NEW ZEALAND ARCHAEOLOGICAL ASSOCIATION

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CREATION MYTHS AND THE ORIGIN OF PA

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In New Zealand the study of the origins of Classic Maori culture is as old as archaeology. It may be that some answers to the old question of culture change will be found in ethnohistory, in studies in midden analysis, or perhaps they will be dug out of stony gardens; all of which are fields of much current delving. Another traditional starting point is the settlement evidence but, for the most part, the site survey reports produced by the Historic Places Trust are languishing in their covers and, in the computer Index, some 30,000 sites are substantially tied up by the cumbersome programs intended to retrieve them.

Among this evidence pa sites stand out as our major field monuments. At the end of prehistory at least, they seem often to have been focal points of settlement. More than 25 pa have been excavated, in fits and starts, and reported at least in part. Yet generally the evidence has been intractable and difficult to relate to other kinds of data. In the intervals when it has been ignored, the problem has not gone away. Now it seems possible that it may be the sites themselves that will disappear. On the soft sandy soils of the Kaipara heads, both north and south, preliminary investigations suggest that approximately half of all pa have suffered conspicuous damage in each decade since the beginning of aerial photography.

Theory about the origins of pa

To make further progress it seems necessary, (1) to get rid of lingering preconceptions about New Zealand prehistory; (2) to separate the threads of different ideas at least at the stage of data analysis; if they get interwoven too soon - as with fortifications and agriculture - the pattern is blurred; (3) to clarify theory to the point where it is able to be investigated in the field. If nothing else we can decide which ideas to jettison.

Theory about pa lags behind other areas of study. Their origins are still put into a two-phase model. Morever agriculture is assumed to be at the root of culture change, of which pa are seen as a primary manifestation. Part of the ideological baggage of an imported archaeology was that New Zealand prehistory was a little mirror of world prehistory. After a century it is now only quaint that to von Haast it was at one time a technological palaeolithic followed by a neolithic of ground stone tools. However,

we are less free of the ramifications of an economic neolithic revolution. To Duff (1956), after a period of hunting and gathering, it was the Great Fleet that brought the vegetables. Then, more than anyone else, it was Sharp (1956) who sank the Fleet Maori. When Golson wrote his paper on prehistoric culture change in 1956, a continuous sequence was likely and by then, in a world context, neolithic meant food producing by definition (Green, 1972). At the Coromandel excavations of the 1950s Golson raised the possibility that storage pits were associated with Archaic middens. It is surprising though, that it was not until the early dates from Palliser Bay and Wiri (Leach and Leach, 1971; Sullivan, 1975) that what might almost have been taken for granted, gained acceptance.

Now in our belated wisdom, we accept that Polynesian cultigens could have been here from the beginning. Since subsistence is seen as so central to culture change, a few more general points can be made.

1. Yen's (1961) model for the adaptation of the kumara was something of a red-herring at least in its setting of chronological stages which sit unhappily on the New Zealand evidence. It could scarcely be redeemed by a climatic change model. In New Zealand climatic variation certainly occurs in space. Whether it did so in time (beyond seasonally) is much more arguable. Chronological climatic models are a rickety prop for theories of agricultural and culture change.
2. Kathleen Shawcross' ethnohistoric work (1968) may have set us on a false trail too. As a cautionary tale about the need to be very careful with the documentary evidence of contact and early post-contact history, it stands as a major contribution. As an assessment of the relative importance of fernroot versus kumara beyond a particular time and place, it may be much less useful.
3. The belief that primary forest was necessarily difficult to clear (see e.g. Vayda, 1960), seems poised to topple (McGlone, n.d.).
4. While it is accepted that the Polynesian plants growing at the time of contact reflect the pattern of survival rather than arrival, we are still rather more generous towards the banana, coconut and breadfruit, in this regard, than to the fowl and pig.
5. On the credit side, some of the current work on agriculture is of great promise. The importance of the species other than kumara and fernroot is being considered. The nature of vegetation succession, the length of the fallow, etc., is being reviewed (Leach, 1980).

Yet although the bugbear of a mechanistic or chronological-stage model for agriculture has been squashed, the intensification of warfare and the development of fortifications are still seen as contingent on agricultural models that we might now be loathe to accept.

A vegetable theory of creation

Perhaps unfortunately, vegetables have taken a tyrannical hold of theories of culture change. To parody the case, a scenario for the origins of pa could go like this, and is close to what is proposed. Once upon a time in New Zealand prehistory there was a particularly optimal environment. It was a bit like the Garden of Eden except that its inhabitants occurred by migration instead of creation. The soil was excellent and the climate was too. Frost was unknown. We do not know where this place was, but archaeologists often call it "Northland". Up there the vegetables grew wonderfully well. (How this related to the fate of the moa is not essential). The people multiplied and that was the cause of their subsequent troubles. Instead of finding social solutions to the problems of numbers - as their Oceanic ancestors in finite habitats may have done for millennia, and still do - our innocents in Northland settled for competition. They competed for land to grow ever more vegetables to feed their expanding population. Vayda (1960) said competition was for cleared land, but land in forest would seem to offer more potential for swidden especially if it was not so difficult to clear as thought. As the population multiplied, they began to engage in quarrels, skirmishing and warfare. Then, if they did not invent fortifications, they dredged the idea up from the bottom of their Polynesian folk memories. The assumption of course being that they did not have it until they badly needed it. Another solution they tried was outward migration from Northland; witness the theories on the movement of the Awa people along with certain styles of pa form (Groube, 1970). And finally, in this new context, with the emergence of a new political order, the social climate was ripe for legitimisation with a new style of art (Mead, 1975).

The problem with these theories does not lie with their plausibility, but with their testability. The outlines of the story are not changing very much, but we seem to be getting no closer to verifying or throwing them out.

A few objections can be made. Firstly to what extent was vegetable power the driving force of culture change in New Zealand? Neither demographic theory nor Pacific ethnography suggest that human population size and structure is simply determined by the nature and quantity of resources. Because people survived New Zealand prehistory, presumably they had enough to eat. Yet that fact alone is not sufficient to explain why their number grew perhaps from 20 to 200,000. Nor do the circumstances of subsistence explain, for instance, why people who had the capacity to sail here from central Polynesia, later allowed themselves to

become marooned. They do not explain why art, which permeated the culture even as far as decoration of the body, underwent major change. This was not something directly related to ecology.

A second objection to the model is the role played in it by migration. The population simply boiled over in Northland and a stream of people ran south down the island. Fox (1976) made the point that the spread of Groube Type 3 pa could be the diffusion of an idea. However, even if it is reasonable to accept that people were on the move too, it may be an oversimplification to see a migration as a fairly sudden and discrete event. That might be rather too coincidentally neat and tidy as an explanatory device. Some ethnographic and archaeological cases (e.g. Terrell and Irwin, 1972) suggest that traditional migrations may have been more continuous processes of longer duration without such a dramatic effect on local culture history. In fact some of the elements of the Awa traditions, which Groube regarded as anomalies in terms of the migration theory (1970:154), may genuinely reflect that internal movements were commonly more or less continuous although with considerable variation, through time, in their scale, frequency, personnel, direction, and social and economic content. Melanesian ethnography suggests too that generally when people travelled to exchange, trade or even fight, they had established social (and antisocial) links with the people on the route.

A third objection to the Garden of Northland model is in its sequence of events. For example, if fortifications had been present long before the onset of the hypothetical population pressure, then that may have made a military solution more likely than say an increase in abortion and infanticide.

A final objection is that the model lacks sociological content. In Oceania food is usually more than mere subsistence. It is also an established medium of competition, status and power. In its patterns of production, display and redistribution food is intrinsic to social identity and the political system. In the Trobriand Islands, for example, the relative rank of chiefs is directly expressed in the size and splendour of their yam houses as well as by the number of their wives. And these factors, in turn, can be predicted from spatial and demographic aspects of settlement (Irwin, 1982). The kumara does not lend itself to such conspicuous storage as the yam, however Frederickson (n.d.) argues that pa could be descended from original elevated, undefended displays of stored food. The value of this suggestion may be in its reminder of the interplay of sociological and ecological variables in culture change.

A strategy for the study of the origin of pa

It remains to be shown that any one part of the North Island offered significantly better prospects for an early centre of population density and culture change than the rest. In terms of soil fertility, rainfall, drainage, the incidence of frost, marine resources and forest, there is little to choose between many areas north of Hawkes Bay and Taranaki. Population growth could have been almost instantaneously early in many places. However, it is still possible that some parts of the north may have been particularly favoured. If one regards communications as an important variable, then the Kaipara, the inland Bay of Islands and especially Tamaki become strong contenders. Yet how could we locate an important centre even if such a thing existed? The chance of finding the crucial early archaeological sites is not high. Perhaps the best way is to use the indirect evidence of the history of human interaction with the environment. Once the pollen record is more complete in its coverage we will be in a position to identify any early northern centre of settlement, or to discard the idea at last.

At present then, the difficulty lies in choosing a place where field work may have a bearing on theoretical issues. The obvious archaeological starting point is the settlement data. Over the past two years, as part of their coursework, anthropology students at Auckland have helped in the interpretation of site surveys previously commissioned by the Historic Places Trust. A number of issues are involved.

First it is necessary to look at pa sites to consider the range of variation in their form, location and size. If one had a model of the evolution of form (say, from small early ring-ditch to large elaborate terraced forms), it would be necessary to work in an area that exhibited most forms. However, if one thought that pa form was a function of topography, it would be necessary to make sure that that dimension was well-represented in the study area too.

This kind of study involves pa classification which has exercised archaeologists from Skinner (1911), to Firth (1925), Best (1927), Buist (1964, 1965), Golson (1957), Golson and Green (1958), Groube (1964, 1970), Bellwood (1971), Fox (1976) and Prickett (1980). It would be fair to say that the current classification owes most to Groube who emphasised morphology. Since then Bellwood noted functional criteria, although most of the points made had been quite well-aired beforehand. Aileen Fox accepted most of the Groube typology but argued additionally that the form of defence reflected the mode of attack. She also

devised a scheme for the development of defensive devices. Yet despite all of the past work one can still make two points. (1) The gathering and analysis of pa data is still scientifically primitive. We have little ratio-scale measurement. Except in rare cases, such as Prickett's Taranaki work, it would not be possible even to draw an accurate graph of pa size. (2) In general, each successive scheme of pa classification has tended to use alternative rather than additional criteria (such as location, topography, morphology and function). While New Zealand prehistory has many thousands of forts, it is ludicrous that it should have only four types. We are only at the stage of identifying attributes. Pa classification, perhaps even measurement, is still in the future.

Another important issue is that although over 25 pa have been excavated there has been a tendency for attention to focus on single sites. In fact, sites do not exist in a vacuum, but in a context that includes relationships both to the environment and to other contemporary sites. Inspection of all the detailed site surveys that have been undertaken in Auckland, Northland and Coromandel shows that in some areas the distribution of sites is dense. More importantly in some of the distributions - not others - the pattern of sites suggests a spatial coherence. Further, on making comparisons between different areas, the overall patterns of settlement can be very different. These differences are so systematic that we can be confident that they are not simply a figment of sampling error.

The obvious objection to distributional studies is lack of control of the time dimension. Admittedly any settlement pattern is just a collection of sites of any age minus those unsuspected, undiscovered or already destroyed. Yet despite this, we can be sure that there was a time when that area was under some kind of natural climax vegetation, was subsequently first settled, and that through time, in some way, a human population spread. Archaeological fieldwork ought to be able to show, if only in a coarse-grained way, how a particular settlement system matured through time. In fact far from being defeatist about our ability to identify contemporary sites on account of dating difficulties, we can use distributional arguments to help predict chronology.

To summarise, in order to study the origins of Maori settlement patterns, emphasising pa, one needs to work where there are enough sites to cover much of the known range of formal and locational variation. Also the distribution patterns of sites of all types should suggest a coherence of man/land and intersite spatial relationships. In such a case it may be possible to investigate how one settlement pattern developed. However, in

order to shift explanation from the particular to the general one needs to compare a number of different kinds of settlement. In other words the settlement system rather than the site is the unit of analysis and a number of case studies are required.

The pa of Pouto

A first such study was carried out between December 1981 and February 1982 in the Pouto region of the North Kaipara Head. This area was chosen for a number of reasons.

1. Both heads of the Kaipara have received a lot of archaeological attention over the last 25 years.
2. Whereas the proximity of the South Head to Auckland city has led to rapid changes in land use, the appearance of deer farming and orcharding, the North Head is more remote and it is easier to do field work there.
3. Unhappily for posterity, the pa of Pouto are eroding badly. Most of the coastal sites are gradually falling into the sea and as such are neatly half-sectioned. Only one is covered in retirement and holiday houses. Inland, the pa are badly damaged, especially by cattle, and display extensive eroded sections, which it is a simple matter to clean up and examine. Thus while the destruction makes archaeological work more pressing as a rescue exercise it also makes it more rewarding for research.
4. The environment of Pouto is a particularly varied and favoured one for habitation, which may lead one to expect there to be a fair time depth of occupation. Certainly traditions and history suggest the Kaipara heads to have been quite densely settled until approximately 1820.
5. The archaeological sites in Pouto are dense, varied in form and location and apparently coherent in distribution.
6. There are some particular advantages for sampling. For instance, while the various ecological zones tend to lie north-south along the peninsulas, the zones are transected by a series of creeks and estuaries which extend inland from the harbour. These offer natural physiographic sampling units and moreover the archaeological evidence is to an extent replicated from one to the next, offering the opportunity to apply various models about expanding prehistoric settlement.

In all, over 20 pa were mapped and surveyed by gradiometer. Their sections were examined and general stratigraphic histories noted. Samples were taken for soil study and dating. Limited test excavations were carried out on a number of pa and extensive excavations were carried out on two. A peat core was taken from a lake-edge and pollen is being extracted for study. The results of this work will be reported in a forthcoming paper.

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