

5. Rejoinder

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We are grateful to *JMA*'s Editors for encouraging a fruitful debate on our paper, and to the commentators for their critical, generally constructive and supportive views.

Barker's piece raises the issue of variation between regions in terms of prehistoric 'visibility' as a product of different land-use histories, which can be related to Mee and Cavanagh's observation that the fragility of prehistoric ceramics can also vary regionally. These points we are happy to accept, since our model sets out to explain the likely effects of surface processes and prior taphonomic effects on certain kinds of pottery which are very common in many areas of European prehistoric culture, and wherever long periods of continuous heavy cultivation have been at work. The degree to which our model relates to the regional prehistoric surface record is a reflection of the importance locally of these factors. We are not quite sure, however, that Barker's proposition can hold—namely, that small collections of prehistoric pottery (even just one or two sherds) could equally well be either off-site material or a vestigial site; our paper argued that the more fragile kinds of pottery, if deposited originally onto the topsoil, are very unlikely to survive until today, and should normally be present within recently exposed subsoil features (which, generally, will indicate 'sites'). Very correctly, Barker goes on to introduce the loss of palaeosols through burial or erosion, which will certainly block the visibility of prehistoric activity traces, whether on- or off-site. Here, as several commentators note, geomorphological assistance is invaluable, although we doubt whether it is feasible to create a landscape reconstruction map by period for every transect walked. Effectively, geomorphic factors

will tend to exacerbate the relative invisibility of prehistoric surface traces.

Mee and Cavanagh provide welcome support for our approach and also correctly underline the likely regional diversity of the key factors we argue to be strongly dominant in Boeotian surface assemblages.

Schon's contribution raises important criticisms, but we consider that some of these are based on a misapprehension as to the aims of the analysis and reconstructions in our paper. The point we must emphasize is that this is the detailed analysis of a survey carried out 10 years ago, and which will not be repeated in the same locality. Our intention is to interrogate the data so as to reveal what we missed and what we recovered in an extremely fragmentary state, in terms of the likely original complement of sites and activity areas across this sector of landscape. We concluded that our recovery of prehistoric ceramic and lithic finds, and of prehistoric sites, was severely limited by systematic taphonomic and methodological factors; as a result, we have a palimpsest whose spatial representation is a product not of original activity, but of (1) serendipitous observation during fieldwalking, and (2) the 'window effect' introduced by intense scanning of the surfaces of historic sites. It is therefore far from our intention to suggest that we can reconstruct the location of all the 'missed sites' from our palimpsest. Instead we use 'guesstimates' to give an impression both of the likely minimal scale of the prehistoric ceramic and lithic finds *not* recorded, and, from the specific characteristics of the sample we *did* recover, to make reasonable hypotheses about the general nature of the original distributions of these two cate-

gories of finds. Through comparison with similar exercises in other parts of Europe, we also speculated about the possible socio-economic significance of these reconstructed distributions, suggesting further testing.

We are rather at a loss to understand Schon's refusal to support extrapolation. Of course, our paper is very much aimed at current and future surveys to try and improve recovery from the start, so as to deal with the biases revealed in older data such as ours; but it is hardly constructive to suggest that we stay with the raw data from Boeotia, when all our inferences cry out for attempts at scale correction to comprehend the nature of the full database. Schon also fails to acknowledge the elaborate arguments introduced to justify our 'hidden landscape' model. For example, he states that 'there is no empirical basis for this upgrading' of small scatters of coarse prehistoric ceramics to site status; this ignores the case-studies we cited from our own and other projects, where further intense study of such scatters has undeniably shown the presence of vestigial sites, as well as the poor surface survival properties of coarse wares in continuously ploughed landscapes. We also must explain again that an easily discovered prehistoric site such as Onchestos exhibits the typical properties of a larger and very long-lived site—in contrast to the short-lived, smaller prehistoric sites which were the focus of our paper.

We think it very unrealistic to imagine that, even in the most intensive survey, every local relevant variable in surface exposures for every transect can be identified: not only would vast amount of time and money be required, but also the very processes of progressive transformation of the ploughsoil and its subsurface make older evidence vestigial, and thus only comprehensible through controlled extrapolation. Even complete excavation of the entire surface of every transect

would thus still produce a palimpsest (if now in three dimensions). We are arguing for general reconstructions of the scale and broad spatial characteristics of prehistoric surface activity as the best feasible goal for intensive survey. A transect-by-transect geomorphological map is certainly not going to offer a quantitative guide to the original complement of artefacts in a transect and to their historical movement and diminution in four dimensions. Every excavator knows that complete clearance to bedrock for prehistoric sites always produces vestigial evidence; the ploughsoil is just one sector of such a larger space of taphonomic processes.

Finally, Schon cites unpublished experimental work on visibility correction: we look forward to evaluating this evidence, but cannot as yet comment on the strength of the argument. In any case, the necessity of significant visibility correction to raw surface artefact counts (which we think we pioneered in the Mediterranean during the early 1980s in Boeotia) is generally acknowledged by most current intensive surveys. We also have evidence to suggest that whereas the directly proportional correction of artefact counts in transects with 40-100% visibility seems to provide consistent results, visibilities less than 40% become increasingly difficult to correct accurately, not least because of the wide variance in small number statistics. Since the typical visibility in the THS/LSE sector presented here was around 40-60%, we doubt that new work will substantially alter the scale of corrections we have applied.

Thompson's comments also miss some of our argumentation. Thus he suggests that the fact that we reconstructed prehistoric site use from a small component at three historic sites with more plentiful later ceramics implies that we can identify the original prehistoric site complement. Our point was very different: these pieces of landscape only revealed

prehistoric scatters because they were gridded following the recognition of 'sites' with historic pottery. The 'window effect' warns us that, outside such intensive grids, we are unlikely to spot such tiny scatters of finds. Thompson states that future surveys must be far more rigorous—and this indeed is the point of our paper, from retrospective study of an old survey. Yet it is surely too negative to criticize our essential attempts to delineate the types of bias in that old survey and to try to give the general outlines of the larger database our small samples reflect for prehistory. As with Schon, the claim that our model cannot be verified ignores the evidence of the case-studies we presented, both our own and those of others concerned with this problem. Of course, all we can do is hypothesize with correction factors for Boeotia, and ask that new surveys modify procedures to obtain better data, but the accumulated evidence we cite does point consistently to the overall likelihood that our model has widespread—if not necessarily universal—application in many regions of Europe.

Thompson is indeed rather too hard on us, by suggesting that we think our model is universally applicable in southern Greece. In fact we believe that it is relevant wherever similar pottery and taphonomic processes are at work, and we introduced some other case-studies to test this proposition. Our evidence supports that claim, as does additional information cited by Barker, and by Mee and Cavanagh, in their comments. We are not sure how useful it is to question whether the model works everywhere: it is a carefully argued hypothesis for testing elsewhere, and cross-cultural parallels certainly suggest a wider applicability than the 2 km radius of ancient Thespieae alone. A technical point on urban interference: although we argue that most of the Classical pottery in the rural area studied here did in fact originate from the city

(as manuring waste), the estimated survival rate of fragile prehistoric ceramics would mean that older sherds brought onto the fields at the same time would not survive today in the ploughsoil—so that the prehistoric surface sherds we did find should essentially reflect recently ploughed subsoil prehistoric features. If Thompson is suggesting that the large city of Thespieae has a special landscape effect, we can report that we have now undertaken a similar study of the surface collections from a much smaller city (Hyettos in northern Boeotia) and found an identical spread of small scatters of prehistoric ceramics and lithics throughout the surrounding countryside. The recovery of transect pottery was at a higher rate here compared with the Thespieae hinterland, and there is a parallel rise in 'visibility' of the thin carpet of prehistoric finds across the landscape.

Thompson makes a good point and does expose an error in our calculations concerning the ratio of lithic to ceramic pieces reconstructed for the area under study in this article: in fact the count of surface finds was almost entirely restricted to pottery, with lithics being noticed and collected only at the second and separate stage, when a small sample from each transect was collected for subsequent dating. However, the net result is unchanged. If we take the counted pottery as some 100,000 pieces, and find that, among the collected pottery and lithics, for every 99 sherds about one lithic item came back, then we remain with some 1000 lithics—our original 'guesstimate'. Thompson, like Schon, also misreads our paper as claiming to give accurate locational characteristics for the original prehistoric activity foci in our district. It is precisely the point of our paper to explain how the palimpsest recovered by our generation of surveys can only provide data for scale reconstructions of what was missed, with future surveys hopefully gaining closer detail

by learning from our mistakes. Thompson's own lithic data cannot be used for comparative purposes, since no account is given of the factors we discuss regarding lithic recovery: 'mixed' or 'pure' lithic survey, or the proportion of distinctive materials such as obsidian among the local stone assemblage. Also, when he says that teams in his Sicilian project were instructed 'to collect all lithics seen during fieldwalking', we would express great misgivings that this somehow equates either to the total still present (whether seen or not) on the land surface, or to an even more remote possibility, the total that was once deposited on top of or into the subsurface layers of that landscape.

As for the specific difficulty in Boeotia that prehistoric finds may be swamped by historic pottery, making them hard to see, this of course is particularly problematic in very fertile agricultural areas with long occupation sequences, and manuring behaviour, at least around Classical Boeotian cities, may have exacerbated the problem (as noted earlier). Yet more important is our argument that wherever one or two pieces of coarse prehistoric pottery are found (on other surveys too, where remaining periods are less of an interference), we suggest a strong possibility that these represent vestigial subsurface features. Thompson proceeds from this misunderstanding to the inconceivable suggestion that our off-site lithics as recovered are a reasonable sample, when our deployment of the historic site 'windows' makes abundantly clear that standard fieldwalking by pottery-focused walkers simply fails even to see the vast majority of lithics. Thompson, again like Schon, also makes quite unrealistic claims—e.g. that from data such as ours some definite reconstructions of the distribution of sherd types across the landscape are possible, to achieve 'reliable characterizations of patterning in prehistoric ceramics'. If any of our mul-

tiple correction factors hold true, then the small sample of prehistoric pottery and lithics recovered can at the most only be interrogated to gain some idea of the minimal numerical scale of missed data and its very general spatial character across the whole district under study.

Thompson concludes by repeating some of our major points (e.g. better surveys needed; overconfidence of older surveys; etc.) and suggesting that small prehistoric pottery scatters could equally well have fallen off a donkey's back rather than reflect vestigial sites—an observation that ignores the case-study and technical evidence mustered throughout this paper and indeed almost takes us back to the uninformed early critics of field survey who deployed such explanations. The final comment that densities in transects are best treated as indicators of land-use intensity takes no account of our careful discussion of the taphonomy of surface assemblages. Thus, for example, whereas the Classical Greek off-site material that dominates this area does indeed, if our manuring model is accepted, reflect contemporary intense land use, the prehistoric low-density scatters are here argued to be equally ubiquitous, but in fact represent exactly the opposite scenario—a small population using the landscape in a shifting mode of extensive land use!

Finally, we wish to thank all the commentators for stirring up a useful debate about the issues confronting the next generation of surface surveys, and we hope our ideas will receive further debate and—even better—field testing. That the Boeotia model will find many empirical parallels in future intensive survey publications is supported by a paper (Attema *et al.*, in press) concerning the surface artefact cover in the Ager Pontinus near Rome. Prehistoric sherds here are fragile, are found in very small numbers over wide areas (typically 1-5 per ha), and suffer discrimina-

tion in recognition due to surrounding higher densities of harder historic ceramics. Most importantly, re-survey of such minimal scatters in optimal soil conditions has led to the conclusion that 'the find of a single sherd of pre-Roman ceramics is likely to indicate the presence of a small site'. Over a large area, very low-density prehistoric finds occurred in almost all fields; their elevation to site status results in small prehistoric sites being reconstructed every few hundred metres. We should note that in this area the prehistoric sherds have a very distinctive fabric, allowing easier recognition than in large areas of Greece.

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