo), stinging plants (ongaonga), prickly plants (lawyer) and spiky plants (spaniard grass). Hookgrass used to use passing moas to spread its seed, now it has to rely on the hairy legs of passing trampers. (Batcheler 1989, Atkinson and Greenwood 1989). It is still a heresy in some circles to believe that moas really were responsible for all these adaptations. If moas were responsible, then it must have taken quite a population of them, even allowing for the fact that millions of years were available for it to happen.

How Much Moa Was on the Menu?

Another Holdaway-Jacomb assumption that sounds dodgy to me is that the moahunters were eating moas at the rate of one female moa per 20 people per week, or, assuming that similar numbers of male birds were also eaten, one moa per 10 people per week. Given that your average moa carcass carried about 55 kg of meat (Anderson *et al* 1996, Table 14.8), this works out at about 5 kg of moa flesh per person per week. Such an assumption might be true for a place like Shag River where 35% of the 610 tons of meat represented by midden remains was derived from moa. (Anderson *et al* 1996, Table 14.8). But most

sites where moa was consumed show very much lower percentages of moa in midden, especially in the North Island, where marine mammals were probably much more important to the early settlers than moas ever were.

Holdaway and Jacomb also ignore the likelihood that consumption of moa flesh per person would decline over time as moas became less abundant and more wary and therefore harder to catch. And once moa populations got really low the hunting of them probably became opportunistic rather than methodical, so that small numbers of moas could have survived in isolated areas for many more years, as the Takahe did in Fiordland.

When is A Moahunter Not A Moahunter?

The archaeological part of the paper is most unsatisfying. To test their model Holdaway and Jacomb needed to determine from archaeological evidence when moa hunting ceased. They did this at one single site, Monck's Cave, Redcliffs (a suburb of Christchurch), which they interpret as a site "transitional between moa-hunter and Classic sites" [sic], but one which failed to yield evidence for moa consumption, even though moahunters had been gorging themselves on moa meat at Moabone Point Cave, a mere two bus- stops away. If moas had still been around the would-be moahunters of Monck's Cave would surely have hunted them. The fact that they apparently didn't means that Monck's Cave can provide a terminal date for moa-hunting. (I paraphrase somewhat unfairly.) Eleven radiocarbon determinations from the cave showed that moa-hunting had ceased at the cave and therefore in this major moa-hunting area (Banks Peninsula) by the late 14th century. Since Polynesians only arrived in New Zealand in the 13th century, it can only have taken them one century to eat all the moas at Redcliffs, if not Banks Peninsula, and the Leslie matrix model is found to be vindicated.

That is it. There was no attempt to look at all the radiocarbon dates for moa-hunting obtained throughout New Zealand. They didn't even try to dismiss evidence from no further away than the other side of Banks Peninsula, where, at Tumbledown Bay, there are good radiocarbon dates for moa-hunting which are later than the Monck's Cave dates (Petchey 1997). There was no consideration of the possibility that moahunters were temporarily shacked up in Monck's Cave for some reason other than moahunting. They may have got sick of poultry up in the hills and come down for the kaimoana, who knows? They may have cooked their moas out on the Redcliffs flat, where it seems everyone else in the neighbourhood did (Trotter 1975). There might have been a seasonal rahui in force.

Holdaway and Jacomb didn't attempt to dispose of the admittedly controversial claim by the first investigator of Monck's Cave, H. O. Forbes (1891, see Anderson 1989: 128), that the lower levels did in fact contain "numerous longish fragments of moa-bones, partly burned and partly broken, scattered around the fireplace..." The heavily-fossicked Monck's Cave simply is not the best place to determine when moa-hunting ceased.

Conclusions

To my mind Holdaway and Jacomb have not made a good case for rapid extinction of the moa. But more importantly I think they should have made their case locally, to an audience or readership familiar with the evidence and issues, before going for overseas exposure. Neither the editors nor most of the readers of *Science* know anything about Monck's Cave, Moabone Point, Tumbledown Bay or Pyramid Valley.

The allegations in *The New York Times* of "blitzkrieg extinction" are not justified. The very word "blitzkrieg", I know, has a specialised usage in the biological sciences (Mosimann and Martin 1975), but it also has emotive overtones. We all know which little man with a toothbrush moustache ordered the first blitzkrieg. Eating moas was not a carefully planned campaign to acquire lebensraum, nor was it intended to be a "final solution" for wiping moas off the face of the earth. There is no justification for whitewashing environmental destruction by Polynesians, but there is no need to over-dramatise or exaggerate it either.

Even if we stick with the narrow biological definition there is no evidence (and Holdaway and Jacomb didn't claim there was) that a blitzkrieg - a linear or radial rolling wave of moa extinction from one end of the country to the other - happened. Both the randomness of the radiocarbon dates up and down the country for termination of moahunting and the fact that the moahunters knew all the important stone sources for artifacts before they had finished off the moas are evidence against a blitzkrieg (Anderson 1989: 181). Worthy (1997) examined collections of midden bird bones including moa from 177 archaeological sites throughout New Zealand and found no evidence of a moa blitzkrieg, although he thought some moa species were probably eliminated from some localities in quite short time periods. The only blitzkrieg (can someone please find a better name soon!) Worthy thought possible was against mainland muttonbirds.

Looking to the future of this debate, I would be interested to learn what the Leslie matrix model can do with 850,000 moas.

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Note: A paper by Atholl Anderson on the same topic has been received too late to appear in this issue. It will be published in the September 2000 issue.