



NEW ZEALAND
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NEW ZEALAND ARCHAEOLOGICAL ASSOCIATION NEWSLETTER



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REVIEWS

PRACTICAL ARCHAEOLOGY - AN INTRODUCTION TO ARCHAEOLOGICAL FIELD-WORK AND EXCAVATION:

Graham Webster, London, Adam and Charles Black, 1965. 11 photographs, 20 line illustrations, 176 pp., index, 21 s (England).

This book is described as the "Companion volume to Approach to Archaeology by Stuart Piggott" in which the author "attempts to set out for serious amateur and young professional archaeologists the back-ground for practical fieldwork and excavation in Britain." Written by an archaeologist "who has spent most of his working life on the problem of the Roman occupation of Britain" it draws more attention to the problems of structures and stratigraphy than to other features found in quite different types of archaeological sites. Within the limitations that it is written for the British amateur, draws on British excavations and stresses structural fieldwork and excavation, certain aspects of the book still lead me to commend it to the New Zealand archaeologist.

The chapter on archaeological organizations and publications in Britain, is, for one not familiar at first hand with the situation, useful in understanding the roots of our own organization and the many parallels shared with Britain. Differences suggest fruitful lines, not yet developed here which we could well pursue.

The chapter on investigation by field-work offers little for the New Zealand archaeologist, except to remind us that in the field of geophysical prospecting we can hardly be said to have begun to exploit the possibilities.

On the other hand the chapter on investigation by excavation is a useful discussion for any amateur or group in New Zealand contemplating tackling a pa or similar site with extensive structural evidence. The treatment of excavations methods, particularly for defensive earthworks, is helpful and worthy of study. The discussion of the more recent open stripping method (opening large areas at one time without control baulks) is also relevant as it suggests that while the problems associated with this technique are considerable, "in time it will become the standard practice in spite of the difficulties." At present it demands much technical skill and financial resources beyond the scope of all but the state, opens problems of efficient soil removal, and necessitates an excavation being carried out in one operation or a connected series of them. The relation to Groube's (1964) call for New Zealand archaeologists to practice more area excavation is obvious, but the difficulties noted above indicates clearly, why for a long time to come, the practice of this technique will of necessity be restricted to one or two University groups who can overcome these problems, however desirable its more general practice may be.

The chapter on scientific examination and classification of finds and samples is again one of less use to the New Zealand archaeologist, but the chapter that follows on publication, especially drawings, is one that could benefit all contemplating writing up some of their findings.

In sum, the book has its limitations, and while these restrict its value for the New Zealand archaeologist, it is not without merit in the fields indicated above.

R.C. Green

REFERENCE:

Groube, L.M. 1964 "Archaeology in the Bay of Islands", Dept. of Anthropology University of Otago (Mimeographed)

ARCHAEOLOGY AND THE MICROSCOPE:

The scientific examination of archaeological evidence.

Leo Biek. London, Lutterworth Press, 1963. 2 colour and 24 monochrome plates, 12 figures and 8 tables, index, 287 pp.

"This book is not about microscopical techniques applied to archaeology, nor is archaeology as such under the microscope". Rather it stresses the co-operation necessary between the laboratory research scientist whose modern techniques can be of considerable aid to the archaeologist and the archaeological field-worker. The book is frankly programmatic; it explores what has been done, what are the limitations, and what is desirable. The examples are drawn mostly from the author's own experiences or refer to work done or being carried on in Britain. The essence of the author's argument is certainly valid and the need for such co-operation and the role of the archaeologist in interesting and involving scientists in similar studies in New Zealand cannot be denied. But to use this book as a basis for such

developments in New Zealand requires retranslation of most sections into a New Zealand framework and the use of examples more relevant to the New Zealand scene than those cited. In short, the book serves neither as guide to practical action for New Zealand archaeologists seeking to develop the scientific laboratory study of their findings nor is it particularly helpful as a text of techniques that would be of particular benefit here. For those interested in the general problem it provides background reading and references, but the book is not one that I would recommend as useful to most Members of the Association.

R.C. Green

A REVIEW OF THE PREHISTORIC SEQUENCE IN THE AUCKLAND PROVINCE

Roger C. Green. Auckland 1963. Monograph No. 1, Auckland archaeological Society; Monograph No. 2, N.Z. Archaeological Association. 5 line illust. 114 pp. (Distributed by A.G. and A.W. Reed Ltd., Wtg.)

In 1959 (Golson 1959:35), Jack Golson predicted that standard sequences linking one aspect of his Archaic phase with a number of aspects of Classic Maori would be found in the North Island. Dr. Green has recently attempted to provide a sequence from the Auckland area, the aim of which is summarized "I do have some hope that it may stimulate investigation into problems and evidence that has been largely ignored and force New Zealand archaeologists to expend additional intellectual effort in applying a broader and more refined set of concepts to the analysis of those materials they are now viewing from the ground by means of precise and sophisticated techniques of excavations." (Green 1963:10)

Briefly, what does Green attempt to do? Following Cumberland and Lethwaite, he divides New Zealand into three geographical areas according to distribution of late Maori population. Iwitini, the area north of the volcanic plateau but including the east and west coastal strips with the densest population and most varied resources; Wanganui, the rest of the North Island and a strip of the North and Upper East coast of the South Island, with colder climate, fewer resources and restricted access to the coast; and Ta Wahi Pounamu, the rest of the South Island with its less favourable conditions for the development of a large population and related type of culture. Following Golson, Green considers the Iwitini to be the focus for the development of what is known as Maori culture. This is a good hypothesis provided, of course, that the favourable conditions themselves do not complicate the record unduly by attracting too many intrusive groups.

The main part of Green's Review is contained in his chapter on the "Organization of the Evidence", that is, the "theoretical framework within which an archaeologist manipulates his evidence" (Green 1963:17). It is basic to Green's theory is that one culture may evolve out of another under certain conditions and come gradually to spread and displace the earlier culture. In this case, a group possessing Kumara agriculture could have achieved such an advantage. In the Iwitini region, Green considers that three factors led to the development of what he calls Maori culture as opposed to the earlier New Zealand East Polynesian culture.

1. Innovations in isolation, as adaptations to a non-tropical environment, must have been continual.
2. The favourable ecological setting for the development of a new variety of systematic agriculture.
3. Introduction of trait units, as a result of later landfalls by individual canoes; including Kumara, some forms of defence, and artistic motifs.

This is the basis of Green's argument. A New Zealand East Polynesian culture begins prehistory in New Zealand. Out of this, by a series of phases, Maori and European-Maori culture develop.

The phases used are defined according to the Guttman technique, in which a number of traits are set up on a scalogram and their presence or absence noted. Dr. Green in a letter has drawn my attention to a paper by D.J. Tugby "On the Use of Scale Analysis in the Study of Culture" (Tugby D.J. 1964 No. 181). Dr. Green remarks "using this model or method it is possible to break up a cultural sequence into stages which emerge from the analysis itself and are not imposed in an a priori fashion upon the data ; a defect of this type of analysis, which is common to many American sequences, lies in the selection of criteria. Even presuming that the use of unilinear evolutionary stages were valid in a particular region, by selecting those criteria which would scale satisfactorily to achieve the desired result, the analyser is imposing a form on the data which may not be present if the total range of traits is considered. It could be argued that in selecting only those traits which are not likely to persist, cultural discontinuity could be emphasized, whereas by considering only durable traits, continuity can be gauged. Implicit in the Guttman technique as applied by Green, Tugby, and others is the idea of a broad cross-cultural evolutionary framework divided into a number of stages through which all societies pass or have passed. This is no new concept, being as old as archaeology itself, and may be traced through Morgan, Tylor and Nilsson (Daniel 1950:184).

While Dr. Green probably has not intended to bring out such inferences, to the reader what has been discussed before under the titles of Settlement, Developmental Experimental and Classic, notably in reference to the American sequences, cannot help but influence his judgment. Perhaps it should be emphasized that the stadial inferences made are not historical facts but merely a method of studying the data. There is a fundamental

difference of approach between the interpretation of sequences when made by people trained in the European in contrast to the American school. The former sees the evidence in terms of history, the latter in terms of social anthropology. This difference is itself historical in that social anthropologists in America were able to study living cultures little different from the prehistoric cultures of the same areas. Information was thus available to American archaeologists which enabled them to interpret their material much more fully and along lines different from their European colleagues. Green has followed Golson in attempting a synthesis of these two points of view. In his review (Green 1963:96) he specifically compares archaeological and ethnographic concepts of culture, phase, component with culture, sub-culture, tribe or aspect with iwi, hapu or sub-tribe (Green 1963:95). Green's clusters of traits which he uses to construct his model are:-

"In each of these phases definitions of the agricultural stages based on Yen (1961), the earlier settlement pattern types based on Willey (1960), and the later ones on a modification of the community patterning in Beardsley et al (1956), and the climatic interpretation that of Cumberland."

The climatic interpretation is rather interesting in that as far as any geographers I have consulted are aware only one portion of it, that relating to the hypothetical climatic deterioration which affected forest cover about 1200 A.D. is commonly known. Green's changes are:-

Settlement Phase 900 - 1100 A.D.

Climate - slightly warmer and drier than today.

Developmental Phase circa 1100 - 1350 A.D.

Climate - slightly warmer and drier than today.

Experimental Phase circa 1350 - 1450 A.D.

Climate - first deterioration of climate toward a cooler and damper phase.

Proto-Maori Phase circa 1450 - 1650 A.D.

Climate - somewhat cooler and damper than today.

Classic Maori Phase 1650 - 1800 A.D.

Climate - somewhat cooler and damper than today.

Early European Maori Phase 1st Half of the 19th Century.

Climate - at the end of this phase a return toward a slightly warmer and drier climate.

The introduction of kumara agriculture is postulated for the Experimental Phase c. 1350 - 1450 A.D. with the presence of separate storage facilities as opposed to those of the earlier Developmental Phase (1100 - 1350 A.D.) where "storage facilities are directly attached to dwellings" (Green 1963:101). Yen, for the sake of argument, uses a 14th Century date for the introduction of kumara based on "traditional" evidence (Yen 1961:338) as to the arrival of the Maori ancestors. Green uses this date, but dissociates himself from its origins by saying "I do not find it necessary to adopt the traditional point of view" (Green 1963:22).

The presence of what is called storage attached to dwellings at the earlier phase (Skippers Ridge site) depends on the interpretation of these units as dwelling and storage. Recent work by Les Groube (Groube 1964) suggests that the whole complex may be for storage. If this is so, then the problem requires much more study. In naming his sequence, Green has chosen terms which are interesting but likely to become obsolete very quickly as more information comes to hand on the process of cultural changes in New Zealand.

The changes in settlement patterns included in the sequence while they may be correct are not demonstrably so. In summary, the changes noted are, in settlement phase, which is characterised by single camp sites with all activities within a single site, followed by the Developmental phase with restricted wandering around a central base, more substantial housing, pit dwellings, and successive midden layers deposited at short intervals. The Experimental phase has separate storage facilities and "the style of dwelling unit alters" (Green 1963:56). Extensive layering of beach middens still occurs.

In the Maori Culture, the Proto-Maori phase is marked by erection of pa, and semi-permanent community patterning with the central village established in successive locations. New architectural forms appear and middens consisting of both mudflat and beach shells accumulate to great depths. As well as fortified villages, Green also postulates unfortified settlements.

In the Classic Maori phase, some people dwell permanently in "large internally differentiated settlements based on a greater complexity of social organization" (Green 1963:39). This is followed by an interesting statement "By now the processes of segmentation and stratification among the major social groups should have proceeded far enough to be recognizable in the archaeological evidence of the interpal arrangements in the pa and the appearance of structures devoted to special activities" (Green 1963:39). This is an interesting general statement not supported by any detailed studies.

Probably the most apt comment on this sequence is Green's own, "This conceptual framework is a theoretical construction which I have derived from a survey of the existing literature, not something discovered simply by excavating sites" (Green 1963:41) - (Underlining mine). Extreme caution needs to be exercised when interpreting field evidence in the light of such theoretical concepts.

The survey of sites arranged according to the sequence is not very convincing. In the Settlement phase is included Tairua Layer 2 with settlement of a camp type with "a few postholes pits and a large oven". Presumably the pits were for storage of some kind, also presumably Kumara which did not survive though the climate had not yet (presumably) deteriorated, and the plants were still being cultivated according to Polynesian custom. Pit dwelling is said not to occur as a feature until

the next phase. Pits for fermenting breadfruit are known in Polynesia but there is no botanical or other evidence for this tree being able to grow or to produce fruit in New Zealand. In any case, breadfruit cultivation requires a type of agriculture and settlement different from that postulated, unless this was only one site of a "semi-permanent community pattern."

(Reference to the Tairua Site Report by Smart Green and Yaldwyn, Dom. Museum Records Vol. 1, No. 7, will clarify the ambiguity in the word pits which occurs in the Monograph under review. Ed.)

The wide range of Moa at this site, according to Scarlett (1962:247), consists only of three species, Dinornis novaezealandiae, Dinornis giganteus and Eurapteryx geranoides if none have been added since. The criterion appears to be the appearance of the Dinornis forms not the number of genera or species. Again further information is needed about the survival of the Dinornis forms in Auckland. In the south of the South Island, admittedly a remote area, Lockerbie (Lockerbie 1959:81) has demonstrated their survival until about 1670 plus minus 60 A.D., this in an area where kumara agriculture was not possible in late times and hunting continued to be the mainstay of the economy until the introduction of the potato about 1800 A.D.

Criticisms of a like nature could be made of the attribution of other sites in the various phases, but they all share a common factor. Artefacts are briefly mentioned in only the vaguest of terms. On some of the sites, of course, they are rare, whereas structural evidence is abundant. This is especially true of the pa excavations. However, to be convincing the sequence needs to make more than passing mention of portable artefacts. The impression is given (see Green 1963:57-69 etc.) that there may be a certain amount to commend the suggested relative placement of some of the sites, other sites either do not fit in or are not mentioned in sufficient detail, nor are they judged upon sufficient criteria to be at all convincing. It would seem that one of the primary factors in placing the sites is obsidian dating, the data for which has now been published (Green 1964:155), and a few radio-carbon dates. The other criteria mentioned by Green are worth considering as they sometimes direct thought along new lines. The overall scheme has some merits but is not definitive, nor is it intended to be. Green's sequence is an attempt to handle the structural evidence correlated with ecological and economic inferences. Some aspects of the sequence have been considered here, but it is much too early to say whether Green's theory will have any lasting affect on the interpretation of New Zealand archaeology. Much more research is needed, and Green's study poses quite a few problems which will help direct the course of this research.

"Thus it is my hope that the cumulative knowledge which a closer analysis of these new materials must yield will in the end bring about the extensive modification of the views presented here." (Green 1963 :11)

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