But that was long ago: theory in Australian historical archaeology 2002

TIM MURRAY

This paper reviews aspects of the role that theory plays in practice of Australian historical archaeology, and compares this with the situation as described 16 years ago. While some notable progress has been made in some areas shortcomings previously noted have continued, notwithstanding the predominance of the heritage over academic context of practice. The paper argues that the most effective way to deal with these shortcomings is to support a model of practice that develops serious collaborations between archaeologists working in either context. An important part of this model is a much more serious and intensive focus on the value of research, both through the conduct of assemblage-based analyses and through the critical evaluation of research designs (and the theories that implicitly and explicitly drive them) at the completion of specific projects.

It is now 16 years since Jim Allen and I discussed the role of theory in Australian historical archaeology (Murray & Allen 1986). Much has happened since then. The teaching of historical archaeology in academic departments has waxed and waned, the number of PhD dissertations in the field has increased to over 20 with at least another 8 in the pipeline, and there has been a steady increase in the scale and significance of heritage archaeology. We know a great deal more than we did about the archaeology of early Australian cities, of the 'contact' period, and of course about the archaeology of primary and secondary industries. We have also been exposed to new sources of theory, new areas for investigation (such as material culture, gender, ethnicity, class, and colonialism), and new frameworks of interaction with traditionally cognate disciplines. Altogether there has been a great deal more historical archaeology done.

A few years ago the occasion of Allen's festschrift (Anderson & Murray 2000) gave me a chance to reflect on what I felt was then the current state of historical archaeology in Australia, if only to continue publicly a discussion that Jim and I have long been having about the relative strengths and shortcomings of prehistoric and historical archaeology. In this reflection I was aided by Egloff's (1994) account of the history of the field, overviews made by long-standing members of the profession (Connah 1998a; S. Jack 1993; R. I. Jack 1993; Mulvaney 1996) and the publications of my colleagues Susan Lawrence (1998, 2000) and Jane Lydon (1999), and others (see e.g. Karskens 1997, 1999). I was also responding to the lessons I had learned as a result of a fruitful collaboration with Alan Mayne in the field of urban archaeology (see Murray & Mayne 2001, in press), and the research I had done to underwrite an application to the Australian Research Council to support the Exploring the Archaeology of the Modern City (EAMC) project (Murray 2001). At that time I gave a pretty upbeat assessment of the strength and potential of the field:

> In the decade or so which has passed since Murray and Allen (1986) called for a more theoretically-engaged and creative Australian historical archaeology the field has shrugged-off its obsession with themes and checklists and developed an agenda which goes beyond the specific needs of archaeological heritage management. (Murray 2000: 146)

I have been thinking about these matters ever since, in an effort to further develop that agenda. Apart from the new experiences and opportunities offered by the building of a for-

mal collaboration between La Trobe Archaeology and the heritage consultancy Godden Mackay Logan, my recent thinking has been strongly influenced by participating in four projects. First, the analysis of 'Little Lon' (see e.g. Mayne, Murray & Lawrence 2000; Murray & Mayne 2001, in press); second, the work undertaken by Penny Crook, Laila Ellmoos and myself in the analysis of the Hyde Park Barracks, the Mint, First Government House, Paddy's Market, Susannah Place, Cumberland Gloucester Street and Lilyvale sites as part of EAMC project; third, the Parramatta Historical Archaeological Landscape Management Study (PHALMS; Godden Mackay Logan 2000), and finally the first phase of the Casselden Place project in Melbourne, for which Alan Mayne and I wrote the research design (Murray & Mayne 2002).

I have come to the conclusion that, notwithstanding the great progress made over the past 16 years, historical archaeology in Australia has reached another crossroads. In this paper I will explain why I feel this to be the case and what I believe we will need to do to progress our field to the point where it can usefully contribute to Australian (and global) history. I will do this by first very briefly recapping some of the essential arguments of our original discussion. I will then provide a short overview of contemporary discussions about theory and disciplinary identity in Australian historical archaeology. This exposition will be followed by a slightly longer consideration of the role of research designs in contemporary heritage archaeology. Throughout both discussions, 'theory' is seen to encompass both explicit statements of the kind, and theoretical influences implicit in excavation reports or the final products of other research projects. The paper will conclude with a sketch of a new, and what I believe will be a more effective, model of research practice that might shape activity in Australian historical archaeology over the next decade.

BUT THAT WAS LONG AGO

Allen and I devoted the bulk of our 1986 survey to a discussion of the things practitioners had to do in order to unlock the potential of the field to provide a unique and important perspective on the history of Australian society. Of course we accepted that practitioners had been able to provide supplementary illustrative material to a range of historical disciplines, but we argued that this hardly exhausted the interpretative and explanatory possibilities of the field. Much of our paper sought to relate the lack of theoretical development, and the concomitant interpretative and explanatory limitations of the field, to

the domination of the field by the needs of heritage management:

> ...the value of archaeology as a discipline, and the value of the material remains of past human action from the point of view of the cultural preservationist, have been inextricably linked for over a century, not just the last twenty years in Australia. (1986: 85–86)

Although our paper has often been cited, detailed analysis of our arguments had to wait for Ireland (2001) who summarised things as follows:

> What concerned Murray and Allen was that because the conservation ethic of the heritage movement had become the central theoretical underpinning of historical archaeology, this had lead to the development of methodologies designed to *enable* preservation, but not to *explain* why it was necessary. The latter would be an explicit statement of the research potential of historical archaeological evidence which of course should lead to the realisation of this potential in the form of new knowledge. (Ireland 2001: 194–195)

Although Allen and I did devote some attention to the link between preservationism and the shape of Australian historical archaeology, our primary goal was to argue for the benefits of a link between the development of a distinctive archaeological research agenda, and the building of theories that would clearly establish the significance of the knowledge that could be created. We put this clearly:

> We must recognise and demonstrate this distinctiveness in its own terms. Our basic data are material results of human action and natural site formation processes—which we deal with by using archaeological methods to cluster data into archaeological entities and which we then interpret in a framework archaeological in its perceptions as well as its procedures (Clarke 1968: 13). (Murray & Allen 1986: 86)

We explicitly contrasted this approach to the uncritical use of themes and checklists in heritage archaeology. Naturally we understood their great value as a quasi-explicit means of assigning value and significance to sites and specific contexts, but we also understood that without a strong commitment to archaeological research and the building of archaeological theory, the archaeological value of such instruments was largely chimerical:

> The lack of formal theoretical development of the field has meant that the interpretation and explanation of sites and contexts has tended to stop with the assignment of a theme or congeries of themes. Meaning has continued to reside in categories that are as yet theoretically unarticulated by archaeologists. As a consequence, few anomalies have been detected between the categories and our experience of classifying the archaeological remains in their terms. The categories of Australian historical archaeology have remained essentially inviolate—predefined, uninvestigated, and privileged sources of archaeological interpretation. (Murray & Allen 1986: 87)

But we also identified a deeper problem, which was the equation of themes and checklists with theory:

> It is our view that the organizing power of the theme and checklist has been uncritically accepted by historical archaeologists as the theoretical base for the field—not simply as a substitute for theory building, but as theory itself. (Murray & Allen 1986: 88)

In 1986 we were convinced that this (seemingly unwitting) process of substitution privileged the perspectives of historians and human geographers, as the themes and checklists currently being used expressed primarily their interests.

Accordingly, we argued that notwithstanding the value of theories and approaches drawn from disciplines cognate to archaeology, they could not and must not comprise the totality of archaeology's theoretical armory.

Much of our discussion was based on our view that for historical archaeology to grow and prosper it needed to develop 'both a body of skills and congeries of problems that enhance the importance of studying the material culture of historic periods' (Murray & Allen 1986: 92). We argued that this development would require us to demonstrate the significance of research in the field for the:

> ...development of historical archaeology and the preservation of its data base depends on the expansion of the interests and activities of its practitioners, the building and development of theory, and through these the demonstration of archaeological problems and methodologies. (Murray & Allen 1986: 92)

I now turn to a brief overview of theory and discussions of disciplinary identity in Australian historical archaeology since 1986.

NOT DEAD, JUST SLEEPING?

Several of the shortcomings identified by Allen and myself have been more or less dealt with since 1986. For example, the evolution of *Australasian Historical Archaeology*, in no small measure due to the efforts of practitioners such as Graham Connah, has provided a much-need venue for refereed publication in the field. The Australasian Society for Historical Archaeology has also played a useful role in promoting contact between far-flung communities of practitioners, and in building international connections (particularly in the USA and the UK).

Although growth in the academic side of historical archaeology has hardly been stellar, when looked at in the context of the generally straitened circumstances of archaeology in Australian universities over the same period, the fact that there has been some expansion rather than the general story of decline makes for positive news. Perhaps a more important development has been the very steady rise in student enrolment at both undergraduate and postgraduate levels, and the sharp increase in the number of doctoral dissertations written on a diversity of topics ranging from 'contact' archaeology through to landscape studies. When this vital research is added to major Australian Research Council funded projects carried out on the mainland and in Tasmania (see for example the Lake Innes House [Connah 1998b, 2001], AWSANZ [Lawrence and Staniforth 1998], Van Diemen's Land Company [Murray 1988, 1993, 2000], 'Little Lon' [Mayne, Murray & Lawrence 2000; Murray & Mayne 2001, in press] and EAMC [Murray 2001]), as well as other ongoing research in urban and rural Australia, we can detect a significant increase in academic research after 1986.

This impression is supported by three recent surveys of theory in Australian historical archaeology by Godden Mackay Logan (2000: 13–28), Paterson and Wilson (2000), and Ireland (2001). While I have deep reservations about the statistics that support the latter two analyses (primarily based on disagreements about what constitutes 'theory' and their categorisation of the various domains of theorising), both surveys point to a broadened and deepened engagement of Australian historical archaeologists in this aspect of practice. Australian research into the archaeology of households (Allison 1998), ideology (Burke 1999), consumption (Crook 2000), material culture (Lawrence 1998) and ethnicity (Lydon 1999) has expanded work in contact (see e.g. Murray 2002) and urban archaeology to underpin a solid contribution to global historical archaeology (see Lawrence & Karskens in press).

However pleasing these developments may be, they come off a very low base, and their capacity to act as exemplars for the field as a whole is constrained by continuing problems with the building and articulation of theory, and with conflict between the academic and heritage aspects of disciplinary identity. Although there is a great deal more historical archaeology being done, the simple fact (acknowledged by everyone in the field) is that by far the bulk of that activity occurs within the heritage sector. This has been, of course, excellent news for recent graduates seeking employment, and it surely must be a contributing factor to the growth in student numbers. It is not my purpose in this paper to investigate the vagaries of student demand or the peculiarities of academic archaeology, but it worth observing that the centre of gravity in Australian historical archaeology decisively shifted to the heritage sector at least a decade ago.

Certainly Murray and Allen (1986) now reads with a slightly anachronistic air, based as it was on the premise that research is what academic archaeologists do, while heritage archaeologists apply and extend it. The notion that such a rigid division now need not reflect the reality of practice is made manifest by the fact that mainstream academic publications such as that by Mayne and Murray (2001) on the archaeology of urban landscapes, and the research collected by Lawrence and Karskens (in press) comprise much work that had its origins in heritage archaeology. This is further supported by the rise of Australian Research Council Linkage Scheme (formerly SPIRT), research that has funded large industry collaborations such as AWSANZ and EAMC, as well as increasing numbers of postgraduate scholarships for industry-based projects.

Assessing the impact of this decisive shift has been made easier by a recent frank exchange between Graham Connah (1998) and Mackay and Karskens (1999) over the nature of publication in the field. For Connah archaeology is in a state of crisis largely caused by the tension between 'archaeological scholarship on the one hand, and archaeological consulting and cultural resource management on the other' (1998: 3). Noting previous arguments that research in Australian historical archaeology was not having much impact outside of the narrow confines of the field (especially among Australian historians) Connah felt sure that he knew where the blame lay:

> 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. (1998: 5)

Mackay and Karskens (1999) firmly disagreed with just about everything Connah had to say. Instead of crisis they saw Australian historical archaeology as 'entering a most exciting and fruitful phase' (1999: 110). The fact that things might be crook in the academic side of the field did not apply to the rest of historical archaeology. 'The reality is that, now and in the foreseeable future, consultancy work in historical archaeology will receive great resources and generate more new research than its academic counterpart'(1999: 110). Furthermore, Mackay and Karskens confidently asserted that the 'future of academic historical archaeology may well lie in large part with cultural resources management' (1999: 110–111).

Connah's argument that the vast (and still increasing) amount of grey literature created by heritage archaeology was inhibiting the capacity of the field to grow and prosper was rejected on two counts. First, the bulk of the papers published in the Society's journal were sourced to heritage projects and written by heritage practitioners, and second, as has often been observed, many heritage reports are too restricted in scope to warrant publication in mainstream journals. Mackay and Karskens were also able to point to the substantial publication that had arisen from the Cumberland and Gloucester Street excavations in The Rocks (see Godden Mackay Logan & Karskens 1999, 2000). The Rocks excavations also bolstered an argument that if Australian historical archaeology was to be taken seriously by say, historians, then the field had to:

> …address important issues and questions, and it has to approach and express them in accessible language. If publication alone were to be the measure of the discipline, it is the quality not the quantity of the material which should be the focus. (Mackay & Karskens 1999: 111)

But their rebuttal of Connah's thesis does not stop there. In their view historical archaeologists have to stop speaking just to themselves, to embrace a wider pool of ideas and perspectives:

> To provide material of quality, that is of importance and relevance, historical archaeology must move outside itself, away from the intensive search for itself, and look outwards. Not everyone is riveted by the 'difficulties facing archaeologists'; not every book or article should be focused on them. In some quarters historical archaeology does have a tendency to be self-absorbed, to ignore and shut out wider perspectives and ideas, in a mistaken idea of 'scholarly rigour', or a vague notion of 'developing the discipline'. Scholarly rigour is, of course, essential, but if confused with close-mindedness, and the obsessive patrolling of disciplinary 'boundaries', the effect is to paralyse the whole enterprise. (1999: 111–112)

It is obviously crucial for historical archaeology to engage with other disciplines and mainstream heritage issues of establishing and communicating the meanings and values of archaeological information. But the argument made by Allen and myself 16 years ago still holds true, in the sense that the interests of historical archaeologists are not entirely coincident with those of heritage practitioners, nor with the interests of practitioners of traditionally cognate disciplines. Notwithstanding the very real benefits that can flow to historical archaeology from engaging with a broader range of historical questions and perspectives, we need to do this as archaeologists.

For this reason alone it is worth observing that asking great questions is one thing, but providing convincing archaeological answers to them is quite another. That must be our fundamental concern, because no matter how much interest is shown in our data and perspectives by historians and others (or indeed no matter how much interest we might show in theirs), our fundamental responsibility is to make sense of those data by means of archaeological theories and archaeological methodologies answering archaeological questions. Active discussion of, and research into, the nature of those theories and methodologies are at least as significant for our field as debates about the sources of questions or interpretative perspectives. Indeed the experience of working closely with an urban historian during the analysis of 'Little Lon' clarified for me the tremendous intellectual challenges posed by integrating the diverse data sets of historical archaeology. Both Mayne and I sought to produce something that was greater than the sum of the different data sets, developing an account of 'Little Lon' where the archaeological data did more than supplement a master historical narrative derived from the analysis of written documents. As we have made clear (Murray & Mayne 2001: 102–104) this is very much a work in progress, which is being pursued in the EAMC project and in our continuing involvement with Casselden Place.

By the same token it could well be that the cautions of archaeologists about issues of data quality and levels of analy-

sis might be seen by heritage managers as impeding the business of successfully communicating the significance of sites or contexts to others. If that point is reached then archaeologists have to embrace the notion that they are not the only players in the heritage marketplace and that no one has absolute control over interpretation. This is all the more important when we recognise, as Mackay and Karskens do, that the well-being of our profession is connected to its capacity to persuade others of its relevance, as well as its capacity to do quality archaeology. But this does not by any means imply that the vagaries of that marketplace should set the standards of research in historical archaeology. Speaking of the methodology employed at the Cumberland Gloucester Street site where research design and a hierarchy of questions were said to be critical elements in developing interpretation they observed:

> We must continue to devise 'mid-range' questions and approaches—practical, workable ways to relate the small things to the large enquiries. The magic lies in the engagement, the dialogue, the shifting between the sources, the moment when one source or idea throws new light upon another, shows it in a startlingly new perspective. (1999: 113)

Australian historical archaeology has been particularly well served by the publication program associated with the Cumberland and Gloucester Street site. Those involved summarised their achievement clearly and directly:

> While on the surface this project simply 'met' the basic requirements of historical archaeology in Australia—informing others of the results of important work, in fact it far exceeded industry practice. Only a handful of industry reports have been published in Australia and none offer the complete set of artefact-analysts' reports. Only one other popular book and one education kit about an archaeological site are available. The results of many excavations do not even reach historical or archaeological journals. To date, the Big Dig is the only one with a long-tern commitment to the provision of public 'deliverables'—that is, telling the story, not just digging the dirt. (Godden Mackay Logan & Karskens 2000: 4)

As a result of this publication program we have far more detail on the process of excavation, research and interpretation at Cumberland and Gloucester Street than for any other heritage project in Australia. Thus we can observe more closely the creative process described by Mackay and Karskens and draw conclusions about the nature and extent of articulation and integration between catalogued, but unanalysed archaeological data, and the various issues that excavation was designed to explore. A detailed consideration of these matters is outside the scope of this paper, but the model of theory building outlined by Mackay and Karskens underscores how vital it is that the theories and perspectives articulated in research design be evaluated against properly analysed empirical data, especially if we are going to be able to serve either academic or heritage interests.

Thus reaching new audiences and persuading them of the value of historical archaeology is not just a matter of adding new interpretative and explanatory possibilities—it is also about making them work. This task is made all the more difficult in Australian historical archaeology when we have so few analysed sites and some real questions about data quality in existing catalogues and databases which are, together, such important foundations for assemblage-based analyses. Crook, Lawrence and Gibbs (2002) usefully canvass many of the issues related to databases but for our present purpose we need to be clear that doing research in historical archaeology will require high standards in the production of catalogues, and the establishment of transparent guidelines to guarantee minimum standards in both academic and heritage contexts. The brave new dawn Mackay and Karskens point to is, therefore, much more than spin, but it poses significant challenges to us.

RESEARCH AND THEORY IN CONTEMPORARY HISTORICAL ARCHAEOLOGY

The overt discussion of the role of research design in the practice of archaeology has spawned a vast, diffuse, and often contradictory literature. Flowing from the principle that the questions asked of a site or assemblage should relate meaningfully to methods of data recovery and analysis (mediated by methodology, or 'middle-range theory') it has been generally agreed that good research design is the key to resolving several significant tensions that inevitably arise from the archaeological process.

Chief among these tensions is the notion that excavation (still a fundamental means by which archaeologists recover information) involves the destruction of sites and contexts. Of course it is true that much archaeological research can be (and is being) done from the desktop, in the laboratory, or through the use of non-invasive and non-destructive technologies. Nonetheless the characterisation of the archaeological record as a 'non-renewable resource' that underwrote much early work on research design in the 1960s and 1970s, is still accepted in both the academic and heritage management spheres of contemporary archaeological practice.

Whatever the sphere of practice, two fundamental arguments logically follow from this acceptance. First, that destruction of significant sites or features should only be sanctioned if it can be demonstrated that an excavation will materially contribute to our knowledge of the site and its social and cultural context however broadly or narrowly defined. Second, that formal means of demonstration need to be established, whether these be broadly accepted standards of data recovery, recording and analysis, or through clear methodological links between data and the construction of knowledge about the place (middle-range theory). Naturally these two arguments, which together encapsulate the significance of research design, have different impacts depending upon the context of excavation.

In the case of pure academic research (which represents a minute percentage of excavation events throughout most western countries) the task of raising funds and gaining permission from heritage managers is very difficult. Raising funds for research projects requires competition with other projects and other investigators drawn from disciplines across the Humanities and Social Sciences. Evaluation of projects is extremely stringent and geared to the requirement that such research (and the researchers who undertake it) is of international quality, and that the research creates new knowledge. In recent years the major source of funds in Australia (The Australian Research Council) has only been able to fund approximately 24% of all applications. The success rate for historical archaeology falls roughly within this average. Typically research projects might last for a minimum of three years, many a great deal longer. Notwithstanding issues related to funding, the approval process adopted by heritage managers has to balance the benefits that might flow to society from the project against the fact that the sites to be excavated are usually not under threat of development.

Excavation in a heritage management context has obvious differences. Funding is supplied by developers, the quantum being established through negotiation between the developer, the archaeologist and sometimes the responsible management authority. The purpose of the excavation is to salvage information from a site that is (in the bulk of cases) to be destroyed as a result of development. The selection of the site, the time

frame of the work, and indeed the kinds of archaeological activities to be undertaken (excavation, cataloguing, analysis and reporting) are ultimately either determined or constrained by others. Of course there are ethical and professional standards that apply to the work of archaeologists in the heritage context (as there are in the academic situation). It is widely appreciated that the simple retrieval of information does not present the best outcome for a society that has to trade off heritage against the benefits of development.

Godden Mackay Logan Pty Ltd in a recent landmark report *Parramatta Historical Archaeological Landscape Management Study* (PHALMS) (2000) summarized this situation well:

> ...most Australian archaeologists—historical and prehistorical alike—recognise that the research design is a useful tool in the best-practice recovery and interpretation of archaeology in Australia. It is a tool for ensuring that, as the record is destroyed, the data retrieved will make a meaningful contribution to understanding the past. The best place to start is with questions that make a meaningful contribution to current and relevant research topics. (2001: 20)

The PHALMS approach, with its emphasis on establishing research frameworks that integrate a diversity of historical disciplines while addressing questions that range from the purely local to regional, national and international contexts, provides a model for creating research designs. But creating research designs is one thing, evaluating them is quite another.

In the academic context research designs are evaluated in the grant application process and through the publication of peer-reviewed research where the growth of knowledge is said to be fostered through criticism and fearless analysis. Despite some scepticism there is no doubt that such evaluations actually occur. However a close analysis of the discourse of contemporary archaeological theory demonstrates that this process of formal evaluation through the publication and open discussion of research findings is happening with much less frequency. There are many reasons for this, but perhaps the one of greatest concern is a view (now quite popular among adherents to a variety of relativist epistemologies) that the value of theories and perspectives should not be judged on their capacity to be verified or even assessed with respect to a body of empirical data. Thus an argument that a certain distribution of material culture is indicative of a certain social process or structuring principle might well persist in certain parts of the profession even when it might be logically invalid or not supported by empirical data. The impact of relativism has exacerbated a declining commitment to intense analyses of artefact assemblages in academic archaeology, because the high cost and long time-frame of such analyses has been thought by some to decrease chances of success with applications to grant bodies.

These are matters of serious concern in academic circles, but they flow through into the realm of archaeological heritage management as well. Here research designs are rarely, if ever, evaluated after completion of archaeological investigations because the capacity for empirical evaluation frequently does not exist. Of course this does not mean that archaeologists working in this context lack the skill or the knowledge to do so. What they lack is money and time. In practice the very large sums of money detailed analyses of major urban assemblages require are rarely available, and heritage managers appear to be content with a report and a catalogue that may or may not have its quality assessed. Neither of these deliverables is in any way a sufficient basis on which to evaluate the quality of research designs (nor the theories that are their foundation), nor are they, particularly in the case of large and complex sites, a sufficient mitigation of the impact of development.

On this basis it would not be too great an exaggeration to contend that in archaeological heritage management the purpose of research design is to sometimes create a space where sites can be excavated and data gathered (often called 'research' in heritage archaeology) in a way that leaves the theoretical underpinnings of the research untouched by an encounter with the empirical. Allied with this is the often implicit (but occasionally explicit) belief that 'future researchers' can undertake the task of analysis and, by extension, evaluation. The fact that if (by some miracle) this were to happen then the costs of such research are not borne by those who made money from the developments, or by those agencies who seem to insist that good heritage archaeology must take place within a research framework. Leaving aside work undertaken by self-funded students, the costs will have to be borne by the Australian tax payer, who might be rightly forgiven for asking can quality research be undertaken in the absence of analysis and explicit systems of evaluation. Unfortunately the brutal truth is that such comprehensive analyses are highly unlikely to occur due to an absence of funds and an absence of will.

Thus the increasing problems noted with respect to the abstractions of theoretical archaeology apply with even greater force in archaeological heritage management. We are now used to research designs asking questions that reflect what is thought to be the cutting-edge of theoretical archaeology. Close analysis of those research designs demonstrates that in many cases little or no chance of answering such questions exist, either because the appropriate methodologies are absent or they are so contested. Recalling Mackay and Karskens' point about the domination of research in Australia by heritage archaeologists, it would seem that our capacity to build theory through the evaluation of its use in archaeological research designs is being seriously compromised. It is also worth recalling that a significant element of the PHALMS approach, mandated by contemporary heritage practice in NSW, is the use of themes and checklists that show few signs of any archaeological elaboration or testing in the 16 years since Allen and I discussed their manifest shortcomings (see also Ireland 2001: 201–203). The PHALMS model explicitly incorporates a review and assessment phase for these elements of research design too, but its implementation and management is a matter for local councils and the relevant heritage authority.

This is an entirely unhealthy situation. While documents such as the PHALMS report espouse the virtues of research design and regional research frameworks, and even innovate by developing structures that build evaluation into the archaeological process, the funding and management of such evaluations is still an open question. Collaborative research between academic and heritage management archaeologists of the kind that is happening with increasing frequency is one way of demonstrating the benefits that might flow from using detailed archaeological and historical analysis to close the circle from research design, excavation, and the production of a catalogue. This can only be useful, but in the end the commitment to quality heritage archaeology must lead us to develop funding and management strategies that will address these very serious issues. This is the crossroads that we have arrived at in 2002, where the matter is not simply about theory or research design, but about the capacity of either academic or heritage archaeology to fulfil the potential of historical archaeology to make its contribution to the history of Australia in the modern world.

MOVING FORWARD

After having identified some, but hardly all, of the possibilities and pitfalls of contemporary practice, it remains for me to sketch an alternative model that might assist us in doing better.

I have mentioned in the brief discussion of the exchange between Connah and Mackay and Karskens that drawing rigid distinctions between the academic and heritage sectors is unnecessary and potentially harmful. It also does not reflect contemporary practice where archaeologists are increasingly working in both the academic and heritage through formal collaborations such as at Camp Street, Ballarat (Murray et al. 2000). Although I concur with Connah's basic argument that sound publication is essential for the well-being of the field, I also think that such publication should release the potential of the field through quality research. The fact that so much of what passes for research in our field is conducted in heritage contexts makes it absolutely vital that models of practice be adopted that help us all to derive the maximum benefit from such work.

In the previous section I argued that existing models of practice were inimical to this process. Although standards obviously differ from practitioner to practitioner, and from agency to agency, it seems fair to generalise and claim that standards of practice and evaluation should be subject to continuous improvement and assessment. I have already discussed the importance of a regular evaluation of consultancy reports and the research designs that power them. In the academic context this occurs through the grant and publication process, but the extensive (and ongoing) publication of work at Cumberland and Gloucester Street provides an example of how similar strategies can be employed in heritage archaeology.

It seems to me that we need to work on several fronts simultaneously, and that the heritage bureaucracies and the key professional associations should lead the reform process. As Crook et al. (2002) have argued, we need to look very closely at the role of catalogues in the conduct of assemblage-based analyses, and embrace the notion that such analyses are the bedrock on which the questions posed by research designs and the theoretical frameworks they articulate, can be evaluated. Recent experience with the EAMC project has made it absolutely clear that heritage bureaucracies need to pay much closer attention to the quality of the outcomes that are being achieved in heritage archaeology, whether they be databases, site archives, or the curation of assemblages. Certainly the capacity of future archaeologists to undertake research on existing sites and collections is critically dependent on getting these matters right (or improving the quality of what is there).

Obviously these reforms must have the support of heritage practitioners, and the best way to gain this is through consultation and open discussion of the current situation. Of course these reforms will cost money, either because the complexity of catalogues will increase, analyses will have to be more directly linked to answering questions posed by research designs, reports and research proposals will need to be more stringently evaluated, or storage of site assemblages and site archives will have to be closely monitored by heritage bureaucracies. The need for funding, and concomitant changes in modes of practice, will provide all of us (not just the heritage bureaucracies) with some challenges, but I think that creative solutions will spring from informed and open discussion. Certainly we can point to the London Archaeological Archive and Research Centre (LAARC) recently established by the Museum of London as a benchmark in this element of our practice (www.museum-london.org.uk/laarc/new/infomenu. asp).

Of course we can choose to ignore the current situation and to allow the problems that I have identified to persist. But while this might serve to make some of our lives easier in the short-term, it will eventually lead to a degradation in the value of our discipline, to say nothing of the archaeological resources we are ethically bound to enhance and protect.

ACKNOWLEDGEMENTS

BIBLIOGRAPHY

ALLISON, P. M. 1998. 'The Household in Historical Archaeology', *Australasian Historical Archaeology*, 16: 16–29.

ANDERSON, A. and T. MURRAY (eds) 2000. *Australian archaeologist: Collected papers in honour of Jim Allen*, Coombs Academic Publishing, Canberra.

BURKE, H. 1999. *Meaning and ideology in historical archaeology: style, social identity and capitalism in an Australian town*, Kluwer Academic/Plenum Publishers, New York.

CONNAH, G. 1998a. 'Pattern and purpose in historical archaeology', *Australasian Historical Archaeology*, 16: 3–7.

CONNAH, G. 1998. 'The archaeology of frustrated ambition: an Australian case study', *Historical Archaeology*, 32 (2): 7–27.

CONNAH, G. 2001. 'The Lake Innes Estate: privilege and servitude in nineteenth-century Australia', *World Archaeology*, 33 (1): 137–154.

CROOK, P. 2000. 'Shopping and Historical Archaeology: Exploring the contexts of urban consumption', *Australasian Historical Archaeology*, 18: 17–28.

CROOK, P., S. Lawrence and M. Gibbs 2002. 'The role of artefact catalogues in Australian Historical archaeology: frameworks for discussion', *Australasian Historical Archaeology*, 20.

EGLOFF, B. 1994. 'From Swiss Family Robinson to Sir Russell Drysdale: Towards changing the tone of historical archaeology in Australia', *Australian Archaeology*, 39: 1–9.

GODDEN MACKAY LOGAN 2000. *Parramatta Historical Archaeological Landscape Management Study*, Report prepared for the New South Wales Heritage Office, Sydney.

GODDEN MACKAY HERITAGE CONSULTANTS and G. KARSKENS 1999. *The Cumberland/Gloucester Streets Site, The Rocks: Archaeological Investigation Report*, Volumes 1–6, prepared for the Sydney Cove Authority, Godden Mackay Logan Pty Ltd, Sydney.

GODDEN MACKAY LOGAN AND G. KARSKENS 2000. Entry to the National Trust Energy Australia Heritage Awards, Godden Mackay Logan, Sydney

IRELAND, T. 2001. An Artefact of Nation: Historical Archaeology, Heritage and Nationalism in Australia, unpublished PhD dissertation, Department of Prehistoric and Historical Archaeology, University of Sydney.

JACK, R. I. 1993. 'Historical archaeology and the historian', *Australasian Historical Archaeology*, 11: 130–138.

JACK, S. 1993. 'Divorce or reconciliation: history and historical archaeology', *Australasian Historical Archaeology*, 11: 124–129.

KARSKENS, G. 1997. *The Rocks: Life in Early Sydney*, Melbourne University Press, Melbourne.

KARSKENS, G. 1999. *Inside the Rocks: the Archaeology of a Neighbourhood*, Hale and Iremonger, Sydney.

LAWRENCE, S. 1998. 'The Role of Material Culture in Australian Archaeology', *Australasian Historical Archaeology*, 16: 8–15.

LAWRENCE, S. 2000. *Dolly's Creek: an Archaeology of the Victorian Goldfields Community*, Melbourne University Press.

LAWRENCE, S. and G. KARSKENS (eds) in press. Recent work in historical archaeology in Australia and New Zealand, *Historical Archaeology*, 37. Society for Historical Archaeology.

LYDON, J. 1999. *Many Inventions: The Chinese in the Rocks 1890–1930*, Monash Publications in History, Melbourne.

MACKAY, R. and G. KARSKENS 1999. 'Historical archaeology in Australia: Historical or hysterical? Crisis or creative awakening?', *Australasian Historical Archaeology*, 17: 110–115.

MAYNE, A. and T. MURRAY (eds) 2001. *The Archaeology of Urban Landscapes: Explorations in Slumland*, Cambridge University Press, Cambridge.

MAYNE, A., T. MURRAY and S. LAWRENCE 2000. 'Historic Sites: Melbourne's "Little Lon"', *Australian Historical Studies*, 31 (114): 131–151.

MULVANEY, D. J. 1996. 'Musing midst the ruins...', *Australasian Historical Archaeology*, 14: 3–8.

MURRAY, T. 1998. 'The Van Diemen's Land Company and the Archaeology of 19th century Private Colonisations', in J. Birmingham, D. Bairstow and A. Wilson (eds), *Archaeology and colonisation: Australia in the World Context*, Australian Society for Historical Archaeology, Sydney, pp. 99–108.

MURRAY, T. 1993. 'The Childhood of William Lanne: Contact Archaeology and Aboriginality in Northwest Tasmania', *Antiquity*, 67: 504–519.

MURRAY, T. 1996. 'Contact archaeology: Shared histories? Shared identities?', in *Sites—Nailing the debate: interpretation in Museums*, Historic Houses Trust of NSW, Sydney, pp. 199–213.

MURRAY, T. 2000. 'Digging with documents: Understanding intention and outcome in northwest Tasmania 1825–1835', in A. Anderson and T. Murray (eds) *Australian Archaeologist: Collected papers in honour of Jim Allen*, Coombs Academic Publishing, Canberra, pp. 145–160.

MURRAY, T. 2001. 'The Archaeology of the Modern City (AMC): Update of Progress', *Australasian Society for Historical Archaeology Newsletter*, 31 (4): 8.

MURRAY, T. 2002. 'Epilogue: An Archaeology of Indigenous/Non-Indigenous Australia from 1788', in R. Harrison and C. Williamson (eds) *After Captain Cook*, Sydney University Archaeological Methods Series 8, Sydney, pp. 213–223.

MURRAY, T. and J. ALLEN 1986. 'Theory and the development of historical archaeology in Australia', *Archaeology in Oceania*, 21 (1): 85–93.

MURRAY, T., K. HOLMES, A. JONES, M. KELLY and R. MACKAY 2000. 'Historical Archaeology at Camp Street, Ballarat, Victoria', *Bulletin of the Australian Historical Association*, 91: 83–85.

MURRAY, T. and A. MAYNE 2001. 'Imaginary Landscapes: Reading Melbourne's "Little Lon"', in A. Mayne and T. Murray (eds), *The Archaeology of Urban Landscapes: Explorations in Slumland*, Cambridge University Press, Cambridge, pp. 89–105.

MURRAY, T. and A. MAYNE 2002. Casselden Place Development, Phases 1 and 2: Research Design, unpublished report prepared for the Industry Superannuation Property Trust, Melbourne.

MURRAY, T. and A. MAYNE in press. '(Re)Constructing a Lost Community: Little Lon: Melbourne, Australia', *Historical Archaeology* 37 (1): 87–101.

PATTERSON, A. and A. WILSON 2000. 'Australian historical archaeology: Retrospects and prospects', *Australian Archaeology*, 50: 81–89.