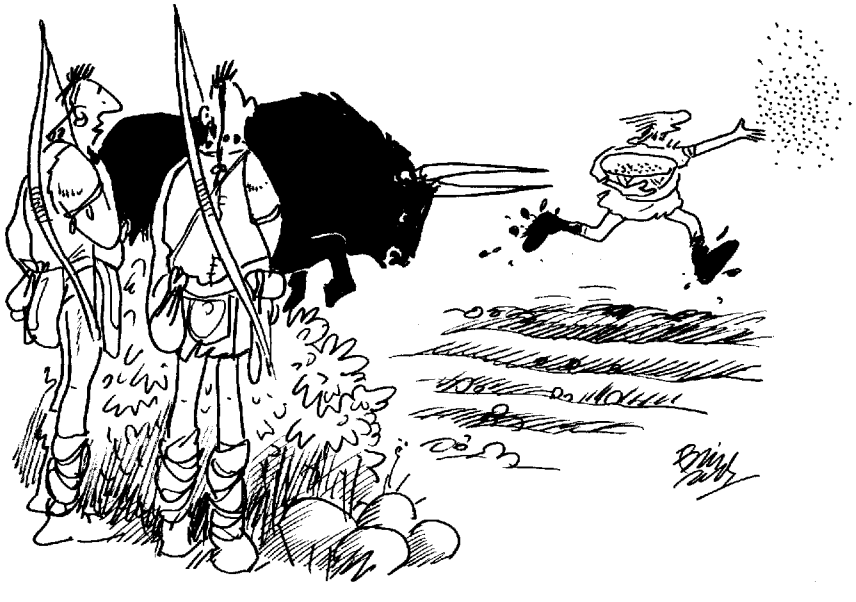
Editorial

SIMON STODDART & CAROLINE MALONE

In our antepenultimate editorial — more on our successor below — we have chosen to work our way further into the past and focus on a second key global transition investigated by archaeology: agriculture. The transition of state formation — our previous theme — is generally dependent on the increased production of agriculture, and this in itself speaks of its importance. Our invitation to contribute comments has developed its own momentum, demonstrating the pace and diversity of work. Contributors were given a free rein, in terms of opinion, content and level of formality (with or without references). The only constraint was provided by space to address the issue of agricultural origins, transitions and development. Agriculture and farming are themes that have received much recent attention, building on the impetus of work commenced in the mid years of the 20th century, such as Braidwood, MacNeish, Clark and Higgs. These days, the research agenda is very specific, and focuses on particular scientific methods, on restricted geographical regions or periods, or on various emerging post-processual philosophies. Together, the following topical comments offer us a sense of the immediacy and importance of the continuing research and study of agriculture, and, how this endeavour is bringing in a rich and varied harvest.

DOUG PRICE (University of Wisconsin) describes the 'Advances and directions of study of early agriculture' and assesses what is actually known of the processes, and what the definitions that we commonly apply to agricultural studies in archaeology really mean within the discipline. He makes the point firmly that it is the 'transition' which needs to occupy our attention and that archaeology still needs to engage with fieldwork alongside the molecular level of analysis that is currently stealing the show. He ends with an appeal to young scholars to enter this exciting field of research. He writes:

'As has often been observed, the transition to agriculture was the most auspicious moment in our human past since we first stepped upon the stage. As parsed by Bruce Smith (2001: 199), agriculture provided the lever for the extraordinary development that subsequent human societies experienced in the Holocene. The success of farming is documented by its explosive spread from cradles to the limits of culti-



'He's right. It is more exciting than hunter-gathering!'

vation, and beyond, in a few thousand years. Remarkable as well is its virtually instantaneous emergence in a variety of environments on all the continents save Australia and Antarctica.

'It is equally extraordinary how little we know about this phenomenon. The intent of his brief essay is to consider the current state of studies and to urge that more resources and expanded research be focused on the problem.

'But first some essential definitions and concepts. *Domestication* is a biological process involving genetic and morphological changes in wild plants and animals. The identification of new plant and animal species documents the domesticates. *Agriculture*, on the other hand, is a human process. As Barbara Bender explained so well some years ago (1978: 206), agriculture is "not about intensification *per se*, not about increased productivity, but about increased production and why increased demands are made on the economy."

'The *origins* of agriculture lie in the biological processes of domestication and concern the time and place that the cultivated plants and herded animals were changed forever. The actual origins of agriculture are such ephemeral moments as to be invisible in the archaeological record. We are in fact everywhere viewing the spread of agriculture, rather than its brief moment of birth. The *transition* to agriculture is the much broader and more important issue that is concerned with how and why these domesticates spread so rapidly across the continents.

'It is possible to examine the transition to agriculture through a set of basic questions — what, when, where, who, and why? Questions about the *origins* of domestication are largely concerned with finding, identifying and dating early domesticated plants and animals. The what, when, and where questions dwell in this realm. The resolution of these questions is largely a laboratory undertaking. New methods are employed to determine the identity of domesticates and their distribution in time and space. Microscopic techniques and the development of comparative collections have expanded research into vegetative parenchymous tissues, phytoliths, and starch grains (e.g. Hather 2000; Pearsall & Piperno 1993; Piperno *et al.* 2000). Root crops are finally coming into archaeological focus.

'The identification of animal and plant domestication is rapidly moving to the molecular level; genetic fingerprinting allows identification of modern wild populations most similar to their domesticated relatives and their geographic home. Examples document the heartland of einkorn in eastern Turkey (Heun *et al.* 1997), of maize in the Rio Balsas region of western Mexico (Doebley 1990), and the dual centres for the domestication of cattle in Asia and Africa (e.g. Bradley & Loftus 2000). Genetic studies of modern animal domesticates point to multiple centres of origin for cattle, horses, sheep, goats, and pigs (Zeder *et al.* in press). AMS dating is revolutionizing our understanding of the antiquity of agriculture by directly dating the remains of plants and animals. The fact that the earliest dates for various domesticate species are changing rapidly at the moment is a testament to the efficacy of this tool (Smith 1998). Archaeology has made giant steps in resolving what, when, and where in the last 15 years, but a great deal remains to be done.

'Questions about the *transition* to agriculture lie more succinctly in the realm of prehistoric archaeology, require substantial fieldwork, and can be investigated in a variety of geographic locations to accrue more information. Questions of who and why can be pursued with studies of material culture and settlement organization and distribution. The who and why questions revolve as well in the theoretical sphere. Any number of ideas have been conjured up over the years to provide answers, but they usually fail to conform to the facts at hand.

'The who question, in simplest terms, involves the identification of the individuals involved in the introduction of agriculture, whether local or foreign, whether male or female. Archaeology has been particularly remiss in this arena; opinion has often raced far ahead of knowledge. Archaeologists employ artifacts as proxies for people, making migration liable for the appearance of new objects and concepts. The who question has evoked the same response for generations — migration, colonization, expansion (e.g. Burmeister 2000; Chapman & Hamerow 1997). Migration is usually indicted for the spread of agriculture as well (e.g. Renfrew 1987), but other mechanisms must be considered (Zvelebil & Lillie 2000). New methods that study humans rather than artifacts will resolve this question, avoiding the pitfalls of proxy data. Archaeology at the atomic and molecular level is beginning to grasp human

movement in the past. Isotopic studies of tooth enamel document change of residence in the Neolithic of Europe (Price *et al.* 1998; 2001). Ancient DNA may provide related information, writing the genealogies of the first farmers.

'In spite of exciting advances, the outcome of this story continues to be how little is known about the prehistoric transition to agriculture. Many early domesticates are yet to be identified; plants used as condiments, medicines, and raw materials have yet to be investigated. Important non-food animals species, used as beasts of burden, pets, or sources of raw material receive little attention.

'So much remains to be done in so many places. Even in Southwest Asia, with the best record of early domestication, 95% of the effort in the last 25 years has focused on about 5% of the area. The first farmers of Turkey, for example, are virtually unknown. An understanding of the processes of domestication in Africa, south and east Asia is in its earliest stages. The last substantive fieldwork on the origins of agriculture in Mesoamerica was more than 20 years ago. South America is a huge continent with crops and herds originating in desert, alpine, and jungle environments, but almost no one is looking for the first farmers.

'The transition to agriculture is an enormous playing field, ripe for major discoveries, intriguing fieldwork, ground breaking lab work, and intellectual stimulation. Focus on the transition to agriculture, rather than origins, will better answer the what, when, where, who and why questions pertinent to this extraordinary phenomenon. Research can be conducted anywhere there have been farmers. In the last analysis, this essay becomes a recruiting poster, encouraging young archaeologists around the world to take up their tools and enter the fray. It's time to solve the tantalizing and wonderful puzzle.'

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KENT FLANNERY (University of Michigan) covers the 'Turning points in the study of early domestication', a set of stages that are intricately involved with his own career, working with many of the key figures. His perspective bridges the Old and New World, placing our current knowledge in historical perspective. He provides a salutory reminder of how recent are many of the means of scientific dating and analysis that we now take for granted, and cautions us to take a broad view of the various forms of data and of course of human behaviour, and not base interpretation merely on one or even two dimensions of evidence. Like Price, he strongly urges students to take up the field, most specifically in the areas of experimentation and modelling. He writes:

'Sixty-six years ago Gordon Childe introduced the concept of a Neolithic Revolution, stimulating a worldwide search for the origins of agriculture. That search began at the level of the archaeological tell and has now extended to the level of the phytolith, the pollen grain, and the DNA molecule.

'The year 1948 will go down as a turning point for both hemispheres: Robert Braidwood

Valley of Oaxaca. (Photo Simon Stoddart.)

began his hunt for early wheat, barley, goats, and sheep at Jarmo while Scotty MacNeish was searching the caves of Tamaulipas for early gourds, squashes, beans, and maize. I can sense how electrifying that era must have been, because it was my good luck to join Braidwood's team at Kermanshah in 1960, and MacNeish's team at Tehuacán in 1962. It was exciting despite the fact that the search for seeds in Iran was unaided by flotation, and the plants in both regions could only be dated by nearby pieces of charcoal.

'In the summer of 1961 I apprenticed myself to Stuart Struever at Apple Creek, Illinois, to learn the flotation method he had pioneered. Frank Hole and I brought this method to the Near East during 1961–63, making it possible to recover 45,000 carbonized seeds from Ali Kosh. Our crude manual techniques, however, were soon superseded by mechanized froth flotation, advanced by Eric Higgs' students at Cambridge. Today we take for granted that if your site has ash, you're going to get plant remains.

'I excavated Guilá Naquitz Cave in Mexico in 1966 and its analysis still continues, thanks to the magnitude of the changes that have since taken place. We found early gourds, squash, beans, and maize at the cave, but had no way to date them directly; the seeds were too small, and the maize cobs too valuable, to send to a radiocarbon lab. We had a series of cave matrix samples taken by palynologist James Schoenwetter, but were unsure what the results meant. Pollen grains identified as *Zea* sp. were present in strata antedating the earliest maize cobs, but were they from maize or its wild ancestor, teosinte?

'Today, with the advent of accelerator mass spectrometric (AMS) dating, tiny bits of squash seeds and maize cobs can be directly dated. In addition, the rapidly evolving analysis of opal phytoliths can be used to augment both pollen and macrofossil evidence. When Bruce Smith had our squash seeds AMS dated, we learned that: (1) the seeds were even older than we thought, but (2) were not always the same age as charcoal from the same stratum.

'Since no charcoal was associated with our early maize cobs, their date was revealed only when Dolores Piperno had them AMS dated. And when Piperno analyzed our phytoliths, we learned a valuable lesson: there were no phytoliths from maize cobs or teosinte spikes in the strata with pollen of *Zea* sp. I was understandably relieved that I had made no claims of early domestication based on pollen alone.

'I see this as a cautionary note for some of my colleagues who, in areas like Mexico's Gulf Coast, base claims of early agriculture solely on pollen grains identified as *Zea* sp. or *Manihot* sp. Beware. I am enthusiastic about the addition of pollen grains, starch grains, and phytoliths to the archaeologist's arsenal, but in a land with several species of *Zea* and dozens of species of wild *Manihot*, we need macrofossils identified to species.

'To be sure, even pollen grains are far from the smallest units now examined in the search for early domestication: one of the newest frontiers is molecular. In recent years we have seen DNA used to establish the links between domestic squashes and their wild ancestors, and between domestic alpacas and wild vicuñas. DNA may also tell us whether early cattle in the Nile Valley were locally domesticated, or introduced from the Near East.

'While I applaud these biological advances, they need to be accompanied by studies of economic decision making. In 1969, I pointed out that the Neolithic Revolution was preceded by a change in economic behavior that I called the Broad Spectrum Revolution. In a warmer but unpredictable early Holocene environment, mid-latitude foragers began to increase their diet breadth and exploit smaller territories more intensively; ground stone technology and storage facilities exploded. Mary Stiner and her colleagues are now able to quantify these new collecting strategies, which they attribute to "pulses" of local demographic increase rather than a worldwide population crisis.

'Stiner's efforts are complemented by (1) ethnoarchaeological experiments at wild plant

harvesting like those of Jack Harlan and Gordon Hillman, and (2) computer simulations of foraging strategy like those of Robert Reynolds and Steven Mithen. Such studies show us that plants did not simply domesticate themselves, as David Rindos argued. Their domestication was the result of human economic decisions and deliberate behavior, such as planting on virgin soil and removing competing wild vegetation. Similar decisions were involved in the shift from hunting to herding, according to the mathematical models of Michael Alvard and Lawrence Kuznar. We need more such experimentation and modeling. We also need to learn why sedentary life preceded agriculture in the Levant and coastal Peru, but only took hold in Mexico after 6000 years of domestication.

'Finally, we need to acknowledge the efforts of those who are no longer with us. Botanists like Hans Helbaek, Paul Mangelsdorf, Hugh Cutler, Thomas Whitaker, and C. Earle Smith, zoologists like Charles Reed and Sandor Bökönyi, and archaeologists like Scotty MacNeish pioneered the study of domestication. As we make the transition to molecular archaeology we should remember that if we see farther, it is because we stand on their shoulders.'

MARTIN JONES (University of Cambridge) in 'Directions of research in agriculture' takes up a particular thread set in train by Kent Flannery, the biomolecular dimension. He comments on the new directions of bio-archaeological research, within the context of the broader questions and key models that are stimulating particular methods, such as DNA and the human genome project. He writes:

'Archaeological interest in the development of agriculture has often returned to an issue fundamental to our understanding of how ordinary people change the way they live. One argument emphasises a diffuse and universal interaction with the natural environment, and a gradual response of evolutionary adaptation. The other argument places emphasis upon more radical and historical changes, occurring in a particular context and "diffusing" from that context by movement of people, materials or ideas. In the context of agricultural origins and spread, Mark Blumler (1992) has described these models in terms of "independent invention" and "stimulus-diffusion" respectively. Much of what is new and current in agricultural research is allowing us to take a fresh and critical look at these two models.

'The pursuit of the "independent invention" model has involved looking at novel domesticates and new areas of domestication. We might imagine that half a century of bio-archaeology had covered much of this ground, but there remain some very significant gaps and a great deal that conventional bio-archaeology can achieve. This is well illustrated by Dorian Fuller's recent seminal work on a series of key plant crops of southern India, simply achieved by small scale excavation and flotation of a number of known Neolithic sites. In addition to these, the key advances are now in the field of tissue and molecular analysis.

'Many of the several thousand known economic food plants have remained absent from the archaeological record simply because they leave distinctive traces of neither fruits or pollen in their tracks. The work initiated by Jon Hather in London, and continued by Victor Paz in the Philippines, on tissue fragments and their cellular structure is opening up the entire field of tropical archaeobotany, heavily geared towards roots and tubers (Hather 1994). Molecular analysis can now reach any species, and the combined study of modern and ancient DNA is sure to expand. Also of key importance are the lipids, which are not only casting light on such elusive crops as the leafy vegetables, but also giving new and precise insight into methods of food preparation and cuisine. Together with protein studies, lipid analyses are proving the best option for exploring the origins of dairying and such related issues as the Secondary Products Revolution (Evershed 1999).

'The pursuit of the "stimulus-diffusion" model has taken a new turn with the debate on the interplay between the spread of farmers, genes and languages (Bellwood 2001), a debate that remains extremely fertile for a number of reasons. The first of these is the sheer momentum of current genome research. The charting of the human genome is having a profound impact upon our view of the human past and of human migrations in particular, and there is nor reason to suppose that this will end with the "completion" of the Human Genome Project itself. Each new addition to the gene map has the potential to generate new hypotheses about

Studying seeds. (Photo Department of Archaeology, University of Cambridge.)

the past movement of people. Many of these past movements have been linked to the spread of farming communities linked by ecology, material culture and language. The real challenge of the new field of "archaeogenetics" is not just to generate yet more arrows across the map. It is instead to exploit the remarkable precision of DNA analysis to reach an understanding of what was happening to the human communities who found themselves along the course of one of those arrows.

'The proposed link between migrating farmers, genes and language has a great deal of potential as a focus of enquiry, although the most simplistic equations of the three are almost certain not to stand up to the growing regional archaeological data. Such data are repeatedly revealing odd mosaics of farmers and hunter–gatherers, mixes of cultivated and wild foods, and mismatches with the conventional artefact markers, such as pottery. The universality of some language groups has to arise from a combination of replacement and language shift, and not just one of these processes alone. I suspect the movement of farming itself will be best followed through the genetics and bio-archaeology of the food species themselves. Conversely, an exciting aspect of human genetics is its potential to unravel the complexity of what happens when a thin population of farmers moves in on an even thinner population of hunter-gatherers. This may involve such issues as marriage and residence patterns, that are now coming within the grasp of modern and ancient DNA studies.

'Much of this rapidly expanding database leads us to accommodate both the independent invention and stimulus diffusion models, but dispersing them in time. Detailed DNA sequence studies are revealing that many domestication pathways are multiple, and generally dispersed more or less as widely as the wild progenitor itself (Jones & Brown 2000). Conventional bio-archaeology is placing those "domestication" episodes at the end of a prolonged ecological interaction with, and adaptation to, fast changing environments of the early post-glacial. In other words, this dispersed evolutionary response by "independent invention" was a rather earlier episode than sometimes envisaged, emerging from the Upper Palaeolithic. Conversely, there do seem to be episodes, best encapsulated by the "stimulus–diffusion" model, in which farmers adopting a narrow food-web, dominated by a few starch-rich plant staples and domestic animals, progressively expanded their range, as did their genes, languages, and socio-political systems. Stretching out between the two, often for several millennia, is a series of unusual and variable societies, that make use of both domesticated and wild plants, vary greatly in their exploitation of wild and domestic animals, and are far less expansive than their later prehistoric successors. This is probably a fair description of the very first consumers of domesticated cereals in Southwest Asia, East Asia, and Central America, and other places besides. Within this prolonged gap between the early "evolutionary" episodes of independent invention among broad-spectrum feeders, and later "revolutionary" episodes of stimulus-diffusion by narrow food-web farmers, we might expect to see significant changes in our narrative. It is in a series of long, ecologically diverse, Neolithic episodes, that I imagine new methods of archaeology and genetics yielding interesting, sometimes quite detailed, and frequently surprising results.'

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Some studies of the transition to agriculture are distant from the underlying change in diet. The cultural-philosophical approach to the change to farming and food production is discussed by CHRIS SCARRE (University of Cambridge), who revisits the ideas and work of Jacques Cauvin who recently died. Was agriculture a single global phenomenon? Cauvin focused on the development of sedentism and food production in the Levant and equated the Neolithic Revolution with the foundations of modern human society, culture and mentality. This is a viewpoint that has been energetically taken up by some Post-Processual thinkers. Chris Scarre writes on 'Jacques Cauvin and the origins of agriculture':

'In a recent issue of *American Antiquity*, Richerson, Boyd & Bettinger pose the key question why agriculture did not emerge during the Pleistocene (Richerson *et al.* 2001). They present their argument in the form of two propositions: that agriculture was impossible during the last Glacial (owing to climatic instability), and that in the long run, agriculture was compulsory in the Holocene. Their explanation is framed at the broadest geographical and chronological scale, and comes down heavily in favour of climatic change — notably the abrupt transition from Glacial to Holocene — as the driving factor behind intensification. The search for common themes or common factors at such a general scale is of course entirely appropriate where agriculture is viewed as a global phenomenon. There are, however, alternative perspectives, which consider specific regional or local trajectories as the more relevant scale of analysis. We might, for instance, question whether agriculture is indeed a single phenomenon, or rather a series of individual instances of a general trend towards intensified interactions between modern humans and their food resources (e.g. Higgs 1972; 1975; Rindos 1984). The reified concept of "agriculture" on which many traditional accounts are predicated is as much a target for legitimate critique as is the Neolithic or the state. Early agricultural systems involved different species, different technologies and environments, and were associated with a diversity of social and economic regimes. Yet any such project to deconstruct domestication would run counter to other recent approaches which seek to understand the origins and spread of domesticates not in terms of economic adjustment but as a cognitive or symbolic shift which redefined human self-awareness.

Egyptian agricultural scene. (Photo Helen/Nigel Strudwick.)

'A leading proponent of this approach was French archaeologist Jacques Cauvin, who died late last year. Cauvin spent his professional life working on early agricultural sites in the Levant, and was a leading figure in the important excavations at Mureybit in Syria. His observation that innovations in symbolism prefigured and accompanied the Neolithic transition was a major influence on Hodder's *The domestication of Europe* (1990). What Cauvin envisaged was nothing less than a change in human cognitive and symbolic outlook, that preceded and made agriculture possible. The case was set out most fully in *Naissance des divinités, naissance de l'agriculture* (1994), which appeared in English translation six years later under the title *The birth of the gods and the origins of agriculture* (Cauvin 2000). In essence, his thesis argues "that it is actually in the Neolithic Revolution that we find the roots of the present state of the human race, not only in its domination and exploitation of the environment, but also . . . in the very foundations of our culture and mentality" (Cauvin 2000: 3).

'An important influence on the development of Cauvin's ideas was the discovery of Ain Mallaha in 1955 by Jean Perrot. This was a "village of hunter–gatherers" that defied the then-dominant model that sedentism should follow agriculture: a small settlement of five or six

sunken-floored round houses with storage pits and heavy ground stone tools designed for pounding and grinding. Such Natufian settlements developed all the technology that was needed for farming but continued to rely on wild resources. It was in the following period — the Khiamian — that the great change occurred, and this was not an economic, climatic or technological adjustment, but a symbolic one. It was marked by the appearance of female figurines and by the placement in houses of aurochs bucrania, both themes that recur in later contexts such as Çatalhöyük. For Cauvin, the Woman and the Bull were representations of deities, and revealed a new religious awareness that underlay and indeed inspired the development of domestication in the following PPNA phase. Thus the Neolithic Revolution provides "the clear demonstration of the fact that man could not completely transform the way he exploited his natural environment, his own settlements as much as his means of subsistence, without showing at the same time a different conception of the world and of himself in that world" (Cauvin 2000: 220).

'The primacy which Cauvin accords to the revolution of symbols and a new religious understanding are worlds away from traditional ecological or demographic models for the origins of agriculture. One wonders, perhaps, how well they would work as a general explanation, applied to other agricultural origins in other areas. We may, furthermore, pose the same question with which we started: if modern humans had already been in existence for tens of thousands of years, why did these changes not occur earlier? There is a mysticism about Cauvin's argument which invites caution. Hodder's interpretation of the Neolithic transition gives symbolism a rather different and more concrete role. He notes how at Çatalhöyük and other East Mediterranean sites, "human death, skulls, vultures ands wild animals were brought into the house. . . . animal death is linked to human death, 'male' dangers to 'female' dangers. This juxtaposition enhances the prestige of the social and cultural order which confronts and controls the agrios [the wild]. It identifies the domestication metaphor as the main mechanism for social control" (Hodder 1990: 294). The emphasis here is on the house as the centre and symbol of domesticated space. Anthropologist Peter Wilson takes a similar approach, arguing that as houses preceded agriculture, so it was houses that domesticated people before people domesticated plants: "the domestication of plants and animals follows the domestication of human beings and is inspired by it" (Wilson 1988: 3). Yet, as is well known, sedentism did not precede plant domestication in key areas of the world such as Mesoamerica (e.g. Pearsall 1995).

'The notion of the Neolithic as a symbolic revolution brings Cauvin close to current thinking on the Neolithic of northwest Europe. There is little evidence in this region, however, that a cognitive or symbolic change preceded the adoption of agriculture. In northwest Europe, the primacy given to the cultural and symbolic dimension of the Neolithic is one of *significance* rather than *chronology*. These societies at the very outset of the Neolithic appear to have engaged in a new project of enculturing the landscape, constructing monuments of earth, timber and stone that indicate a changed perception of the world. In many areas evidence for substantial permanent residential structures is slight, and life-styles may have remained relatively mobile for many generations. Furthermore, a number of authors (e.g. Bradley 1998; Thomas 1999) have sought to play down the significance of cereal cultivation in early Neolithic societies, a revision which would focus the spotlight all the more sharply on the Neolithic transition as a cultural or ideological phenomenon.

'Whether such interpretations will stand the test of time remains to be seen: palaeodietary evidence from northwest Europe is increasingly supporting the alternative argument, that the beginning of the Neolithic was marked by a relatively abrupt and significant switch to cultivated plants (Schulting 1998). Whatever the outcome of this debate, the importance of an associated symbolic shift is beyond question. In the final analysis, indeed, both Cauvin and Richerson may be held to be right, the difference being one of scale. Viewed in the broadest perspective, it may be entirely appropriate to consider agriculture the outcome of a "natural" evolutionary process operating at a global level, waiting only on the development of modern humans and suitable climatic conditions. Yet domestication and the manipulation of plants and animals were also embedded in regionally-specific social and ideological contexts which first made them possible. Furthermore, there is little question that domestication

was not just an economic process but that, as Cauvin remarked, it introduced concepts and ideas with which altered human awareness and inspired new cosmological and ontological understandings.'

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LEE NEWSOM (Penn State University) provides a more detailed analysis of the regional scale of analysis. She comments on the developments of research in early Agriculture in the eastern and central United States of America, where surprising evidence has demonstrated the origins of important crops, such as gourds, sunflowers and chenopodium, in several different centres. Problems of taxonomy, pollen data, dating, and reliance on present day specimens to understand ancient samples are discussed. She writes:

'Eastern North America has been recognized as an independent centre of plant domestication, where the timing and trajectory of domestication and the circumstances behind food producing economies are fairly well understood. I emphasize new developments and recent controversies concerning this region.

'Among the earliest plants from archaeological sites in eastern North America are gourds of the genus *Cucurbita*. Once characterized as exotic domesticates from Mexico, these were ultimately recognized as part of the native flora. That they derive from indigenous wild stock is supported by biosystematics, genetic, and isozyme data, as well as seeds from north Florida greater than 12,000 years of age. Together this evidence demonstrates a lengthy independent history in eastern North America (Decker-Walters *et al.* 1993; Newsom *et al.* 1993; Wilson *et al.* 1992). An increasing number of early to mid-Holocene cucurbit identifications have been reported and Fritz (1999) suggests they represent the earliest cultivated plants in the region. Of particular interest is whether *Cucurbita* identified from Maine and Pennsylvania might be an indication of the former natural range of the genus, or signifies gourds cultivated beyond that range. The identity of the early gourds, i.e. the wild ancestor of later domesticated *C. pepo* ssp. *ovifera* in the region, has been linked to ssp. *ovifera* var. *ozarkana*. Recently, there has been a call also to reconsider ssp. *fraterna* (Sanjur *et al.* 2002) of northeast Mexico as the progenitor. I suspect the situation will prove complex, with perhaps the Florida *Cucurbita* as part of gulf coastal developments (possibly including northeastern Mexico), separate from var. *ozarkana* and the Phillips Spring and other mid-continental archaeological gourds.

'Aside from gourds, at the epicentre of agriculture origins is a suite of weedy annuals variously cultivated and domesticated by at least the 3rd millennium BC in the Midwest (Fritz 1994; 1995). The earliest of these appear to have been sunflower (*Helianthus annuus* var. *macrocarpus*) and sumpweed (*Iva annua* var. *macrocarpa*), followed somewhat later and in some places by chenopod (*Chenopodium* sp. [considered *C. berlandieri* ssp. *jonesianum*]). By this time there is considerable evidence that *Cucurbita pepo* was domesticated and cultivated widely as a food or container crop. Less clear is the status of other plants — erect knotweed (*Polygonum erectum*), little barley (*Hordium pusillum*), maygrass (*Phalaris caroliniana*) — among others. However, the conditions of their occurrence and other criteria suggest they were part of this emerging horticultural tradition. All of this interaction represents indigenous developments and innovations, long recognized as a local trajectory of domestication comprising an independent centre of domestication and agricultural origins (Smith 1992). The later appearance and spread of the tropical cultigen maize, among others, has been clarified with new AMS dates of particular specimens together with isotopic

evidence. The earliest known maize dates to *c.* 2000 years ago (Illinois), and an increasingly widespread presence is documented for the succeeding Middle to Late Woodland periods (McElrath *et al.* 2002), followed by the transition to maize (maize, beans, squash) agriculture. (Claims for earlier maize based on pollen are tenuous, largely for the reasons outlined by Crawford *et al.* 1997 and Eubanks 1997.)

'Away from the Midwest, maize and the weedy domesticates' presence varies, although cucurbit remains are commonly reported. For any given location, whether and when specific taxa were grown or achieved prominence appears to be highly dependent upon local ecology and social-political dynamics. In some areas wild plant resources seem to have been intensively managed; even the Calusa of South Florida maintained gardens with gourds, papayas, and peppers. The evidence for widespread, ancient but varied cultivation systems is very provocative.

'The status of eastern North America as an independent centre of plant domestication has been challenged by Lentz *et al.* (2001), after their discovery of apparently domesticated sunflower from southern Mexico. Believing, unlike me, that multiple domestication events are unlikely, they question the status of North American archaeological sunflower, as well as the reality of an independent centre of domestication, citing problems with measurements from carbonized specimens, relative ages of specimens, and recent molecular data on extant sunflower populations. Their argument is problematic focused as it is on one seed and achene of a single taxon. Lentz *et al.* seem unaware of the complete body of data regarding the eastern domesticates, the archaeology of the region, and the full complement of AMS dates now available. Moreover, as Heiser (2001) points out, the molecular data do not negate a North American origin for domesticated sunflower, as Lentz *et al.* assert.

'Vast numbers of seeds and fruits from the Midwest have been carefully measured and analyzed; the data supporting inferences of domestication of the various taxa are sound. Details of seed size, surface characteristics, coefficients of variation, and so on are critical to describe and compare archaeological plant specimens. Nevertheless there are limitations to morphometric data, including problems with using means (Lentz *et al.* 2001: 373) to compare the Midwest seeds. New initiatives combining traditional archaeobotanical approaches with molecular studies are therefore very promising. I agree with Sanjur *et al.* (2002) and others that the only clear way to resolve nativity and to distinguish between inherent morphological variation in natural populations and human-induced variation in the domestication process is to examine together morphometric and molecular data directly from ancient remains. Also, we should be very judicious about the classification of archaeological specimens in terms of modern binomials based on extant or recent taxa. To specify that an archaeological taxon is in fact *that* particular entity in the past is potentially misguided as it may fail to recognize, even mask, the fact of extinction and ephemeral taxa. In other words, assigning ancient material to modern species and finer taxonomic levels is quite a statement, given the nature of species. Unless and until we have corroborating molecular data, we may be unable to make such a precise identification. This is not to imply that we discard taxonomy, but to emphasize that some of these discussions may be heading off into unproductive territory lacking the molecular support, and we may be missing opportunities to discover and discuss truly unique events. Considering this and population ecology, sunflowers from distant, vastly differing regions might well not be the same thing, but a case of convergent domestication.'

Acknowledgements. Many thanks to Gayle Fritz, Frances Hayashida, Bruce Smith and Alan Walker for their comments on earlier drafts of this piece.

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Gary Crawford (University of Toronto at Mississauga) provides a parallel assessment of the 'Advances and new directions of agricultural research in east Asia'. Rice has dominated recent research, but several other crops deserve attention as well. Approaches include island studies, especially in Japan, work in Korea and new dating projects. Crawford cautions against the rather simplistic economic views that tend to dominate cultural and economic classification of some cultures, such as the Jomon, where their perceived hunter–gatherer economy overrides archaeological evidence. He writes:

'The rapid progress in agricultural origins research through the 1990s in East Asia has slowed somewhat. A number of projects some international, are underway with results still forthcoming. Key developments are eminating from Korea and Okinawa however. More local scholars are trained to work on the problem so I expect to see significant progress over the next few years.

'Comprehensive reports of plant and animal remains assemblages in good archaeological contexts are still rare in China due in part to a preoccupation with rice. We still know more about early rice than any other plant in the Early Neolithic there (see Crawford & Shen 1998). Bryan Gordon at the National Museum of Civilization in Ottawa has prepared a web site (http://www.carleton.ca/~bgordon/Rice/), where papers otherwise inaccessible to westerners are posted as English translations. One of the most significant papers at the site is by Y. Satoh (http://www.carleton.ca/~bgordon/Rice/papers/sato99.htm) and was only available in Japanese at the time Shen and I wrote our paper for Antiquity.

'While I was pondering how we might balance the dominant influence of rice on the East Asian scene, *Science* published an issue (vol. 296, No. 5565) devoted to the first rough draft of the complete genome of the two closely related subspecies of rice, *Oryza sativa* ssp. *indica* and *O. sativa* ssp. *japonica*. So much for the balance. It's too early to know what impact this accomplishment will have on understanding rice domestication but the impact will, indeed, be felt. This is the first cereal and only the second plant to have its genome worked out. Apparently cereals tend to have the same genes and in the same order so this is a significant step in documenting the genomes for all cereals. Detailed studies based on the results are beginning to look at species divergence and gene expression. We might anticipate better understanding of non-shattering phenotypes and the evolution of temperate adaptations such as changes in flowering time. An apparent large-scale difference beween the two subspecies may point to a method for differentiating archaeological collections but we are warned against making too much of these apparent differences.

'A former student of Deborah Pearsall, Zhujin (Jimmy) Zhao, is now at the Institute of Archaeology in Beijing. He is coordinating flotation and the analysis of resulting samples from the sites the Institute is excavating. Zhao is working to establish the Institute as a national research centre for archaeobotany. His work has clearly become a priority in China.

'Projects by Harvard's late K.C. Chang in eastern Henan, Liu Li (Latrobe) and Henry Wright (Michigan) in North China (http://www.archaeology.latrobe.edu.au/research/survey/index.htm), Jian Leng of Washington University, and Anne Underhill (Chicago Field Museum) in Shandong all have palaeoethnobotanical data undergoing analysis at the University of Toronto. The bulk of the data are Late Neolithic but the analyses will provide a detailed look at plant use, agriculture, and anthropogenesis in mid-latitude China. Rice seems to have played a role in socioecconomic systems in the Late Neolithic in the area but millets and other plants were likely more significant if the samples from Shantaisi, Henan are typical (Crawford *et al.* 2001).

'My own research and that of few of my students over the years have been exploring secondary agricultural origins in northern Japan.

Masakazu Yoshizaki, who is a great supporter of agricultural archaeology in Japan, retired and his lab is no longer operating. But the importance of this type of research is now deeply ingrained in the north so it will continue. The main university laboratory in the north now is run by Hiiroto Takamiya who is piecing together the agricultural history of Okinawa. As a former student of Timothy Earle, he is working within an island ecosystem theoretical framework. Takamiya (2001) has rejected all previous hypotheses about agricultural origins in Okinawa. The transition appears to have been abrupt and took place between the 8th and 10th centuries AD, not long after the same transition occurred on Hokkaido (Takamiya 2001). The principal data are from the Nazakibaru site where rice, wheat, barley, foxtail millet and legumes have been recovered along with weedy plants associated with agriculture.

'Until now, probably the most poorly known area in terms of agricultural origins in East Asia has been Korea. This is changing. Gyoung-Ah Lee, currently a doctoral candidate at the University of Toronto, has integrated palaeoethnobotany into several CRM projects in South Korea. One project is along the Nam River while another is on the southeast coast facing Kyushu, Japan. A series of AMS dates on cultigens from a range of periods and contexts sets out a hitherto undocumented agricultural history of Korea beginning at least as early as the Middle Chulmun (Crawford *et al.* 2001).

'There is a downside to the attention agricultural origins receives in East Asia. Taking Japan as an example, simplistic views are dominant. In the case of Japan many scholars are locked into an epistimology that sees classification of the Jomon as agricultural or not as an end in itself. This "dualistic epistemology" (Smith 2001: 2) is common elsewhere of course. Most, or at least many, archaeologists accept that Jomon peoples had a few crops. So the Jomon is one of the "in-between" economies Bruce Smith would like archaeologists to pay more attention to (Smith 2001). For many, Jomon people are hunter–gatherers and the crops from Jomon contexts are dismissed as irrelevant because they are so rare. Others feel the Jomon is agricultural because of the presence of crops. Researchers must come to terms with the complexity of economic and social issues in the Jomon. Until this happens, the Jomon will still not be explored in the proper way. Studying the middle ground as Smith calls it (2001: 1) should become a significant research area not only in Japan but in China and Korea as well.'

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Finally, ANTHONY SNODGRASS (University of Cambridge) comments on 'Archaeology and Mediterranean agriculture', a developed agriculture that supported the complex societies of the region. He reveals the fact about which so many scholars, particularly prehistorians, prefer to remain ignorant, that archaeological knowledge of agriculture, its role, landscapes and the societies that relied upon it, are best known now from work in areas such as the Mediterranean. Decades of intensive field survey, environmental work, sampling and, indeed, using the rich historical sources provide an extraordinary depth of knowledge of town and country, farm and food. He writes:

'"In the Mediterranean world, there is still a relative lack of attention to infrastructure", wrote the Editors in the last issue (ANTIQUITY 76: 2), and of course they were right. But here we have a topic which, if it belongs anywhere (a question which not so long ago was by no means a rhetorical one), belongs to infrastructure. The countryside, for all urbanised societies, has been a place where history of a kind might be made, but never History with a capital H. By way of contrast, 'a pattern of dynamic and changing political worlds' (to quote again from the same Editorial) can be seen as 'the product of a text-led analysis'. A strength of the more static agricultural sector is that it can hardly be classed as 'text-led'.

'But a paradox has gradually emerged: intensive surface survey, the main instrument by which overdue attention has been brought to bear on the agricultural infrastucture, has achieved much its best results (or so I would argue) for the fully historical periods: the Classical Greek, the Imperial Roman, the Medieval, the Ottoman. For the prehistoric and proto-his-

Don Spratt, the well-known investigator of the land boundaries of the North York Moors, posing with traditional agricultural equipment in a Mediterranean landscape (Casentino, Tuscany). (Photo Simon Stoddart.)

toric eras, the outcome has been much more muted. I see the operation of two prime factors behind this. First, the greatest general asset of Mediterranean archaeology is what might be called the 'quantitative bonus': the sheer proliferation of finds creates a vastly larger sample, on which to test almost any theory or generalisation that one cares to name, than in most other areas of the world; and enables diachronic trends to be followed with far greater confidence, aided by relatively accurate chronological schemes. Yet for periods before an advanced stage of the 1st millennium BC, this prime quantitative asset (and in consequence its by-products) has proved not to operate fully: survey has repeatedly failed to find either "sites" or artefacts in numbers that compare with those of later centuries — to the point where anxious head-scratching has given way to tentative explanatory theory (of which Bintliff *et al.* 1999 will serve as an example).

'Secondly, survey in the agricultural sector of an historical society may not be text-led, but it derives rich benefit from being what one might call "text-followed". Once a pattern of settlement, of cultivation, of demography, of internal migration is detected, by purely archaeological methods, it can be nuanced, modified, sharpened and even explained by recourse to historical documents, among which inscriptions and coins can play as great a role as texts. Sources which completely failed to predict such archaeological discoveries can still offer great enlightenment once they have occurred, in survey as in excavation.

'Thus it is that discussion of the agricultural regimes in these historical periods of the Mediterranean cultures has moved on to secondary issues of a more sophisticated kind, many of which had simply not arisen elsewhere, or else had been regarded as the province of excavation archaeology. This is not to say that the evidence of surface finds has yet enabled us to settle such issues. A good example here is seasonality of occupation of agricultural buildings: a generation ago, in the heyday of processual archaeology, this was a prime topic of debate in the interpretation of excavated sites all over the world (see for example Courbin 1988: 76–7, 157). In Mediterranean survey, it has been grappled with inconclusively; but the encouraging thing has been that it is felt to be one of the questions posed by the evidence. Here too, the eventual answers may emerge from a more far-sighted handling of excavated finds, in which palaeobotany and animal bones are given their due.

'Rather more positively, agricultural regimes of really high overall intensity seem to have been a property of certain historical epochs, and their study enjoys all the quantitative bonus of Mediterranean historical archaeology. This has produced a number of fruitful secondary debates. Some arise from the interpretation of the distinct rural "sites": on the establishment, for example, of criteria for determining levels of occupation, and explaining the wide quantitative and qualitative variation in the evidence which the "sites" present. Others arise primarily from the observation of the territory intervening between these locations: a prime instance here has been the dis-

cussion of the prevalence of past fertilizing activity, the "manuring hypothesis" (for the most recent discussion of these and other issues, see Pettegrew 2001 — a paper drawing over 80% of its references from the Mediterranean lands in historical times — with the attached responses).

'For the first time, the archaeology of Mediterranean agriculture has begun to acquire faith in itself. Instead of continuing to look nervously over its shoulder at other studies, it has a new air of blazing a trail in its own right. Text-based studies of similar subject-matter — Sallares (1991), to name one important but underrated work — still have much to contribute; but they in turn draw increasingly on archaeology. Ethnographic evidence and ethnoarchaeology are handled more critically than in many areas of world prehistory. We may compare the progress of work on agriculture with that of the vastly longer-established study of Mediterranean urbanisation: which is making the more measurable advance today? In the Roman world, agriculture has become the central topic for work in more than one Mediterranean province (Gallia Narbonensis, Hispania Tarraconensis), as much as it is in Britain. Perhaps most significantly, even among the more militant prehistorians, with their inbred distaste for any field remotely linked with the study of the Classical world, there is an admission, however grudging, that here at last there is such a field from which they can learn something.'

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☙ We have invited one of Anthony Snodgrass' pupils, JAMES WHITLEY (University of Cardiff), recently appointed director of the British School at Athens, in a country perceived by some as the heart of the Mediterranean, to set a programme for his period of office. The British Schools abroad — centres of academic research in Athens, Rome, Ankara, East Africa for instance — have been considered by a number of British (generally insular) academics to be an expensive infrastructure, in place merely for historical reasons. The roles of the Schools may have changed, but we consider them to have a vibrant and effective role which can project young scholars into cultural exchanges much earlier in their careers than in countries which lack such facilities. Each of these schools has generated profoundly original research, a taste of which has already been given by Anthony Snodgrass. The successes and the opportunities for fresh success deserve more publicity. James Whitley writes:

'The British Schools and Institutes abroad have not had a particularly high profile in archaeological debates in Britain in recent years. They have rarely figured in the pages of ANTIQUITY. Debate about the nature and direction of archaeological research in Britain has been driven either by the demands of rescue archaeology and "heritage management", or by the theoretical issues raised and debated at successive TAG conferences. This does not mean that these Schools and Institutes have been inactive; far from it. But engagement in long term research projects, projects which, perforce, require the maintenance of good relations with the host country, do not necessarily oblige one to maintain a high profile.

'The British School at Athens is the oldest and (some would like to think) the most distinguished of these schools. Founded in 1886, its archaeological work has, over the years, embraced excavation at Knossos, Palaikastro in E. Crete, Phylakopi on Melos, Sparta, and Lefkandi on Euboca. All these are sites crucial to our understanding of the East Mediterranean in the Bronze Age and Early Iron Age. Excavation at these and other smaller sites (one thinks of Myrtos, Phournou Korifi) have recently been supplemented by large scale survey projects, of which the Boeotia survey is the most ambitious. But the School is not simply an office. It is an academic institution in its own right. It has been instrumental, not only in providing a context for the pioneering work of Evans and Wace, but in the intellectual development of many major figures in current British archaeology. I am not simply thinking here of distinguished figures in the"Classical" field, such as John Boardman or Anthony Snodgrass, but also of highly "theoretical" prehistorians, such as Paul Halstead and Colin Renfrew.

'The School's academic distinction in archaeology has often led to the misconception that it is a school *of* archaeology. Certainly, it has many archaeological facilities — the Fitch Laboratory of archaeological science, where pioneering work on ceramic petrology and petrography was undertaken from the 1970s onwards being only the most important. Certainly too much of the School's administration is taken up with the processing of permits for archaeological study, excavation and survey, as is expected by our host country. But this does not exhaust the School's interests, which traditionally have been both broad and liberal. Its purpose has been to support all kinds of research into Greek lands, from the Palaeolithic to the present. The School has supported Byzantine as well as Classical studies, and has always been involved in the study of modern as well as ancient Greece. It numbers amongst its past assistant directors distinguished Mediterranean anthropologists, such as Roger Just. The School's library and hostel are as likely to be filled with historians or anthropologists as they are with the experts on Mycenaean pottery, and the School's common room (the Finlay) provides a forum for the kind of interdisciplinary conversation that is rarer and rarer in the RAE dominated environment of modern universities.

'In an ideal world, the School should simply maintain this liberal policy, one where the institution simply accommodates itself to the interests of those scholars and scientists who wish to work in Greece, whatever those interests happen to be. But this ideal world, the world of unconditional state support for knowledge and culture, is not the world in which we now live. Both the political and the academic climate is more and more concerned with measurable outcomes. We may protest that this climate is essentially illiberal and philistine (it is), but it is unlikely to change much in the near future. The School's liberal philosophy is already difficult to sustain in practice, and will become more difficult in the future. Moreover, though in theory the School seeks to support all kinds of research into prehistoric, ancient, medieval and modern Greece, it has in practice its closest links with departments of Classics and Archaeology, and it is most closely identified with archaeological endeavours. It is by these endeavours that many believe that it stands or falls.

Knossos.

'Here the School faces a number of difficulties. First, though its income is small, the School has a number of long-term responsibilities, which it cannot simply slough off. Chief amongst these is the management of research into the major sites for which the School is, in the eyes of the Greek authorities, responsible, particularly Knossos. Second, too many of the School's field projects remain unpublished. Third, too small a proportion of the School's budget is spent on research, especially in comparison with other British Schools and Institutes abroad.

'All this is to suggest that the principal difficulty the School faces is in relation to its standing in the United Kingdom, and its relation to British universities. Here the School faces the conundrum of trying to maintain its liberal philosophy and yet adapting to new conditions. There are however a number of directions which can be pursued. First the School must develop a long-term research strategy for those sites with which it is inescapably associated, principally Sparta and Knossos. A strategy is not merely an aspiration to excavate, but an attempt to address important questions using the range of scientific and other techniques now available to the archaeologist. Such a strategy must embrace survey, future excavation and a programme of publication of all past work. Second, the School must seek to develop a number of high-profile, "flagship" projects in association with a number of UK universities. These projects should not simply be excavation or survey, but should be, in the broadest sense, interdisciplinary. A project on ancient terracing, which would involve not only archaeologists and historians, but geomorphologists and even astrophysicists may provide a model for this type of endeavour. It is precisely such

https://doi.org/10.1017/S0003598X00090268 Published online by Cambridge University Press

projects that both public (NERC; AHRB) and private (e.g. the Leverhulme) funding bodies are currently most interested in. To this end, the officers of the school (the Director, the Director of the Fitch laboratory, the Assistant Director and the Knossos Curator) should have all recently acquired an affiliation with UK institutions, an affiliation which will (we hope) allow them to apply to the research councils for funding. At present, Schools and Institutes abroad do not count as Higher Education Institutions, which bars their officers from applying to the research councils for money. The only state funding they receive comes from a grant-in-aid from the British Academy. This creates an absurd situation in which a major laboratory of archaeological science, such as the Fitch, cannot apply for any scientific funding from NERC, simply because it is based abroad. This anomaly needs to be remedied. Finally, the School has to fund raise. Fund-raising is now a deeply serious, professional activity, and the School needs all the assistance it can get. This fund-raising should be research led, and relate directly to the research projects with which it has, is and will be associated.

'All this is not to say that there are not other problems the School might face, or other important tasks for the director. It is the job of the director to take an interest in students' research, to introduce students to Greek scholars in their field, to promote debate at seminars and elsewhere, and to maintain good relations with the Greek authorities and with Greek universities. But good relations with other Schools, with the Greek authorities and with Greek academic institutions depend in large part on the vigour of the School's research. Maintaining this vigour will be the future director's primary task.'

We are delighted to announce the appointment of Professor Martin Carver (York University) as our successor as Editor from 1 January 2003. Martin is well known for his excavations at Sutton Hoo, and it is highly appropriate that he should take up the editorship of the journal that gave the first detailed information of that significant site. He is less well known for the fact that he provided one of the editors with his first lecturing post! In the next issue we will give formal details of the new editorial offices.

ANTIQUITY continues its programme of celebration of 75 years of publication. As we write we have just attended the Society for American Archaeology conference in Denver where a symposium reviewed the achievement of ANTIQUITY, and whose papers will be published in December. The conference was also distinguished by a joint Cambridge and ANTIQUITY party, notable for its Scottish ritual. By the time of publication of this issue, we will also have held a celebration of ANTIQUITY in the Society of Antiquaries. Part of the marking of the celebration of 75 years of publication has been the setting-up of the Antiquity Papers to reprint classic papers from those formative years of archaeology. The first of these, *Landscapes from Antiquity*, was published in 2000. The second, *Celts from Antiquity*, has just been published and further details can be obtained from the ANTIQUITY office (catm20@cam.ac.uk).

In this Celtic spirit, we publish two short reports by authors whose articles also appear in the edited volume, *Celts from Antiquity*.

ANDREW FITZPATRICK (Wessex Archaeology) provides an update on the treatment of 'Treasure', entitled 'A tale of two hoards: the Snettisham Iron Age treasure ten years on':

'The recent volume of collected papers, *Celts from Antiquity*, includes two contributions on the great hoards of Iron Age gold torques from Snettisham, Norfolk that were first published in 1991 and 1992 (Stead 1991; Fitzpatrick 1992). The decade following their first publication has

seen fundamental changes in the frameworks within which finds of this kind are treated, as the way the discovery in 2000 of the Winchester (Hampshire) hoard has been treated shows.

'Until 1997, the application of Treasure Trove to protect antiquities required that the intent to recover objects of precious metal had to be demonstrated. Reporting on the Snettisham find in 1991, Ian Stead commented that the law was "archaic" (1991: 455), while in response it was suggested that the opposition between sacred and profane that this medieval law had come to enshrine might "not be helpful" (Fitzpatrick 1992: 397).

'Since then writers, who, unlike Ian Stead (1995), have been unfettered by the responsibility placed on the British Museuum to examine and analyse potential cases of Treasure Trove have tended to regard the Snettisham hoards as votive (e.g. Davies 1996; Haselgrove 2001: 49–51). For Barry Cunliffe, there is "little doubt that the motive for deposition was ritual" (1997: 196). As argued in 1992, the broader patterns of combinations of objects and their deposition in particular places in the later Iron Age of central and western Europe appears to be supported by new finds (e.g. Van Impe *et al.* 1997; Fitzpatrick forthcoming).

'The 1992 contribution concluded by asking whether it was time to reconsider again the inapproprate frame of reference of treasure trove. Since then the context has changed. In July 1996, a new Treasure Act gained Royal Assent and come into force in England, Wales and Northern Ireland in September 1997. The new law removed the worst anomalies of the treasure trove, and has led to a ten-fold increase in the number of cases of treasure. But as it still applies essentially to finds of precious metals, over 95% of archaeological finds are effectively excluded.

'Recognising that there was a need to improve arrangements for the recording of all archaeological finds, the Government supported the Portable Antiquities Scheme. The success of the partnership that the scheme has engendered is exemplified by the tale of the Winchester hoard.

'Discovered in 2000, this hoard is the only major discovery of gold objects of Iron Age date other than coins in England since 1990. The hoard of neckrings, bracelets and brooches was reported by the finder to the Portable Antiquity Scheme recording officer for Hampshire, and the findspot was subsequently examined by archaeologists from the British Museum and Winchester Museums Service (Hill 2001). The hoard, the initial interpretation of which had been as a votive offering, was declared Treasure in March 2001 and has since been acquired by the British Museum. The finder and the landowner have received *ex gratia* awards for reporting the discovery. (See colour picture p. 310.)

'The tale of the Winchester hoard exemplifies the ways in which the Portable Antiquity scheme is succeeding, but its future funding remains uncertain. [Heritage Lottery funding for three years more was confirmed in late April — Ed.] And it is only one part of the equation to improve the ways in which the cultural heritage is protected in the United Kingdom, as the continuing story of the Snettisham hoards sadly shows.

'In 1991, a vast hoard of Iron Age coins was looted from Snettisham, excavated illicitly close to the British Museum excavations. Around 6,000 coins had been buried in a silver bowl, and a separate deposit of around 500 gold coins with some ingots lay under the bowl. The "bowl hoard" is slightly later than the hoards of torques from Snettisham, but the placing of the coins as discrete deposits is analogous to the ways in which some of the hoards of torques were deposited. But little else about the "bowl hoard" is currently clearly defined. Dispersed through the Antiquities market, most of the coins had been to America — and back — in the 18 months before Ian Stead was able to vouchsafe the context (Stead 1998: 147–8).

'Also in March 2001, the Government announced the decision to accede to the 1970 UNESCO Convention on the Means of Prohibiting and Preventing the Illicit Import, Export and Transfer of Ownership of Cultural Property. A recent Review of the Treasure Act has recommended that the definition be extended to include deposits of base metal objects of prehistoric date. Government has accepted the recommendation and the order extending the definition will be introduced.

'These are welcome steps forward. But the journey to improve the ways in which our common past is valued and protected remains a long one.'

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Over the past five years we have been to many conferences, and we plan to report on collective experience of the culture of conferences by the editorial team in the next editorial. VINCENT MEGAW reports here on a small, and even beautiful, meeting that he entitles, 'Fresh sardines and stone knights'. He writes:

'The past twelve months seem to have been a good year for Celtophiles and Celtosceptics in Iberia. Between April and December, master-minded by the ubiquitous Martin Almagro-Gorbea, the major exhibition *Celtas y Vettones* was held at Ávila — Spain's answer to the 1991 Venice block-buster, *I Celti*, together with a catalogue to match. In January 2002, a two-day symposium was held in Lisbon on the subject of '*Die lusitanisch-gallakischen Kriegerstatuen*'. The meeting was organised with great attention to detail by Dr. Thomas Schattner, Deputy Director of the Madrid section of the German Archaeological Institute, and with Emeritus Professor Otto-Herman Frey as the genial *éminence blanche*. The opening was in the Museu Nacional de Arquelogía, splendidly located in the harbourside monastery of Belém built by Manuel I to mark Vasco da Gama's discovery of the route to India. Here four of the warrior statues central to the meeting's theme kept a watchful eye on those congressistes seduced by the sea air and the smell of fresh fish frying.

'Attended by some twenty invited speakers, not only from Spain, Portugal and Germany, but also France, Hungary and Australia, — a case, one might say, of rounding up all the usual Iron Age suspects — the papers were not restricted to Galicia and Lusitania. Indeed, the opportunity was taken to review what is and what is not known of Iron Age sculpture from the Iron Gates to the Atlantic seaboard. Thus Frey spoke of the relations between the Celtic and the Mediterranean worlds manifest in the remarkable stone knight discovered in 1996 within a kind of early La Tène *temenos* below the Glauberg northeast of Frankfurt. 'Glaubi' is currently the centre-piece of the must-see exhibition, *Das Ratsel der Kelten* which has recently opened at the Schirn Kunsthalle in Frankfurt. Both Dirce Marzoli, speaking about the late Hallstatt naked warrior statue found in 1962 at Hirschlanden, Kr. Leonberg, and Bruno Chaume on the seated figures found in 1991 in the excavation of a square enclosure at Vix 'Les Herbues' — drew attention to earlier evidence of the influence of Italian models on local sculpture.

'This influence must have been one of the invisible imports which accompanied the trade for Etruscan bronzes and Greek fine pottery. André Rapin, in reviewing the evidence which is currently emerging from re-examination of the statuary of the Celto-Ligurian area, showed that statuary at the sanctuary sites of Roquepertuse and Entremont is at least of 5th to 3rd century BC date. Here the cross-legged Roquepertuse hero-figures with their breastplates echo the iconography of the Glauberg finds. On the other hand, it seems difficult to link this area with Iberian warrior-figures (See colour photo p. 310).

'Hardly suprising, it was the discussion on the Iberian warrior figures which generated the most interest. Indeed, as these turned to aspects of ancient ethnicity, one felt that the Spanish Civil War was about to break out again. Thanks to Francisco Calo Lourido, there was at least a good foundation for debate in a fully referenced and illustrated catalogue of the 32 surviving warrior statues of north-western Iberia, the region of the so-called "Castro culture".

'Several speakers spoke of the enlargement of the distribution of castros in the Augustan period and J. Alarcão drew attention to the clusters of these defended settlements as reflecting tribal distributions, each tribe having its own warrior — or better hero — figure, an idea recently extended by Barry Cunliffe who suggests their role as territorial markers. However, as Fernando Quesada Sanz says, some 24 inscriptions on the sculpture seem to argue for a 2nd-3rd century BC date — at least two centuries earlier than that conventionally offered. Reuse in the Roman period when other inscriptions

occur, also seems likely. There is clearly a major problem with dating the Iberian figures. Despite 140 years of research, serious discussion as to their origin and development is only beginning. Thomas Schattner, having given a historiographic overview at the beginning of the conference, also contributed a suitably heroic attempt at a stylistic analysis in which he indicated arguments for an earlier (Iron Age) as well as a later (Roman) phase. Debate then continued as to whether the sword, helmet and shield common to the warrior-statues, could, in fact, be pre-Roman. After all that, it hardly came as a shock to discover how uncertain the chronology of the castros themselves is; despite excavation in recent years, much remains to be done before fundamental questions of chronology and cultural affinity can be firmly established.

'In view of the trouble-free nature of the organisation throughout, it seems almost superfluous to add that the proceedings of the conference have already been edited and will be published in the next volume of *Madrider Mitteilungen*.

'So an action-packed two days and nights with only one obvious complaint — it would have been good to have had the opportunity to visit some of the castros which still hold the clue as to the true age and cultural significance of the warrior statues of Lusitania and Galicia. And whatever did happen to that promised visit to the Restaurante Celtas for a night of *Fados e guitarrandas*?'

☙ We are pleased to add two details to previous editorials. Professor VALERIE MAXFIELD has provided us with information that was not publicly available at the time of publication of the last editorial. We are delighted to add Archaeology at Exeter University to the list of those who earned maximum marks in the teaching assessment of archaeology departments (QAA) and only missed a top Research rating by one grade, earning an excellent 5. This latter grade was a major improvement from a 3a rating in the previous assessment, achieved by one of the smaller departments of archaeology (8 people). We have equal delight in reporting that MARGARET HODGE, the Higher Education Minister, has promised that individual university departments will not have to suffer the bureaucratic nightmare of QAA again.

TERRY MANBY adds some details to our cursory reference in the December editorial to the monuments of Sledmere on the Yorkshire Wolds: 'These two monuments are separate commemorations: the cylindrical monument is the "Waggoners Reserve" the local Service Corps Unit; the Eleanor Cross was converted into a memorial for the local men of the 5th Battalion (Territorial Force) The Yorkshire Regiment — usually known as the Green Howards (Banbury 2000). All ranks commemorated are named and depicted in the brasses, the distinction is that some of the officers are portrayed as mailed knights but others are in service uniform like the NCOs and men who are depicted in great coats and steel helmet.'

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☙ We have invited JOHN BARRETT (University of Sheffield) to write a celebration of the life of PIERRE BOURDIEU from an archaeological perspective.

Pierre Bourdieu

Born 1 August 1930, died 23 January 2002

Pierre Bourdieu was born near Pau in the Hautes-Pyrenées, the son of a postal worker. In his late 20s he taught briefly in Algiers and he served in the Algerian war of independence. It was in Algeria that he began to formulate his methodology for sociological research. His preoccupation was with establishing the means of analysing the details of life, not as a purely intellectual exercise but as an obligation to the struggle of those seeking to maintain their own dignity in the face of the intellectual arrogance and the economic power of a political and cultural élite. As many commentators have observed, this placed Bourdieu in an uneasy relationship with the academic world that he had entered and through which he rose, ultimately to gain the Chair of Sociology at the Collège de France in 1981. None the less, throughout a highly productive academic career he remained committed to the development of the systematic and scientific analysis of human conditions and towards speaking and writing on behalf of the dispossessed by confronting the political and cultural mechanisms of globalization.

Out of more than 25 books, English reading archaeologists are most likely to be aware of just three; *Outline theory of practice* (1977), *Distinction: a social critique of the judgment of taste* (1984), and *The logic of practice* (1990).

Distinction traces how the French middle class mobilize their resources of economic power, judgment and taste to define the high culture to which they grant themselves access and from which others are excluded. The book's emphasis upon the production of culture as a strategy to maintain class distinctions chimes well with recent archaeological approaches towards the circulation and consumption of material resources as strategies of social exclusion and with the growing archaeological interest in modern material culture studies. The subject matter of the two volumes on *Practice* is difficult and, despite the excellence of the translation, the mode of expression is also challenging. That such work has gained a limited currency in archaeology is because it develops an intellectual apparatus to understand human agency, a theme that has been central to the early development of 'post-processual' archaeology.

It would be pleasing to report that the impact of Bourdieu's work for archaeology has been profound, but sadly this is not the case. Initially the reason may be thought to lie in the difficulty of applying a theoretical work of sociology to archaeology. This might be especially true given that it draws its empirical support from the practices and rituals of Kabyle peasants which emphasizes their embodied actions in the maintenance of such moral values as honour and respect. Closer inspection however reveals other, more interesting reasons for the lack of archaeological development.

Central to Bourdieu's programme is an investigation of the role of the observer who first defines the system or organization that they wish to study, and who then proposes an explanation for the functioning of that system, using the same terms by which it has been defined. Such procedures do little more than assert the supremacy of the observer in the first move they make by characterizing the object of their studies. It is the observer who has discovered by the definition of their object the very nature of its explanation. Challenging that position requires the construction of strong positions of self-reflexivity as components of a more rigorous scientific programme. The natural and physical sciences have long underscored this point, the importance of Bourdieu's work is to carry the case to the social sciences. Notice that if we follow Bourdieu we will not embrace the call to relativism as a consequence of a loss of faith in objectivity (the position of which 'post-processualists' can easily stand accused), but rather we must re-establish the stringent demands of objectivity through a clearer analysis of our role as observers.

In the particular case of the social sciences, consider the way the observation of a pattern of regular behaviour may be characterized, by the observer, in terms of a series of cultural rules. These rules appear to comprise a single coherent and functioning system whose logic is obeyed by the actors who are being observed. The observer then seeks the structure that is assumed to generate these rules, thus explaining how the system is carried forward through the lives of its participants. Various structural determinants are usually on offer. If we treat the cultural rules we have discovered as symbolic, then we may refer to mental structures; rules of kinship, on the other hand, are regarded as the products of social structures, while functional rules speak of structures of adaptive efficiency, and so on. Archaeologists are used to treating the material remains they excavate as the record of such behavioural regularities and a great deal of argument has taken place over the last 40 years concerning the nature of the rules these archaeologically attested regularities are deemed to represent. Consequent upon the choices made at this stage of analysis is the nature of the structural explanations that will then be offered for particular historical conditions. Academic careers have been built from the heat of these debates, where each participant claims to reveal more and more about the people that we study and whom we shall never meet.

Bourdieu renders these structural interpretations problematic by handing them back to the observer as the product of their own unexamined certainties. He reintroduces the subjects of our enquiry, not as people determined by the structural logics that we alone are able to discover, but as emerging in their negotiated relationships between themselves and others where, by drawing upon available resources and by reworking conventions, they establish their own values of dignity and honour, relationships of affinity and subservience, and their own means of objectifying and evaluating the conditions of their own lives.

There is no doubting the complexity and subtlety of the case Bourdieu makes, but its implications are startling. Processual and

post-processual positions now appear little different in the levels of power and authority each bestows upon an observer who is content to explain the meaning of the evidence each from their own perspective. Bourdieu's work calls for us to move away from the treatment of the archaeological evidence as a record requiring interpretation (the meanings of the patterns of things) to one that invites us to understand how the different material conditions we recover may have enabled different conditions of humanity to come into being.

RAY INSKEEP's commemoration of the life of DESMOND CLARK provides an important link to the theme of our next (September) editorial: advances in the study of early humanity. We had the pleasure to witness the enduring 'vitality' of Desmond Clark at the World Archaeological Congress in Cape Town, South Africa. For those who did not meet him, Ray Inskeep (University of Oxford) records his considerable achievements, his interest in a sport with a sound archaeological heritage (shared by one of the editors), and the important partnership with Betty that gave strength to his career, a partnership that the current editors also recommend.

Professor J. Desmond Clark
1916–2002

Desmond Clark's vitality of mind and body are unmatched in the annals of African archaeology, with few to match him even in the wider world. He saw the roots of his passion for archaeology in the antiquarian interests of his paternal grandfather, shared too by his father with whom he visited 'many a castle, monastery, Roman villa or ancient hill-fort'. At Monkton Combe School, near Bath, which he attended following prep-school, he found formal and sympathetic encouragement in his archaeological interests, and learned the importance of the critical approach to history which was to appear so strongly in his later career. It was here, too, that he developed a life-long love of rowing which he indulged with enthusiasm during his years at Cambridge and on the Zambezi at Livingstone until 1961, when he moved to the University of California at Berkeley. His last outing was with a crew of old Monktonians, on a visit to his old school when in his 70s.

Fortunately, he was not good at classical languages, and so was precluded from following the interest of his mid-teens into a career in Egyptology. At Cambridge, which he entered in 1934, he read history for his first two years and archaeology under Miles Burkitt and a young Grahame Clark in his final year. In his own words, 'it was Miles who gave us the enthusiasm' and 'Grahame who showed us the need for precision in archaeology'.

Clark's career began in January 1938 with his appointment as Secretary of the newly formed Rhodes–Livingstone Institute (for social anthropology) and Curator of the David Livingstone Memorial Museum, which had been merged with the Institute. The museum he inherited was a small affair, consisting of Livingstone memorabilia, a collection of early maps of Africa, mineral from the Copperbelt, sundry curiosities, and a few boxes of stone implements from the Gatti/Dart excavations at the Mumbwa Caves, all housed in an ill-lit building that, in an earlier existence, had served to house the United Services Club. His enthusiasm undoubtedly had much to do with the decision by the Trustees to build a fine new museum, of which he became Director when it was opened in 1951. He was instrumental, too, in the setting-up, in the same year, of the Northern Rhodesia National Monuments commission, of which he was *ex officio* first Secretary.

It is hard to imagine the sense of isolation he must have experienced in an environment so very different in every way from that in which he had grown up, with his nearest colleagues hundreds of miles away in Nairobi, Bulawayo and Cape Town: a sense of isolation ameliorated only because it was shared with his wife, Betty, the fellow undergraduate he met at Cambridge and whom he married in 1938. She was to be not only his life-long companion but, as he put it in a Retrospect in this journal in 1986, 'What I have been able to do in archaeology has been essentially a team effort by the two of us and, if it had not been for her input, it would not have been possible to do half of what we have managed to do between us'. The visible part of this partnership is to be seen in the fine stone implement drawings which Betty contributed to virtually all his publications, from Mumbwa in 1939 to Kalambo Falls in 2001.

With little or no funding he turned without delay to the task of establishing for Northern Rhodesia (Zambia) the kind of succession of Stone Age cultures that Goodwin and Van Riet

Lowe had established for South Africa in 1929. Initially most of the fieldwork was done in the vicinity of Livingstone on foot or with his own 'rickety transport', aided by a grant of £15 which enabled him in 1939 to re-excavate the Mumbwa Caves. After the war, in 1948 and 1949, the work was extended to sites in the Northern and Central Provinces and in 1950 he published the results in *The Stone Age cultures of Northern Rhodesia* (*SACNR*), a work that remained a standard reference for many years.

The intervening war years took him to Somalia and Ethiopia as a sergeant with the 7th East African Field Ambulance; a service of his own choice, as he was opposed in principle to the taking of life. During lulls in the fighting he was able 'to work out the archaeology recorded in the numerous exposures of Quaternary sediments in Somaliland and some parts of Ethiopia'. After the cessation of hostilities in 1941, and a brief spell in Madagascar, he returned with a commission to the Somali Scouts, fitting in more archaeology, facilitated still further by his transfer to the British Military Administration as a Civil Affairs Officer. This remarkable patchwork of opportunities was brought together in another ground-breaking volume, *The prehistoric cultures of the Horn of Africa* (*PCHA*), published by Cambridge University Press in 1954. These first two impressive volumes not only achieved their objective of establishing the Stone Age cultural succession for the two regions, they determined the general field of Clark's interest for the rest of his career: he was, preeminently, a student and elucidator of Africa's Stone Age past. The absence of radiocarbon dating for the first decade of his career in Northern Rhodesia (Zambia) meant that there was, effectively, no way of knowing if the pottery scatters marking the sites of abandoned villages were 50, 500 or 1000 years old, and whilst geological strata (river terraces and wind-blown sands) provided at least a coarse-grained key to succession for the Stone Age, there were, in Zambia, no obvious equivalents to attract one to the unravelling of the prehistoric past of African farmers and pastoralists.

This does not mean that Clark was disinterested in that past. Twelve pages of his earliest excavation report (Mumbwa Caves, in 1939, published in 1942) were devoted to 'The Iron Age culture', and pottery was compared with material from a number of sites south of the Zambezi as well as with modern Soli pottery. He described and illustrated pottery from at least two sites in Somaliland (in *PCHA*) and similarly for Northern Rhodesia (*SACNR*) where he also included notes on Bantu tribal tradition in his discussion of the Upper Northern Rhodesia Wilton, and on Bantu tribal legends in relation to the Nachikufan. Whilst undoubtedly firmly committed to the Stone Age his interests were far from narrow, and he published a number of important papers in other fields: on 'The pre-Bantu inhabitants of Northern Rhodesia' (1950), 'Pre-European copper working in south Central Africa' (1957), 'River craft and fishing practices in south East Africa' (1960), 'Charcoals, sands, and decorated pottery from Northern Rhodesia' (1965) and on the rock paintings of Northern Rhodesia and Malawi in 1959. When given an additional post of prehistorian at the museum in 1957 he specified the Iron Age as a required field of research which, under successive holders of the post, has led to a rich understanding of Zambia's more recent prehistory.

From the outset he was keenly aware of the potential of palaeoenvironmental evidence as a key to a better understanding of the role environment played in subsistence and the ways in which this might be reflected in the products of material culture; an interest developed in several papers arising from exploration of the Kalambo Falls record. The appendix on charcoals from three sites in Somaliland (*PCHA*) is probably the earliest attempt in Africa to use charcoals for this purpose.

The discovery (1953) and subsequent excavation (1953–1988) of the Kalambo Falls site was probably the high point of his career. He had long been impressed with Mary Leakey's excavation of Acheulian living surfaces at Olorgesailie in the 1940s, and saw here the opportunity for similar studies, with the added bonus of well-preserved wood and other plant remains. The excavation itself was a triumph of organization and determination, for it involved removal of sediments on a scale previously unmatched, and through a complex sequence of deposits containing cultural material of all the major stage from Acheulian to Early Iron Age. Radiocarbon dates provided a chronological framework for a major part of the sequence while, more recently, amino acid racemization has provided dating for at least part of the Acheulian. The task of orchestrat-

ing the third, and final, (700-page) volume, dealing with the Early and Middle Stone Age levels, and with no fewer than 14 contributors, was published only six months before his untimely death. It is a fitting monument to his powerful mind and tireless energy.

From the outset of his work in Africa he sought the collaboration of geologists and human and animal palaeontologists, and saw the early Pan African Congresses on Prehistory and Quaternary Studies as playing a seminal role in the development of African archaeology, both by bringing together archaeologists and natural scientists, and by breaking down geographical barriers and facilitating freer exchange of information between the diverse regions of the continent. He attended every one of the conferences during his lifetime, and regretted that, with the passage of time, the meetings became more and more archaeological, partly because the growth of specialist conferences tended to focus the attention of geologists and palaeontologists elsewhere.

Although he wrote in 1986 that most of his fieldwork had been carried out in Zambia he made field trips, at the invitation of the government of the day, to Malawi, in 1950, 1968 and 1972, and at the invitation of the Diamang Diamond Company of Angola carried out several seasons of fieldwork on the Pleistocene sediments of the northeastern part of the country, building on the earlier work of Janmart and Louis Leakey, in three substantial monographs. After taking up his post of Professor at the University of California at Berkeley his interests broadened to north and east Africa, and beyond. These new interests took him in the early 1960s to Syria to excavate an Acheulian site at Latamne, to the Central Sahara, the Nile Valley, and the Sudan, resulting in a flurry of papers on early agriculture in those regions. From 1974 he was heavily involved with the Early Man/Africa Program at Berkeley and, more recently, until his death, the on-going Middle Awash research project. 1980 and 1982 saw him working with Indian colleagues on an Upper Palaeolithic site in Madhya Pradesh and, on the opposite side of the continent, studying bead-making at Cambay, and the mining of the semi-precious stones used in their manufacture. In 1989 and 1990 he made two trips at the invitation of Chinese colleagues to participate in the investigation of Plio-Pleistocene deposits in the Nihewan Basin, and even found time for a visit to New Guinea to study stone axe manufacture. In October 1999 he delivered what was probably his last public lecture by invitation to the Faculty of Letters and Philosophy in the University of Rome 'La Sapienza', followed by a few days with the family in England before flying, once again, to Addis Ababa to confer with colleagues on finds from the Awash Valley.

In the 64 years of his active career he published well over 100 journal papers, book chapters and distinguished lectures, wrote nine books or monographs and was editor or co-editor of nine others. He was elected Fellow of the Society of Antiquaries of London in 1952, appointed CBE in 1960, and made a Fellow of the British Academy in 1961. He was a Gold Medallist of the Society of Antiquaries of London (1985) and of the Archaeological Institute of America (1989), and received the Grahame Clark Medal for Prehistory of the British Academy in 1997. He was a Fellow of the American Academy of Arts and Sciences, and of the national Academy of Sciences (USA). In 1975 he was awarded his ScD by the University of Cambridge and received Honorary Doctorates from the University of Cape Town and the University of the Witwatersrand. But above all he was admired and loved, for his great scholarship, kindness and gentle humour, by the numerous students he taught and colleagues with who he worked. His presence will be greatly missed, above all by his wife Betty, and his son and daughter.

11 April 2002

As a footnote to the above memoir of Professor Clark we have to report the sad news that his wife, Betty, died peacefully at the home of their son, in Kent, on 14 April. As we have recorded above, Desmond had acknowledged in general terms the role played by Betty in his professional life. As is the case with the wives of many professional men, the role that Betty filled was often unobtrusive. Playing host to the endless stream of friends and colleagues who made their way to her door was something not done without effort but, whatever the circumstances, that effort was never allowed to show, though the warmth with which Desmond seemed to invite all and sundry to join them for a meal, or to stay for the night, must surely have taxed her patience from time to time. Often such occasions were more in the nature of professional

Granite warrior-statue. Lezenho, Vila Real, Portugal. Ht = 2·06 m. (Photo Museu Nacional de Arqueologia, Lisbon.)

debates than social occasions; and were important moments for the exchange of information and the laying of plans. But there were other, more professional involvements. When Desmond was called away for military service during the war, it was Betty who took over the task of keeping the museum going until his return, and then served as Museum Secretary until their departure for Berkeley in 1961. In drawing, with such skill, the stone artefacts for so many of Desmond's publications Betty was following the example of two other great lady illustrators, Peggy Burkitt and Mary Leakey. She was one of the raporteurs for the 1965 Wenner Gren symposium, published as *Background to evolution in Africa* (1967), whose daily attention to recording and typing out discussions contributed so much to the success of that event; she also assisted with the editing of the volume. When circumstances permitted, she accompanied Desmond into the field, taking charge of the commissariat at Kalambo, and assisting in the analysis of tables for publication in 1971–72. She was a loyal, though not always enchanted fieldworker and once asked the writer, with some feeling, why it was that 'all the best sites have to be in the most out of the way and uncomfortable places'. Her role in her husband's professional life was, indeed, a devoted and active one. The double loss of Desmond and Betty, so close together, must leave great many people with a deep sense of sadness, lightened only by the joy of having known them.

RAY INSKEEP
15 April 2002

Part of the 1st-century BC Winchester hoard. (Photo © Trustees of the British Museum.)

https://doi.org/10.1017/S0003598X00090268 Published online by Cambridge University Press