

Pattern and Purpose in Historical Archaeology

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The practice of archaeology inevitably requires that we constantly assess the activities of both ourselves and others, as well as asking what the purpose of such activities might be. In the case of historical archaeology in Australia and New Zealand, there seems at present some danger of losing sight of what it is that we are trying to do and why. In origin archaeology is a scholarly discipline and yet, after three decades of growth in our respective countries, historical archaeology remains poorly represented in academic institutions both in staff and in undergraduate courses. In contrast, there has been a substantial increase in private archaeological consulting and in state-funded cultural resource management activities. Consequently, in spite of some remarkable exceptions, greatly increased archaeological activity has generally resulted in only modest additions to the body of published material that in the end constitutes the discipline. It is surely timely to ask just what we are contributing to the study of archaeology as a whole, and indeed to society in general?

In my opinion, historical archaeology is facing a crisis. Certainly this appears to be the case within Australia but I suspect that the situation is much the same in New Zealand and indeed that the problems that I wish to discuss are probably to be found also in other areas of the world where the archaeology of later literate societies is practised. To some extent the origins of this crisis lie in the character of the archaeological discipline as a whole and in the substantial and ongoing changes that have taken place within it over the last half century, but the most important of the issues relevant to my discussion are specific to historical archaeology itself. Furthermore, what I have to deal with here is not the result of some external threat that needs to be identified and neutralized but is the product of what we ourselves have been doing over the years and of the reasons we have had for doing those things. My intention in this paper is to try to identify the nature of the problems, as I see them, and to make suggestions about how they might be addressed.

A perusal of Australian publications over the last few years, and to a much lesser extent those of New Zealand, leads me to believe that I am not alone in my concern. In a brief paper such as this it is impossible to mention all of the critiques of the subject of Australian historical archaeology that have been produced in, say, the last fifteen years, but there are several that do require attention. The first of these must inevitably be the Birmingham and Jeans paper of 1983 entitled 'The Swiss Family Robinson and the archaeology of colonisations' which claimed that 'Australian historical archaeology is now at a stage of development where it is essential that we pause and ask ourselves "What are we doing and why are we doing it?"' (Birmingham & Jeans 1983:3). The paper then went on to advocate an explicit problem-oriented approach, rather than what was characterized as descriptive data collection and suggested a colonization model around which such an approach could be structured. Significantly, some readers had difficulty understanding this paper and in seeing how research projects could be constructed on the basis of the suggested model. Nevertheless, the Birmingham and Jeans paper sparked off a debate about what some saw as the theoretically impoverished state of Australian historical archaeology, a debate in which two papers by Damaris Bairstow in 1984 (Bairstow 1984a; 1984b), a paper by Tim Murray in 1985 (Murray 1985), and a paper by Murray and Allen in 1986 (Murray & Allen 1986) figured prominently.

My own concerns in those years were somewhat more pragmatic; in the same journal issue as the Birmingham and Jeans paper I published one that has since become known, to the disrespectful at least, as 'Connah on stamp collecting' (Connah 1983a). In it I identified what I saw as a dilemma facing Australian historical archaeology, on the one hand the urgent need to record rapidly vanishing data, but on the other the necessity to conduct problem-oriented research that might

actually contribute to our understanding of Australian history. This paper had originally been written in 1981 and, looking back on it, it seems to me that what I was seeing at that time was the beginning of the now-familiar tensions between archaeological scholarship on the one hand, and archaeological consulting and cultural resource management on the other; tensions that have been at least a contributory factor leading to the crisis situation that I have said now exists. Through the 1980s the fastest growing area of archaeological employment was in archaeological consulting, and it is interesting to note that by 1990 (actually published 1993) we find Judy Birmingham warning that 'isolation from mainstream disciplinary studies' was the consultant archaeologists' 'most critical occupational hazard' (Birmingham 1990:20).

It was 1994, however, before the situation that now concerns me began to be really recognized. It was left to Brian Egloff, in an insightful paper entitled 'From Swiss Family Robinson to Sir Russell Drysdale: towards changing the tone of historical archaeology in Australia', to warn that historical archaeology might 'have failed to make a sufficient intellectual contribution to issues of interest to our contemporary society' (Egloff 1994:4). 'One wonders', wrote Egloff, 'if we need yet another heap of artefacts from a pub, mill or working class neighbourhood if that material does not yield information which can be focussed upon matters that concern Australians' (Egloff 1994:3-4). Significantly, Egloff also identified two of the major weaknesses of historical archaeology that concern me in this paper: first the amount of archaeological work that is never properly published and, second, what he called 'a restricted university base' (Egloff 1994:3). At about the same time as the Egloff paper there were also indications that the historians were getting restless.

Two papers published under the date 1993, but which did not actually appear until 1995, are particularly relevant here. One by Sybil Jack commenced with the threatening sentence: 'The value of historical archaeology in Australia is currently subject to new scrutiny' and towards its end warned that: 'Eventually the public will not pay for sheer antiquarianism — what it wants is something which contributes to our understanding of how things are now' (S.M.Jack 1993:124, 128). Interestingly, this paper cited a meagre 17 sources, of which only two were drawn from Australian historical archaeology, the very subject of which it was so critical. More important was the second paper, by Ian Jack. This opened with the statement: 'The impact of history on historical archaeology is inescapable' but went on to criticise historians as well as archaeologists. Although archaeologists had 'failed to make sufficient impression on the academy', historians were 'not very aware of what is being done in Australian historical archaeology' (R.I.Jack 1993:130).

Historical archaeologists would do well to give careful

attention to both of those papers but even more so to two papers only recently published, in 1997. Both are by writers who are now self-confessed historians, but the first of these, Campbell Macknight, made one of the earliest significant contributions to Australian historical archaeology with his work on Macassan sites in the far north, and the other, John Mulvaney, has played such an important role in the development of archaeology generally in Australia that many of us will continue to think of him as primarily an archaeologist, albeit one with an unusual breadth of vision. Macknight admitted that 'in general "Australian history" as it is commonly taught includes little consideration of archaeological evidence' (Macknight 1996:9) and questioned why archaeology remained so marginal to the study of Australian history both within universities and more generally. Mulvaney, in his paper, reviewed the growth of Australian historical archaeology in the 25 years of existence of the Australasian (formerly Australian) Society for Historical Archaeology from 1970 to 1995. Commenting on the 1983 call by Birmingham and Jeans for an explicit problem-oriented approach, he made the following typically trenchant comment:

Regrettably, in my opinion, the rising pre-eminence of the environmental impact statement as employer has meant that most fieldwork remains at the "let's go and see" level. Frequently that data is contained in unpublished reports, but seldom analysed and published with rigour, because consularancies do not cover those costs. (Mulvaney 1996:4)

All of the papers that I have mentioned so far, are concerned with Australian historical archaeology. I confess that my knowledge of the equivalent New Zealand literature is sadly deficient. However, judging from a paper by Ian Smith that is dated 1991 but was actually published in 1993, there are similar problems in this country. Reviewing the development of historical archaeology in New Zealand from 1921 to 1990, he commented on 'the relatively poor publication record' (Smith 1991:10). Of all the historical archaeological excavations carried out in New Zealand, Smith found that:

While 78 percent of the excavations have been described in some published form, only 50 percent have anything like full descriptions of the artefacts and other remains that were recovered, and most of these were sites investigated within two projects of the late 1960s and early 1970s. More detailed studies of site contents such as specific artefact types, faunal remains or site features have been exceedingly rare. (Smith 1991:9)

In addition, he pointed out that:

There have been very few attempts by New Zealand historical archaeologists to present their work to the wider world. Less than four percent of publications in the field have appeared in international journals, and almost all of these derived from a single project on the archaeology of Chinese miners in Central Otago. (Smith 1991:10)

Published at the same time as Smith's paper, was one by Neville Ritchie that provided an introduction to historical archaeology in New Zealand. Significantly, he noted how by the early 1980s historical archaeology was 'increasingly funded by government agencies for management, interpretation, mitigation objectives as opposed to university-sponsored research investigations' (Ritchie 1991:3). He also identified 'a continuing need for detailed comparative studies of artefacts and faunal remains, and further material culture studies', work which he described as 'the nuts and bolts and building blocks of archaeology' that 'must proceed apace with the development of new theoretical and methodological perspectives' (Ritchie 1991:4).

Before my rather cavalier gallop through the literature, I commenced this paper by claiming that in my *opinion* historical archaeology is facing a crisis. Living as we do in such a post-modern age, I should perhaps try to identify at least the major influences that may have shaped that opinion. I will not inflict

on you a detailed analysis of my childhood and cultural background, as one politically correct speaker did at an ASHA conference a few years ago, but I should, I suppose, indicate (to use contemporary idiom) where I am coming from. Briefly, 1998 will make 50 years since my first involvement with archaeological excavation, an activity that I have pursued in Europe, Africa, and Australia and on many different types of site. My first appointment as a professional archaeologist, in a research capacity in Cambridge in 1959, was followed by ten years in Nigeria, in West Africa, where I worked at various times as a government research officer, a museum curator, a university research fellow, and a university senior lecturer. By 1971 I was in Australia, where I founded an archaeology department at the University of New England (in 1974), taught almost every sort of archaeology imaginable at one time or another, and became involved first with Australian prehistoric archaeological field research, and from the mid 1970s onwards with historical archaeological field research (Connah 1977). However, I frequently return to various parts of Africa and four of my five principal books over the years have been on aspects of African archaeology (Connah 1975; 1981; 1987; 1996), with only one being concerned with Australian historical archaeology (Connah 1988). So, you can see the situation: I am an excavator from way back, although I am generally opposed to excavation except as a last resort. I am also very much a writer, I even tolerate editing, and I was the founding editor of *Australasian Historical Archaeology*.

Clearly, with the background that I have outlined, I regard archaeology as basically a scholarly discipline. I am therefore saddened by the failure of historical archaeology to make what Ian Jack has called 'sufficient impression on the academy' (R.I. Jack 1993:130) and by what Brian Egloff has referred to as its 'restricted university base' (Egloff 1994:3). After nearly three decades of development in our two countries, there are still only a handful of academic appointments in this subject and only a few undergraduate courses being offered. In Australia, La Trobe University is the only one that has given the subject the attention it deserves, although Flinders and in the near future perhaps James Cook also deserve recognition of their efforts. Elsewhere, Sydney University, which was the main pioneer of historical archaeology in Australia, will soon have no specialist in the field due to non-replacement following retirements. The University of New England is not much better off, and the Australian National University has nobody who would claim any special competence to teach or supervise the growing number of students who want to pursue the subject at both undergraduate and postgraduate level. As for the rest of Australia's universities, there seems little or no interest, although a scattering of people involved in culture history, heritage studies, architecture, geography, and some other subjects, are active in cognate areas. I thought that perhaps things were better in New Zealand, until I was told the story of what became of an advertised lectureship in historical archaeology at one of its better known universities.

Here then, is the basis of my claim that historical archaeology is facing a crisis. For unless it can properly establish itself in university education, it seems unlikely to achieve the wide acceptance by the tax-paying public at large which is surely vital to its continuance. The questions that we must urgently ask ourselves are: how has this situation come about and what, if anything, can we do about it?

First, some blame must inevitably lie with archaeologists working in other areas of the discipline, particularly in prehistoric archaeology. They have had a tendency to dismiss historical archaeology as merely descriptive data collection, lacking in theory, and generally unnecessary. In Australia this situation has, I think, been made worse by the heavy emphasis on hunter-gatherer archaeology, which is in many ways a very specialized field, and the unhappy location of some prehistoric archaeologists in university departments dominated by social and cultural anthropologists. Because of my own background and experience,

which I have outlined, I cannot accept the notion that historical archaeology and prehistoric archaeology are different species, incapable of interbreeding. Working for so long on the archaeology of later African societies, I am no longer willing to draw a line across human time and call one part 'prehistoric' and another 'historical'. In the African context it is virtually impossible to do, and surely we must admit that in Australia and New Zealand it is sometimes singularly difficult. In short, I would like to see archaeologists take a far more open-minded view of their discipline and accept that all aspects of the human experience are worth our attention. In this matter I am apparently not alone: in a 1997 interview Lewis Binford, renowned since the 1960s as a theorist in the context of prehistoric archaeology, had the following to say:

In terms of goals, in terms of approaches, I don't think there is any difference between historical archaeology and any other kind of archaeology. The subject matter for study is the archaeological record and the patterning in the archaeological record should define our problems. (Thurman 1998:54)

In practical terms, it is increasingly necessary that archaeological appointments in both Australian and New Zealand universities become far more representative of the discipline as a whole than they are at present.

Some blame must also lie with the historians. According to Ian Jack: 'Most teachers of Australian history in the period since European settlement have had very little exposure to the use of non-documentary sources' (R.I. Jack 1993:130). That being the case, it is hardly surprising that there has been very little pressure from historians for the creation of academic posts in historical archaeology. It seems that many historians think that archaeology has nothing new to tell them that matters very much. Indeed, as I have argued more than once in print (Connah 1986:41; 1988:4-5; 1994:3, 53), it is doubtful whether the value of historical archaeology should be assessed merely on the basis of its contributions to history. Important though historical sources are, in the end our task as archaeologists should be to ask archaeological questions of archaeological data, or as Binford puts it 'look at what the patterning is in the archaeological record and use that to define our problems' (Thurman 1998:55). We are surely just as entitled to ask what history can tell us about archaeology, as historians are to ask what archaeology can tell them about history? Binford and others were making this point years ago (e.g. Binford 1983:104).

It is my belief, however, that the greatest part of the blame for historical archaeology's failure to develop properly as a scholarly discipline must lie with historical archaeologists themselves. The problem, as I see it, is that in spite of an increasing amount of archaeological activity, the body of published material in historical archaeology has remained limited in both quantity and quality, so that its intellectual impact outside the discipline has been insufficient to establish it as an area of scholarship meriting priority in academic appointments and other developments. In a 1984 editorial in the then *Australian Journal of Historical Archaeology*, I wrote of 'my conviction that a discipline consists very largely of its body of published material' and already at that time I was complaining of the increasing amount of archaeological work, particularly by consulting archaeologists, that was not getting published (Connah 1984:2). In fact, it was my concern over this situation that had led me to found the *Journal* in the first place, the year before. I felt that the provision of a quality annual journal would provide an outlet for material that might not otherwise be published (Connah 1983b:2). I was similarly concerned that the findings of historical archaeological research should reach a wider public, and it was for this reason that I wrote the book now called *The Archaeology of Australia's History*, a book originally published in 1988 but significantly still in print even if it has sold more copies in the United States than in Australia (Connah 1988). There have, of

course, been many other archaeologists as well as myself who have stressed the importance of publication. Shanks and Tilley, for instance, have emphasized the 'centrality of publication to the science of archaeology' (Shanks & Tilley 1987:17). My favourite passage on the subject, however, comes from a paper on report writing and publication that Peter White, of the University of Sydney, wrote for my 1983 edited book *Australian field archaeology: A guide to techniques*. White had the following to say about non-publication:

Research which is not available for others to use does not exist. The hard work, time and money which went into the fieldwork and analysis have been wasted. Part of our heritage from the past, along with some present resources, have been destroyed as totally and uselessly as if they had been dynamited or bulldozed over a cliff. (White 1983:171)

Don't get me wrong here, this is not merely an attack on non-publishing consulting archaeologists, although I do think that they need formal professional regulation and deserve better security of employment. There are those other than consultants who have (metaphorically speaking) unpublished skeletons in their cupboards, including myself. In fact, I have only ever known one archaeologist who could honestly claim that he had published every piece of research that he had ever undertaken, in the course of a long and varied career (Emeritus Professor Thurstan Shaw, formerly of the University of Ibadan, Nigeria). Other than him, there must be very few that are guiltless: publication takes time and serious backlogs can quickly build up. My concern here is rather with those who have actually published little or nothing at all. All of us, I am sure, can think of instances of large projects into which substantial funding and other resources have gone, without any published outcome worthy of the name.

So, what can we do to improve the situation? Immediately, I think that we all have to increase the volume of our published material. In the economic climate of the 1990s this is a publish-or-perish situation, in which historical archaeology itself is in danger of losing credibility, whatever may happen to some of us as individuals. To both stimulate research activity and to broaden the base of the available teaching literature, we need more research monographs, augmented journal outlets, and a systematic programme for publishing relevant postgraduate theses such as that provided for many years by *British Archaeological Reports*. However, we also have to take a long hard look at *what* we are publishing, bearing in mind some of the criticisms of our activities to which I have referred. We would do well, indeed, to question what our efforts are contributing to scholarship as a whole, and to see to it that our work *is* relevant to a wider audience.

Brian Egloff, for instance, has suggested that historical archaeologists are in a unique position to throw light on the interaction of both Aboriginal people and European settlers with the Australian environment and with each other, and has cited the example of work by Tim Murray in northern Tasmania in this connection (Egloff 1994:4-6; Murray 1993). It must surely be the case that the achievements and failures of the forgotten ordinary people of Australia and New Zealand, both indigenous and settler, can only be properly appreciated from the physical evidence that they have left us. In addition, the detailed environmental changes of the last two centuries, in our respective countries, are another area of high relevance to contemporary society on which we could provide new information. For example, matters such as soil erosion and vegetational change can now be investigated using the finer chronological resolution made available by ²¹⁰Pb dating, as was recently done by Gale, Haworth, and Pisanu for an eastern Australian lake basin (Gale, Haworth & Pisanu 1995; Beale 1997). These are just two examples of some of the many possibilities, for this is hardly the place to present a detailed recipe for future research.

Finally, research publication, however abundant and however widely relevant, will not be enough on its own. We need to do more than merely write esoteric papers for intellectual audiences. We must reach a wider audience, particularly those few of us who teach archaeology within the universities. In the latter case, considerable efforts need to be made to see that departments do something more than merely train further archaeologists. Archaeology, be it historical archaeology or any other sort, is just as good an educational medium as is history, geography, or philosophy, and departments teaching those subjects have long realized that society does not need to be flooded with professional historians, geographers, and philosophers. However, not only do we need to teach the subject to as broad an audience as possible, including future school teachers, politicians, shopkeepers, and many others, but we need also to reach out to the public as a whole, with popular books, with videos, with CDs, on the Internet, by participation in field projects, in every way that we can conjure up. We have to persuade our respective publics that New Zealand and Australian cultural heritage is actually the business of archaeologists and that archaeology is not just about things that are very old or far away overseas. We have to demonstrate in a public way that historical archaeology can tell us a lot more than we can learn from historical records, and can tell us different things, things that are both important and interesting. According to *The Sydney Morning Herald* for 8 September 1997, when questioned by the press about the discovery of the remains of an early nineteenth-century dockyard during building construction in Sydney, Meredith Walker remarked: 'Archaeology is not just for archaeologists, and if it's worthwhile digging it's worthwhile us seeing that the information discovered is a public matter' (O'Brien 1997).

Note: This is basically the text of a paper read at the Annual Conference of the Australasian Society for Historical Archaeology, held at Queenstown, New Zealand, 3–6 October 1997. Its length was determined by the limit of twenty minutes allowed for each paper. The only changes made in this published form are some minor alterations to the text and the addition of references and a bibliography. The author wishes to thank Neville Ritchie and two unidentified referees for their helpful comments, which were of material assistance in the revision of the paper.

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