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PARALLELS AND CONTRASTS IN THE SETTLEMENT PATTERNS OF PREHISTORIC GREECE

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ABSTRACT This paper revisits two of Oliver Dickinson's older but prescient articles on Bronze Age settlement patterns in Greece in the light of the accumulating data from intensive survey, and the current rethinking of intensive survey results.

KEYWORDS Greece, Bronze Age, (methodology of) survey, settlement patterns, regional diversity.

Oliver's publications are characterised by careful thought and an unparalleled knowledge of the material. The gestation of his already classic survey of the Aegean Bronze Age (Dickinson 1994) was very long, but it remains central to our discussions ten years later. Thus, when Oliver put pen to paper on the question of the evolution of prehistoric settlement patterns on the Greek Bronze Age mainland, on two significant occasions (Dickinson 1981–1982; 1982), the comments he made have provided fodder for scholars to ponder on well into the future. And it is these two articles I should like to use as my starting point for a critical discussion of our knowledge of the later prehistoric landscape of Greece. In the first paper, Oliver asked what we knew and what we needed to know about the mainland Bronze Age. In the second paper, Oliver challenged the use of a 'uniformitarian' approach to mainland Greece, especially as regards the criticisms raised by proponents of intensive archaeological survey (particularly me!), against the reconstruction of prehistoric settlement patterns in Messenia by the University of Minnesota Messenia Expedition team of the 1960s (McDonald & Rapp 1972). What I want to do in this homage to Oliver is to combine these two issues into a focus on the questions: what do we know about prehistoric Greek settlement patterns and what do we need to know? This requires us to delve in a way into the history of Greek landscape archaeology, both data now up to 40 years old and data fresh from current field projects.

In their monumental regional survey publication of 1972, McDonald, Hope Simpson and their team used extensive survey information from a large region (some 3800 km²) to project a model for the evolution of Bronze Age settlement patterns: Neolithic (first farmer) sites were very rare, then with the EBA occurred a proliferation of settlements; in MBA times, site numbers increased further, to reach an even greater climax with the LBA (Mycenaean) civilisation. As the last period was the central focus of interest for the University of Minnesota Messenia Expedition project, this evolutionary trajectory was a very logical one. In the same year, 1972, that the Messenia synthesis volume appeared, an equally major book of synthesis was published by Colin Renfrew—*The emergence of civilization* (Renfrew 1972A). In the latter publication and in another, detailed study of patterns of population growth in the prehistoric Aegean published in the same year (Renfrew 1972B), Renfrew compared the continuous growth curve for Messenia with site numbers in other regions of Greece, again using extensive topographic survey results. Most other regions had a contrasting, double-wave pattern: low Neolithic, high EBA, low MBA, then high LBA again. Renfrew considered these patterns (dubbed A and B) to indicate potentially contrasted regional growth trajectories.

However, by the late 1970s intensive field surveys had begun in Greece. Their field-by-field methodology was very divergent from the far patchier cover of the landscape practised by the preceding extensive approach. Not only did intensive surveys, hardly

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surprisingly, locate more sites in a given area (Cherry 1983). Their attention to the individual field and even small scatters of sherds, elevated to great prominence a class of small rural sites—farms and minor hamlets—which were likely to have been missed by the tell-, village- and acropolis-focused search of the previous tradition of extensive surveys. Was it not probable that the Messenia survey had systematically missed large numbers of small sites and overemphasised the proportion of larger hamlets, villages and towns (Bintliff 1977B; 1977C)? Since the EBA was dominated by such sites in more intensely-surveyed regions, adding large numbers to Messenia would have the effect of bringing its settlement evolution into line with most other southern mainland areas so far surveyed, with a double wave of growth and decline. A second point of critique focused on the way that we might interpret the more widespread pattern of site number decline in the MBA between plentiful EBA and LBA sites (Bintliff 1977C): could a tendency to greater nucleation of settlement be as important a factor, as simple population decline?

Involved as he was during the 1980s and 1990s with both older projects such as Messenia, and younger, such as the 'New Wave' intensive surveys (Bintliff 1992), Oliver could see the strengths and weaknesses of both sides in this debate. In his two articles, he welcomed the increasing detail that field-by-field survey brought for local settlement developments, but asked why we should expect that every region of mainland Greece would have had the same density of settlements for each phase of prehistory and the same trajectory of landscape history. This was certainly ahead of its time, and indeed as more and more intensive surveys appear, his critique has been justified; indeed it is now abundantly apparent that sequences in each region have local peculiarities and even notable contrasts with each other. Oliver already was able to point out that the supposed signs of an emergent élite—MH tumuli and early LH tholoi—could not be given a blanket status value regardless of region where they occurred, as marking a uniform wave of rising social stratification over those periods. Rather, Messenia was precocious in its deployment of such burial forms, and other regions never achieved such numbers of complex tombs, or perhaps reserved them for a much narrower class of people than in Messenia (inspiring more recent studies such as Voutsaki 1998).

The renewal of survey in Messenia from the late 1990s in the guise of the Pylos Regional Archaeological Project (Davis *et al.* 1997; Davis 1998) was in many respects designed to remedy what appeared to be deficiencies, or perhaps to be fairer (and all of my generation were in retrospect pretty ungenerous to the achievement of the University of Minnesota Messenia

Expedition!), 'unfinished business' left over from the 1960s Messenia Project. It was anticipated that intensive survey would fill in the landscape with very numerous small sites for both prehistoric and ancient times. In actuality, to the surprise of everyone, Oliver's warning appeared here also to have been justified: although the density of small sites did increase significantly, it was nowhere near as great a rise as predicted from the survey of other mainland regions, and did indeed suggest that Messenia's settlement pattern did not conform to that of most other areas. The University of Minnesota Messenia Expedition's development scenario was not so far off the truth, it seemed!

However, on this point I would rather radically argue that the University of Minnesota Messenia Expedition, Oliver and even the Pylos Regional Archaeological Project are probably wrong. The detailed arguments I have presented elsewhere (Bintliff *et al.* 1999), but these develop from a further stage of archaeological survey theory, which I believe we are in the early stages of. If intensive survey brought greater spatial and quantitative resolution to landscape history, it still tended to treat the presence of numerous sherds of a given period as equivalent to a settlement site, saw the presence of a few sherds as casual activity of no real significance, and rarely plotted the spread of the material for each phase across the surface of settlement sites. There usually were far more 'dots on the map' as a result of these New Wave surveys, but internal variation between official 'sites', and the origins of the 'non-site' thinner scatters of sherds remained little researched. Moreover, often the recording techniques deployed made it impossible later to investigate these problem-areas from the data collected.

Based on more than 25 years of intensive survey in Boeotia, our team has increasingly sharpened its comprehension of these last two phenomena. Since 1988, we have argued that, in some regions of Greece and in certain periods of the past, agricultural manuring was carried out from urban centres on a vast scale, leaving remarkable amounts of worn sherds across the landscape. More frequently and perhaps rather generally, rural settlements are associated with 'infield' zones or 'site haloes', i.e. areas of landscape immediately beyond site borders with intermediate sherd densities between site level and regional off-site levels, where we suggest varied activities took place: rubbish disposal, estate-originating manuring and dispersal of site material by erosion and ploughing (on all this, see Bintliff & Snodgrass 1988; Snodgrass 1994; *contra* Alcock *et al.* 1994).

The task of recognising an archaeological site from broken sherds on the surface immediately becomes much more complex than previously imagined,

wherever and whenever such past behaviours have been at work. But even where they are less common than in Boeotia or even absent, our work suggests that considerable refinement is still needed to clarify what surface sherds of period 'x' could signify when located during field-walking. Sites need to be gridded on a systematic basis, even where we are sure that densities place the locality well above the levels expected from manuring or site haloes. The reasons are also part of our new interest in the *Quellenkritik* of intensive survey. Let us, for instance, imagine a spot in the landscape where a small farm was set in prehistory, then abandoned, only to be returned to generations later (perhaps being reestablished in a slightly different part of the locality). Let us envisage this sequence repeated many times over a thousand years or so. Then in historic times, our locality witnesses a hamlet; then a farm; then a shed used by people from a nearby village to store field equipment and have meals at, during harvest or tree-pruning; then the site returns to use as a residential farm or hamlet. This kind of complex life-story, or the 'cultural biography' of the locality, is far from fanciful. Indeed very many survey sites betray signs of significant human presence in diverse forms and from multiple periods of the past in their surface ceramic and lithic debris. However, till recently, even intensive survey and almost all extensive survey teams collected a sample of surface finds from sites without complete gridding; these samples could be rather small in number. It was generally assumed that any period with more than a handful of finds was occupation, that the handful or less was some sporadic and hence not important site use, whilst the maximal overall extent of the site could be generalised to all its periods of occupation, unless there was striking imbalance in the spread of finds of a particular period.

We now can see how hard it is to detect the imaginary site scenario outlined above with data collected in these ways. Firstly, it is widely agreed that prehistoric sites reoccupied in historic times will have impoverished prehistoric surface assemblages (Bintliff 1985; Cherry *et al.* 1991A: 222–23, fig. 9.7); the same goes for historic periods overlain by major subsequent occupation. Therefore, simple numbers do not equate in a direct way with levels of activity on multi-period sites. It is also becoming likely that settlements with a long tradition of mudbrick architecture will see brought to the surface far older occupation debris through recycling of site deposits, than those with houses featuring a stone and wood superstructure. Let us now add the problem of poor spatial control of our surface finds. We do not usually possess a map of finds for each period across the site surface, to see if the site area was occupied selectively in different phases of its use (surely rather more than less likely).

Sample size creates additional problems. As the surface representation of a period declines, it is increasingly less likely to be picked up in a small collection. Even if it *was* included in the surface sample collection through a unique appearance of a few finds, the chances that those pieces would indicate the full extent of that period over the locality are very low. The edges of surface sites are indeed normally defined by the period of widest use. It takes specific strategies, rarely employed, to tease apart the individual contributions of other periods present, according to their varied distributions. If we then add the question of site haloes—also widening and narrowing according to a variable site edge over time—then I think we can appreciate how really complex, surface sites have become in our understanding of them and how hard it remains to design a field methodology to deal with these innumerable variables. After more than 20 years of collecting data and making provisional (and now I see very naïve) interpretations of them, in Boeotia we are only now in possession of a first version of a rigorous approach to interpretation, which has been designed to try and tackle the most serious problems facing us with multi-period sites in this region (Bintliff & Howard 1999).

Going back to the history of survey, when we look back to Oliver's paper (Dickinson 1982), that compares the Bronze Age *Gazetteer* with the first results of intensive survey on mainland Greece and raises the difficulties with bringing these datasets into debate with each other, we can now see that we are at a similar watershed in trying to bring together the published intensive survey results with the implications and first results from the ongoing *Quellenkritik* of intensive survey itself.

To take just one example: I have argued that much of mainland Greece in the Bronze Age utilised pottery which was predominantly coarse, such that once it enters the ploughsoil it is prone to progressive destruction. This implies two things. Firstly, that evidence for prehistoric manuring on the mainland is unlikely to survive till today, because the pottery within such rubbish would have been applied into the ploughsoil and by now be reduced to negligible or no presence in the surface assemblage. Secondly, where typical prehistoric coarsewares are to be found, they probably emanate from a protected taphonomic context: either a sediment only recently disturbed, or most likely a site deposit now being ploughed up. Given the fragile character of most material of this age, what we would see now in the ploughsoil would remain reasonably plentiful if emanating from a village with significant deposits still in the process of being destroyed by cultivation, but a mere handful or

even just one or two sherds would commonly represent a prehistoric farm (Bintliff *et al.* 1999).

Were we to focus on the small farm sites, it remains the case that in hardly any period do we have sufficient chronological resolution from the sherds recoverable to allow us to say that a series of such sites must be contemporary to each other, since most material even in historic times tends to be assignable merely to phases of several hundreds of years. The occurrence of some fine-dated pieces merely allows one to say that the site was certainly in use at that narrower time-slot, but does not delimit the start and end of occupation. John Cherry many years ago identified this critical limit to our control over small farms of 'family type' in a paper discussing EBA small sites on the island of Melos (Cherry 1979), where it was equally arguable that all such sites were sequent rather than contemporary, or in variable use at any one time.

It is worth summarising the likely conclusions from these considerations:

- Most surveys (all the extensive and the majority of the older intensive ones) lack the resolution of data collection and recording to allow us to define site size per prehistoric phase or the functional character of site use in each prehistoric phase.
- Where prehistoric assemblages are typified by coarsewares, smaller sites may be either invisible to survey or produce such low densities of finds that they are usually (but not always) relegated to non-site status.
- Conversely, larger prehistoric sites, and/or those without multiple historic reuse, will stand out as 'characteristic' for their period.
- Even larger prehistoric sites could be the result of lateral settlement drift or multiple minor use over long periods. They should not always be assumed to be coherent areas, used to the same degree throughout the periods that their sherds point to.
- Small rural sites, even where identified, will usually lack fine dating to allow us to calculate how many were contemporary with each other.

What our survey data for Greece in later prehistory can already be used for, is to give a general impression of the dispersal of human activity for each period, across the wider landscape in the most general sense, e.g. exploitation of marginal land, high upland, coastal use. We cannot, I suggest, deploy these data in general to evaluate the balance of farms versus villages and towns in each phase, or document phenomena such as site shrinkage or growth from phase to phase with any

confidence, or to ask if extensive sites are the product of contemporary habitation or horizontal movement around a settlement locality. It follows from this, that population reconstructions for later Greek prehistory, such as those attempted by the Argolid Project (van Andel *et al.* 1995), are premature and of doubtful validity. However, exceptional aspects where we can already work with our data towards such questions can be identified:

- There are ceramic traditions where the material is finely made and very diagnostic, and here it is possible to recover a much higher proportion of even small sites and to suggest likely contemporaneity of such sites. The recent survey of Kythera by Cyprian Broodbank's team (cf. Bevan 2002) has exploited this potential because of the very different Minoan pottery assemblages found on the island in the MBA and later Bronze Age (and I think predictably, therefore, Minoan manuring off-site pottery has been claimed on Crete [Palmer 1995]).
- If one accepts the argument that traditions dominated by coarsewares will leave polarised surface traces of reasonable spreads of sherds over village sites and a handful or less for farm and hamlet sites, then it is possible to try and 'reconstruct' the nature of the total settlement pattern at least, by obsessive attention to the quantities and dates of all on- and off-site ceramics. Even one or two prehistoric sherds may be the clue to a vestigial prehistoric small site. This requires us in field-walking, to revisit the locality where these occurred, where hopefully more material can be found by 'hoovering' and inspection (in our experience, perhaps even lying flat on the ground to allow the material to become more visible to the eye!). This method has produced exciting new prehistoric landscapes in Boeotia for our Tanagra Project (Bintliff *et al.* 2002) and has been developed independently for the similarly problematic later Bronze Age and Iron Age landscapes of Italy, where the characteristic Impasto Wares provoke the same low visibility to invisibility of smaller surface sites (Attema *et al.* 1999–2000). Even for older surveys, where low scatters of prehistoric sherds have been recorded, but then assigned to doubtful site status or off-site activity, these can tentatively be revaluated as concealing 'hidden' small sites (Bintliff *et al.* 1999).

How does this all affect the test-case of the Messenia prehistoric settlement record? The recent intensive resurvey of a district within the original survey region of the University of Minnesota Messenia Expedition by the Pylos Regional Archaeological Project, namely

that immediately around the Mycenaean palace centre of Pylos, as noted earlier, failed to elevate small sites to the level that the critics of the University of Minnesota Messenia Expedition had expected, although there was still a significant increase in their representation. Our own commentary on this result (cf. Bintliff *et al.* 1999) raised doubts about the data being used and the comparison being made. Firstly, the area chosen for resurvey had been the focus of intensive landscape research since the 1930s, when the palace was rediscovered, so that the level of new site recognition should be well below the effect of a survey in most other districts of Messenia (and one can show this effect on site density statistics even before the University of Minnesota Messenia Expedition also turned especial attention to this district in the 1960s). Secondly, the Pylos Regional Archaeological Project decided not to collect coarsewares in field-walking, which to my mind is the chief way retrospectively (i.e. in the pottery lab) to pick up those 'twos and threes' of prehistoric sherds from a locality, which suggest a targeted revisit. The off-site maps and occurrences of small numbers of prehistoric sherds on sites of later date in the Pylos Regional Archaeological Project archives do seem to indicate to me that there may be a 'hidden landscape' of small later prehistoric sites, which is systematically being missed or misinterpreted even by this otherwise state-of-the-art survey team.

In passing, the phenomenon of the 'historic-site window' is worth a brief comment here. We have argued that vestigial small prehistoric sites in densely occupied historic landscapes are most likely to become 'visible' to surveyors when a very visible historic rural site is found, with its big and well-made tiles and amphora sherds. Amidst the dominant collection of historic pots there tend to be found, usually at the stage of post-sampling laboratory analysis, small numbers or just one to two prehistoric sherds, which are then often written off as off-site. We would suggest that for the periods when coarsewares dominate, especially the mainland Bronze Age in many regions, these tiny scraps of evidence are vestigial small settlements or burial sites, whose presence is signalled primarily when researchers look and collect with unusual care at the surface of known historic sites. Even those critical of these radical proposals are ready to admit some startling evidence, which can be used to support them: thus John Cherry and Jack Davis recently mentioned in this respect that some 40% of the rural sites in the Nemea Project, mostly located due to historic surface materials, have revealed some prehistoric surface materials (Cherry & Davis 2001: 153).²

² Davis' most recent attempt (2004) to address our critique and defend the reliability of prehistoric site distributions neither convinces us nor does it follow our arguments and those of others in the debate with much accuracy.

In conclusion, then, it remains to be shown that Messenia really lacks the pattern of innumerable small sites in the EBA and LBA. On the other hand, Oliver's other point (Dickinson 1981–1982) that the *details* of settlement trajectories may well diverge for historical reasons from region to region, is more and more clear from the overall character of our regional survey data from Greece. Indeed, although Oliver's main target in one of the two papers (Dickinson 1982) was myself, I have sufficiently had to shift my position on this issue to the point of publishing a long article on regional variability in settlement histories from the survey data for the Classical Greek and Roman eras in the Aegean (Bintliff 1997; cf. the earlier review with a similar approach for the prehistoric Aegean, Halstead 1994). On the other hand, I do not think that historical variability need imply a purely relativistic approach to the regions of the Aegean. To say that pathways to civilisation in the Bronze Age Aegean vary in nature from region to region, does not necessarily imply that we cannot look for comparable processes operating at different timescales or out of phase chronologically with each other. If we agree with Oliver, that Messenia is a precocious area of build-up of site numbers and social stratification in the later MBA and early LBA, whilst the Nemea Valley at the other end of the Peloponnese only seems to take off in mature LBA times, perhaps under direct stimulus from the powerful centre at nearby Mycenae (Cherry & Davis 2001), the underlying picture is nonetheless, for both regions, one of land intake, population growth, increasing social complexity and the creation of, layer after layer, a settlement hierarchy. When we see these processes operating out of phase to each other, but culminating in a common climax—here in the florescence of palatial society in LH III—surely this is a most exciting nuancing to our settlement analysis, suggesting that we are seeing the evidence for a sequence of linked historic events and developments. The same can be said for Oliver's point—even clearer now than when he made it (Dickinson 1982)—that status burial forms in the MBA and LBA vary in meaning and popularity from region to region of mainland Greece. This is a powerful way to shed light on the development of social symbolism and regional élite structures, by allowing us to detect differences in the rise of individual polities and their internal organisation.

With some notable exceptions (e.g. Harding 1984), Aegean Bronze Age studies tend to be rather cut off from the discourse of European prehistorians as a whole, and I suspect the chief reason lies in the origin of many of its scholars in Classical or Near Eastern studies. Because my own faculty includes a very active group of prehistorians of north-western Europe and since we have good networking with similar specialists in other countries of western Europe, I have the good

fortune to be able to follow current trends in method and theory in Neolithic and Bronze Age studies elsewhere than in the Aegean. Something worthy of attention for the Aegean is the ability that north-western European prehistorians have achieved, in several regions (the Netherlands during the Bronze and Iron Ages, the Rhineland during the Neolithic), of following the evolution of a settled landscape on a generation-by-generation basis, utilising combinations of horizontal stratigraphy, refined typologies and multiple radiocarbon datings. The way individual families or groups of families construct and modify their dwellings, settlements and field systems can now be followed in all their individuality. I am sure that these advances will eventually be achieved also for all the regional landscapes of Greece (and for a case-study where this has almost been possible already, see the Kythera Project cited earlier). As we work towards that goal we will require an overarching theoretical model to allow us to see the wood *and* the individual trees, so that Oliver's prediction (Dickinson 1981–1982) of variable pathways of social development in Bronze Age Greece can be extended, to allow for the individual agents of change and also for the bigger picture of civilisational rise and fall.

More than ten years ago I suggested that the Structural History of the French *Annales* School of historians offered exactly the approach to accommodate the different timescales and spatial scales opening up before us, with the continual refinement of our data for the later prehistory of Greece (Bintliff 1991B; revisited and expanded in Bintliff 2004B). The past is never the simple result of one human agent or faceless historical forces, but is an unpredictable (but postdictable) field of interaction between individuals and groups, local and wider communities, natural conditions from the physical environment, short-, medium- and long-term processes, and effects resulting from human planning and from uncognised human actions. As Oliver reminded us, the trajectory of the past is a unique and locally conditioned one. Structural History is designed to allow a landscape to evolve with inputs from all these forms of interaction. What we need to know is how to obtain the most accurate data to pour into our Structural History framework, so as to let it rerun the course of the past for us. I believe that the current *Quellenkritik* in Aegean landscape history will allow us to enter this exciting new stage in our understanding of past societies.

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