5. What are we counting for?

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'The refinements of micro-measurement lead the observer below the level of significant phenomena, to delicate precautions to account for the wobble of individual chairs, unevenness of floor, slipperiness of linoleum or variation in woodgrain and so on. As if those were not problems enough, the measurer is always at the mercy of an arbitrary change in the programme "this is a dining room" which may be modified without his knowledge in some such way as "except on Thursday from seven until eleven when it is a dance hall'. Whenever human beings intervene we are in this scientifically undignified position.' (Ardener, 1971: 451)

INTRODUCTION

Since the beginning of intensive systematic surveys in the 1970s the most fundamental change of policy has been the attitude towards the quantification of off-site and low-density material. Work in the southwestern United States demonstrated the importance of these scatters and there seemed to be no reason why they would not be equally important in the Old World. Experiences such as the Boeotia Survey were vital for the perfecting of the techniques involved: the happy innovation of the 'supermarket' clicker allowed easy and consistent counting of pottery in transects (Bintliff and Snodgrass, 1985: 131), and in many subsequent projects, such as that on Hvar, *all* artefacts except tile were collected and bagged (Gaffney *et al.*, 1991). Although many surveys still do not operate such an intensive approach, there is a general consensus around Gallant's recommendation that 'the recording of visibility and artefact density on a regional scale (should) become routine in the design of Mediterranean surveys' (Gallant, 1986: 418).

When sites are found the quantification of material is intensified. Martin Millett advocates the counting and weighing of all artefacts from a sherd scatter (Millett, 1991: 25), while on Hvar total collection of all surface artefacts is carried out within a 10x10m grid, a procedure enhanced by systematic coring of the site with a petrol-powered posthole drill to test the subsoil and its contents.

The amount of data recovered is, inevitably, enormous, and not without its own costs. Of these, the most impressive is research time. At Hvar, less than a kilometre was surveyed in the 1987 field season (Gaffney *et al.*, 1991: 61). The Rieti Survey covered only 22 km in three years, for a total of *c.* 30 person/days per kilometre (Coccia and Mattingly, 1992). In contrast, intensive surveys in which off-site material was not quantified (Biferno Valley, Albegna) tended to average around 10 person/days per kilometre. On-site total recovery by gridding is even more time-consuming: one such collection, using a 5x5m grid on a Roman villa whose scatter measured two hectares, took around 40 person/days, or 20 person/days per hectare (Celuzza and Fentress, 1990: 146–147). Naturally the study (and washing and storage) of the materials is not included in this figure.

The cost in research time translates, in practical terms, into a very limited coverage of the landscape: Boeotia's 45 km over *c.*5 years contrasts with Albegna's 220 in roughly the same amount of time (Fig. 5.1); Rieti's 22 in three years with Biferno's *c.*400 in five years. The practitioners of total quantification are hardly unaware of the problem – indeed, in the early seasons of the Boeotia Survey much time was spent trying to render procedures more efficient (Bintliff and Snodgrass, 1985) – but in general the consensus seems to be that the results of counting all ceramic material are worth the effort, and that, conversely, ignoring the quantification of low-density scatters renders the rest of the survey meaningless. There have been only a very few expressions of doubt as to the necessity and validity of this sort of quantification. Ammerman remarks that 'the burden of trying to work with low-density surface material is that of its inherent stochasticity' and advocates a far more complex view of the formation of the ploughsoil assemblage since, after the initial deposition of the artefact, the assemblage is affected by an unknown number of factors (1985: 40). Redman considers that the surface is interesting only 'to the extent that it reflects what is underground or to the

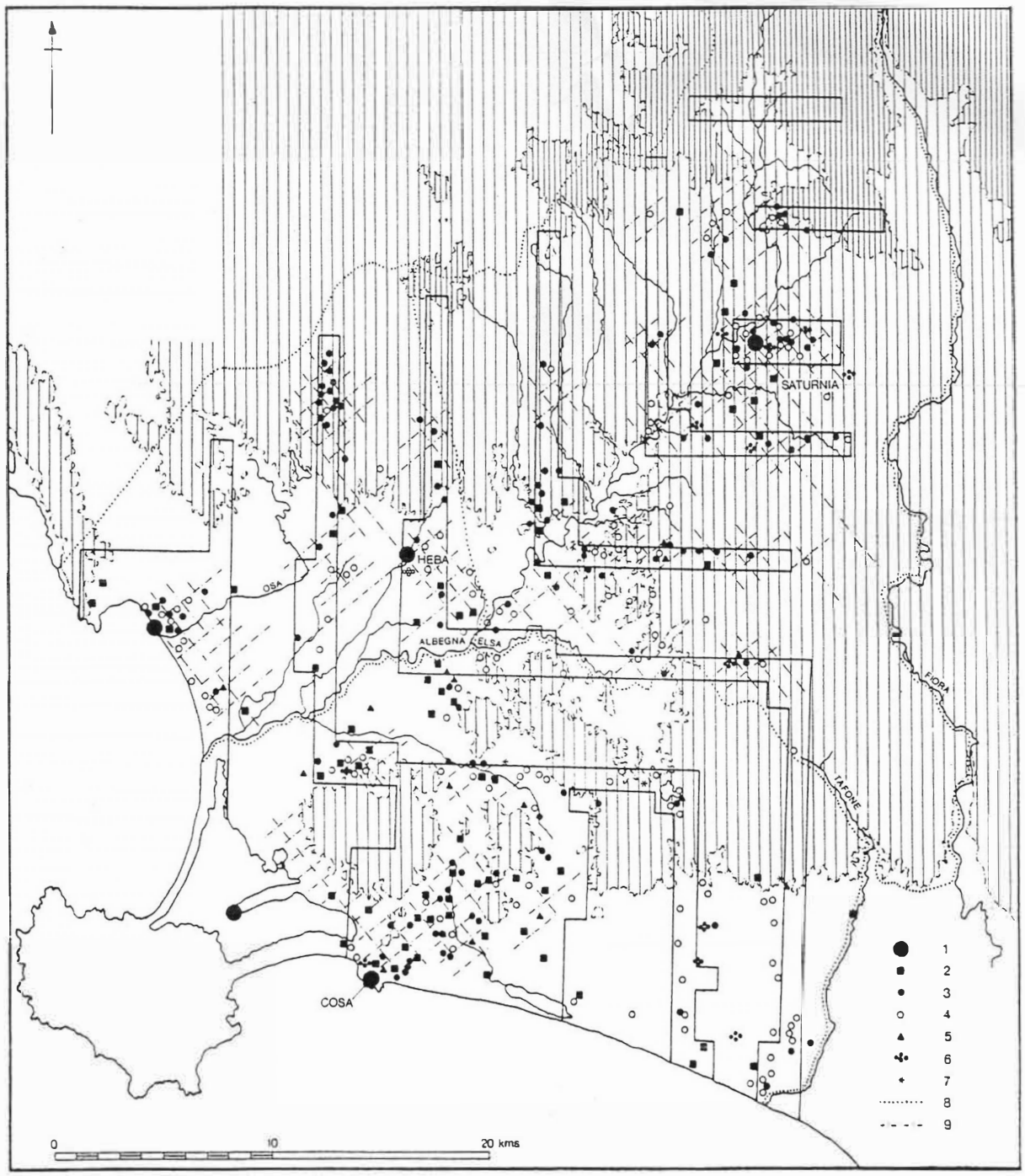

*Fig. 5.1 The Albegna Valley Survey: the settlement pattern in the late second century BC. 1. major centre; 2. domestic site; 3. domestic site of uncertain date; 4. tomb; 5. tomb of uncertain date; 6. village/*statio*; 7. kiln; 8. territorial boundary; 9. centuriation.*

extent that it is preserved for us as if it were a subsurface deposit' (1987: 250). Even Gaffney, Bintliff and Slapsak warn that 'it is not sufficient simply to transfer methodologies developed for the location and analysis of survey data on the wheat growing belt of northwest Europe or the arid lands of America to a Mediterranean context. The problems created by intensive terrace agriculture ... are alien to the simple application of techniques designed for use elsewhere' (Gaffney *et al.*, 1991: 76).

It should be evident from the foregoing that I am going to take a critical view of uncritical counting, and it would be hardly honest to ignore an element of self-justification, in that I was one of the designers of the Albegna Valley Survey, where a deliberate choice was made not to count

off-site material (Attolini *et al.*, 1991; Cambi and Fentress, 1985). The age of innocence which could be claimed in the case of the South Etruria Survey was coming to an end when we began the project in 1978, although our rejection of an off-site approach was perhaps more instinctive than carefully thought-out. This paper results from a reflection on that experience, as well as the necessary methodological planning for a new survey, on the island of Jerba in Tunisia. I have never surveyed in the eastern Mediterranean, so observations about survey experience reflect central Italy, Sicily and north Africa, where conditions may well be different. The paper is thus designed to provoke debate, not simply as a rear-guard action against an established technique – which was the easy, but not necessarily accurate, way to view the Hope Simpson-Cherry debate of a decade ago (1983; 1984) – but in order to move towards a more organic and integrated way of viewing landscapes and the ancient materials within them.

COUNTING OFF-SITE MATERIAL

The reason for counting off-site ceramic material is that, simply, it is very often there and thus must reflect something – I shall omit the question of lithic assemblages, which may *only* occur 'off-site', for this is an entirely different issue (Schofield, 1991a). Naturally a project could chose to count off-site lithics and not to count ceramics. In the American southwest, where the holistic approach to the ceramic landscape was started, off-site distributions could clearly be related to the actions of people whose economy included a substantial component of hunter-gathering. In very mobile societies, equating a 'site' with a 'settlement' would thus leave out a large amount of the record of human activity. But what of the settled agriculture of the Mediterranean? What part of human activity is reflected by low-density off-site material?

It has been forcefully argued by Bintliff, Snodgrass and Wilkinson (Bintliff, 1992; Bintliff and Snodgrass, 1988; Wilkinson, 1982; 1989) that the sort of off-site pottery distributions which result in 'haloes' around sites are the product of systematic manuring of nearby fields and garden plots. These haloes are particularly marked around large population centres, but are present on rural sites as well. Blankets of off-site material may extend over large tracts of the landscape. Thus studying the off-site material should give us an excellent insight into agricultural practices during the various periods of occupation in the landscape we are studying. Its presence provides an index of the intensity with which farming took place.

Recently, however, Alcock, Cherry and Davis have offered a convincing argument that manuring is not the only explanation for sherd halos around rural sites or, *a fortiori*, for the presence of artefactual 'background noise' in Mediterranean fields (1994). They point out that it not necessarily legitimate to import models based on medieval, northern European practice to the Mediterranean. Although there is no doubt that sanitation around large centres required the removal of excrement, both animal and human, and that logic would suggest that it be used as fertilizer, there is no necessary reason to assume that broken pottery would be mixed in with it. However, it is very likely that urban sites would have had some 'manuring halo', even though this might coincide to some extent with simple rubbish disposal (the area outside the walls of Taroudant, in Morocco, is entirely covered with modern rubbish to a distance of 300m). Rural sites, however, would probably have been manured principally by the penning or grazing of the animals in the fields: available manure was in any case limited, and not much of the *kopros* would have had a chance to be added to cultural refuse (p. 153f). What there was would have been concentrated on the vegetable gardens. They conclude that the role of manuring in Greek agriculture is being overplayed, although they do not deny that some manuring may have spread cultural refuse (p. 157).

To the arguments of Alcock *et al.* one could perhaps add, from a Roman point of view, that the farming activities of large villas were infinitely more articulated than the small farms which they consider. At the villa of Settefinestre well-constructed manure pits (*stercilina*) were found below the villa, near the vegetable gardens (Regoli, 1985). However, these lay over 200m from the kitchens, and the pottery rubbish (which was duly found piled up against the wall of the service wing of the main building) was hardly likely to be confused with the manure (Carandini, 1985: 73, 79). In a small farm with a courtyard almost anything would have ended up on the manure heap, unless it was fed to the chickens – much of what goes on modern compost heaps would have been otherwise disposed of on a farm with livestock. As the stables and barns move away from the kitchens the risk of confusion decreases. As Alcock *et al.* point out, intensive manuring is likely to be applied to the most intensively cultivated land, particularly the vegetable garden. This, again, would have been a specific, clearly articulated location even on a small farm. Rather than the sort of generalized halo observed in the fields around rural sites, manuring of a vegetable patch would have created a discrete concentration.

So what did Romans do with broken pots? Rather than deliberate manuring, observation of modern Italian rural practice suggests almost random dumping, particularly near streams or in small copses. One could go on, but the conclusions reached by Alcock *et al.* are strong enough already. Manuring does not, by itself, provide a sufficient explanation for either haloes or background noise, except in the case of large population centres.

Although many areas can be shown to lack any significant haloes around rural sites (projects listed in Alcock *et al.*, 1994: 141; to which might be added survey in the region of Marsala: Fentress *et al.*, 1986 and in the Belezma region of Algeria: Ait Kaci *et al.*, 1993), other projects do seem to support the manuring hypothesis. Gallant shows that although sites were not correlated with good soils in

the Lefkas-Pronnoi Survey, background noise was highly correlated both with good soils and with distance from a major settlement centre (1986: 415). So 'background noise, then, is a valuable source of information in its own right, as well as a crucial aid for defining sites' (p. 416). What we don't know, however, is the correspondence between major settlements and good soils. If background noise were in fact being generated by rubbish removal from the major settlements, and these were founded near the best soils, then the first correlation would be implicit in the second, and not in itself significant. Further, it is a *topos* of rural settlement that the farms are not placed *on* good soils but near them, so as to avoid occupying valuable arable land. The Middle Eastern examples cited by Wilkinson (1989) are more convincing, but even there the largest concentrations are found outside major agglomerations rather than around rural sites.

Bintliff and Snodgrass's list of other possible ways in which the off-site scatters might develop (1988: 507; Alcock *et al.*, 1994: 143) is not exhaustive, but gives us a starting point. Although we can probably ignore the pot which fell from the donkey (model 1), other off-site activity (model 2) would have scattered rubbish. In many areas of Italy workers do not return home from distant fields at lunch or even at night during ploughing and harvesting, and some pottery breakage would have taken place in the sort of temporary shelters used during those times. Insignificant in Classical Greece, transhumance was certainly taking place in Roman Italy (Barker, 1989; Gabba and Pasquinucci, 1979) so we can imagine that some scatter of cultural material would have occurred along the transhumance routes. Indeed, the edges of the principal Roman road up the Albegna valley are marked by a continuous low-density scatter, not reflected near other known roads in the area. A final common activity which leaves rubbish in fields is (tautologically) the deposit of rubbish in fields, with no better aim than to get it out of the immediate area of the settlement. This is sadly common today, with effects which may be just as permanent as that which took place in the Classical period.

All of these activities might thus produce off-site pottery scatters, but a far greater contribution to the sprinkle of material in the landscape is likely to have been made by climatic and mechanical destruction of buried sites (model 3). Experimental work such as that of Ammerman show that ploughing can account for substantial displacement of ceramic material (1985; Yorston *et al.*, 1990). Ammerman (1985: 38f) observed a consistent downslope movement of artefacts over two years, with a major movement coinciding with a probable episode of downslope ploughing. Experiments in Britain and the United States have produced less worrying results (summarized in Boismer, 1991: 17, but see Clark and Schofield, 1991; Yorston *et al.*, 1990), but these cannot be applied to the Mediterranean insofar as ploughing is regularly up- and downslope as well as (at least in Italy) around 0.8 m deep. This will, of course, exaggerate the displacement already present due to erosion events. Allen has shown that even without ploughing rilling caused by storms can have a significant effect on the displacements of artefacts downhill, causing them eventually to end up at the bottom of the slopes where they will tend to be buried by colluvial soil deposition (1991: 46f). Terracing may have an even more devastating effect on sites, covering material on the downslope side of a terrace, cutting it away on the up-slope.

Both farming practices and erosion can thus smear sites, and, if carried out over long enough periods, eradicate them. In the Albegna Valley, sites which were perfectly legible one year might be reduced to scattered tile fragments five years later, under conditions of equally high visibility. In one case, the farmer had simply re-shaped the slope with a bulldozer. How then can we interpret our off-site scatters? Is it possible to distinguish post-depositional scattering from manuring?

Qualitative analysis may give some clues. The various human activities (manuring, with models 1 and 2) would produce very different cultural material than post-depositional destruction of sites. Pottery is dropped off the proverbial donkeys, pottery is broken in fields, pottery ends up on manure heaps and amid the courtyard rubbish. Tiles do not, or would do so in far smaller quantity than pottery. Now, if the proportion of tile to pottery in a given quantity of off-site material is the same as that found on an average site, it seems reasonable to assume that the off-site material is site-derived, rather than resulting from a different and selective kind of deposit. Conversely, if pottery occurs in a higher proportion than it does on sites, we may look to the other explanatory models. Experimental work outside Gubbio has shown that the off-site scatters near the town walls had a high proportion of pottery to tile (up to 79 to 3), and thus manuring or rubbish-dumping would provide an acceptable explanation for them (Stoddart and Whitehead, 1991: 147). On the other hand, four sampled scatters of Roman material away from the town show a marked tendency towards tile, in the order of 5 to 1. This suggests that they derived from the erosion or ploughing-out of sites. Certainly it is generally agreed that the majority of off-site material conforms chronologically to the period of densest occupation of the landscape, which is exactly what one would expect if it were simply generated from site destruction.

However, although low-density scatters may be derived from sites they have been disassociated from whatever remains *in situ*, and can no longer be said to reflect it. If this is the case, is there any reason to count off-site material? Insofar as it is ultimately derived from sites the answer might be yes: Bintliff argues (1992: 116) that the distribution of off-site material should aid in the placement of 'missing' sites; although he bases his argument on manuring practices, it would be equally valid if the off-site material represented the immediate residue of those same missing sites. However, the fact remains that most of the off-site material in a field is impossible to date, especially when walking with a supermarket clicker. We

can loosely date tile fragments by association with datable material on sites, but this is hardly valid in an off-site 'blanket'. We cannot *quantify* the missing sites beyond noting that there is evidence that they existed and suggesting that they may coincide with off-site scatters. However, in that case it is sufficient to note that off-site material *exists* in a given unit (or forms a halo around a site): it seems quite unnecessary to count this highly degenerate sample. Perhaps the best solution is to restrict our attention to off-site material that is datable, recording for each unit the presence of diagnostics. These, and any associated scatters, could simply be indicated on a map in the field or on the record of the unit. Although we would still not know the ultimate source of the material, at least we would have its period-by-period distribution at a glance, without any significant expenditure of time. In this way, hypotheses as to, say, manuring and medieval field systems could be tested by more appropriate means (trace metals, phosphates etc.) in fields where one could be certain of some chronological indicators.

DEFINING SITES

Although Bowden, Ford, Gaffney and Tingle remark in horror that 'many early works attempted to force survey data into preconceived perceptions of sedentary habitation sites' and feel that the site is 'simply an archaeological construct' (1991: 108), Mediterranean archaeologists persist in attempting to populate the landscape (at least from the Bronze Age onwards) with the cemeteries, huts, farms and villages of settled agriculture. This gives the second major argument for the detailed recording of off-site material: its use in establishing what actually is a site, 'providing quantitative criteria for fixing site boundaries which can only be established relative to the surrounding level of find-density' (Alcock *et al.*, 1994: 138).

The most sophisticated treatment of this approach is that of Millett, where detailed quantification of all finds for a given period allow sites in that period to be defined by an appropriate measure, based on the probability that any find for that period will be lying in a field (Millett, 1991). This takes into account the extreme variability in pottery supply (Barker, 1991; Fentress and Perkins, 1989; Millett, 1985; this volume; *contra* Bintliff, 1992: 119f; Bintliff and Sbonias, this volume; Bintliff and Snodgrass, 1988: 511), particularly in periods such as the early Middle Ages, where it is almost absent (in Britain) or very rare (in Italy), and wood was probably preferred for domestic vessels. If, however, we use his criterion that sites are those with the top eighth of the values of sherds per hectare, then one sherd per hectare will constitute a late imperial/early medieval site. Although I am inclined to agree with him (many of the sixth and seventh century AD sites in the Albegna Valley consisted of thin 5 m2 spreads of tile with one or two sherds of pottery) this is to reduce the quantification of pottery to absurdity: in effect, we are making a qualitative judgement that since post-Roman pottery is so rare that *anything of the period we find* constitutes a site, and there is consequently no sense in counting it. This point also made by Schofield (1991b: 4) apropos of Saxon material. He suggests that density alone is not a reliable measure by which to study variations, when sites such as Cowdery's Down in Hampshire might be represented by two or three sherds, and where Roman 'background noise' might actually be denser.

But what of sites of the same period as the dense background noise? Isn't a statistical, quantified approach necessary there? Except for the most conscientious projects (such as the *Ager Tarraconensis*) there is an implicit consensus that we perceive a 'site' in the field, for all discussions of methodology distinguish between collection behaviour on- and off-site, and none suggest that all the data be fed into a GIS system before a decision is made (although Gallant comes close, 1986: 409). If we depended on ceramic material alone the problem would certainly occur more often. In fact, our perception of sites depends on many factors besides ceramic density: the presence of organic materials such as bone and charcoal; of 'heavy' artefacts such as *dolia* or querns or slag; of building materials such as stone, faced stone, 'foreign' stone, mortared stone, mortar, tesserae, floor tiles, burnt daub; of soil changes such as dark patches of earth (perhaps the visible correlate of the invisible phosphates and trace elements: Bintliff, 1992: 121; Cavanagh *et al.*, 1988), patches of subsoil (common in the Albegna Valley on cemetery sites) or the light clayey patches caused by the dissolution of *pisé* walls. None of these can be quantified (unless we were to establish Munsell values for the soils of each field) but all are qualities which can be perceived and registered.

What if none of these are present, and the site is only marked by a change in density? Surely we should measure that? The point is, of course, that we can perceive it without measurement. If concentrations of ceramic material without other physical characteristics did not have edges which could be perceived by field walkers, the elaborate methods used to plan sites which involve measuring the length of transects from the centre would be irrelevant (e.g. Bintliff and Snodgrass, 1985: 131, 133 etc.). It is only the limiting cases which might benefit from statistical analysis, and in the end it may never be possible to eliminate the subjectivity of the occasional judgement call, and to ascertain whether a concentration was omitted from the record because the fieldwalking team wanted lunch, or included because they hadn't found a site all day. These are the areas and sites which insist on a second visit, particularly under different conditions, and a proper recording of doubts on the part of the investigators. But doubts are very difficult to quantify. Finally, it is worth stressing that these borderline cases are rare, for the non-ceramic correlates of sites are present in the vast majority of cases. The qualitative correlates are perhaps less elegant than the picture of sites as bulges in the steady flow of off-site

material, but far more useful. Sites are rarely the sum of their sherds. They are indeed an archaeological construct, a meta-level of interpretation which resists definition by quantification.

COUNTING MATERIAL ON-SITE

The final major use of quantification in the field is within site boundaries, in order to study sites in greater detail. Such a study can be extremely useful, although of course the clarity of the results tends to be closely correlated to the degree of destruction of the site. In the Albegna Valley (Fig. 5.1) gridded collections were carried out on one villa site and one city site (the Roman Heba: Celuzza and Fentress, 1990: 147, 159). The former was studied with a 5m grid, the latter with a 20m grid divided into five 4m transects. In both cases sherds were collected, tile fragments counted, and an overall density plot established. Study of the pottery allowed chronological and functional areas to be established. The tile densities corresponded to some extent with an articulated plan for the villa, although only when combined with comparative material, which showed that at least 40 per cent of the main structure was not covered with the densest tile scatter. In the case of the city the tiles gave wildly aberrant results, with an irregular area on the upper part of the site apparently far more densely occupied than others in the centre of the town. Analysis of the density plot when compared to the contours and the air photographs showed that the density plot was almost wholly a product of the combined effects of ploughing and erosion. In a fashion predicted by Allen (1991: 45) as well as by Bintliff and Snodgrass (1988: 52), the tops of the slopes have been 'lagged', the soil has been washed away and the heavier material, such as tile, has remained. The destruction deposits of the town are thus more likely to be dragged up by the plough. Conversely, in the valleys between the three small ridges the accumulated soil has protected the archaeological deposits, which have been only marginally affected by deep-ploughing. Not by chance, it is these areas which showed the clearest traces on air photographs, and responded best to resistivity survey. Tile density at Heba thus reflects *nothing* besides post-depositional processes. Six hundred person/days were required for the site's 15 hectares, or 40 person/days per hectare. One is reminded of Hatchery West's 48 person/days per hectare, and 300 person/hours spent analyzing the material, only to prove that heavier points travelled further down the slope than light ones (Flannery, 1976: 52)

In fact, there is no reason to think that within a building, or urban cluster of buildings, tile density *should* reflect anything other than stochastic processes which we can neither quantify nor correct for. Given the aleatoric nature of our evidence it is almost impossible to identify areas which are more or less occupied on the basis of tiles. Indeed, the Boeotia survey found that there was no clear evidence for separation into distinct classes on the criterion of density, although sites occupied longer had slightly higher densities (Bintliff and Snodgrass, 1985: 135). Further, the tendency of modern farmers faced with a major Classical site is to attempt to get it out of the way, resulting in large stone-clearance piles which further distort the evidence, for it is only the larger material which ends up in the piles (Gaffney *et al.*, 1991: 72f). Other types of mechanical farming may obscure large parts of a site, such as the deliberate dumping of earth over stony ground. Even simple site-formation processes may produce results which are not immediately easy to interpret. Bintliff's work on trace metals (1992) or Cavanagh *et al.*'s on soil phosphates (1988) both suggest that the chemical remains of a site are consistently larger than the tile scatter. This may be due to the fact that the collapsed building (covered, in central Italy, with a thick layer of *pisé*-derived mud, which smooths the contours of the fall) forms a mound of perhaps a metre at most higher than the surrounding area. In the case of a farm this surrounding area may include courtyards and byres, pressing floors etc. Although the site will be 'smoothed' over time – by ploughing which lowers the top of the site, or by colluvial action carrying earth down around the site of the mound, or by deliberate dumping, as was the case in the excavated site at Giardino Vecchio (Celuzza and Fentress, 1990: 145) – the mound will remain more available to the plough than the rest of the farmyard.

Tile scatters on site are thus affected to some degree or another by stochastic processes, and remain a degenerate sample. At best, as in the case of the villa, we can map the dense tile scatters which certainly indicate buried buildings, and then make a guess as to where the tile scatter is missing because it is more deeply buried. But, if we are seriously interested in the building's plan, geophysical techniques would be a more appropriate means to study it.

This is not to say that gridded studies of large sites are in any way useless: quite the contrary. The differential distribution of diagnostic sherds for chronological information, or of activity-related artefacts, such as loomweights, querns and dolia for domestic areas, marbles, architectural fragments and inscriptions for public areas, slag and kiln wastes for artisan production, amphorae for storage – all of these can help in a close reading of the archaeological site (e.g. Perkins and Walker, 1990). Again the distinction must be made between qualitative and quantitative information. In the end, we cannot hope to quantify what is missing, but only record what is there. Distribution maps of archaeological presences have the advantage of a certain transparency: they do not account for what was not found, nor attempt to weigh the statistical validity of what was. They are also far more efficient than gridding. Collection of diagnostic material and mapping by air photographs on the 240 hectares of Doganella, without a rigid grid or quantification, required 150 person/days, or roughly 0.65 person/days per hectare (Celuzza and Fentress, 1990: 159; Perkins and Walker, 1990: 7).

This does not imply that gridded collection is appropriate for all sites. An initial random walk-through of a site is usually sufficient to reveal the degree of complexity in its (visible and available) articulation. Systematic sherding can simply follow this initial passage. Gridded collection serves to bring into sharper focus patterns apparent on the ground: we cannot seek for complex patterning in a site which at best is represented by 30 diagnostic sherds. Data in this sort of quantity is resistant to all but the simplest interpretations – domestic, 2nd century BC, 50m2 – and no amount of close measurement will provide anything more significant.

The rigorous application of gridding to all sites is thus rather like the vogue for 10mm contour surveys of floor surfaces which followed the publication of a survey of the floor of St. Mary's church at Winchester (Biddle, 1972: 106–107). Intensive methods are adopted in the hope that the systematic recording of all data will reveal hidden patterns in it. Yet in most cases, if patterns exist they will have already been perceived (Biddle, 1972: pl. XXXIV), and intensive quantitative techniques are simply used to demonstrate them.

QUANTITATIVE INFORMATION AS A RESEARCH TOOL

The foregoing would suggest that quantitative information is never useful, and always a waste of time. This badly overstates the case. I feel instead that automatic counting, especially of undated and unsorted material, tends to be wasteful of research time. The use of quantification to answer specific, appropriate questions is, of course, perfectly valid. There are any number of instances in which a specific research question can be answered by quantitative information. The best of these compare distinct classes of material rather than gross numbers and weights. Some of these have already been mentioned, such as Martin Millett's quantification of the occurrence of identifiable pottery types in the *Ager Tarraconensis*, which not only helps to define sites but also gives us detailed information as to the quantity of pottery available in a given period in a given place. This is a fine example of a Popperian controlled experiment designed to demonstrate a previously conceived hypothesis. I see no reason to repeat the experiment, which is entirely convincing, but others may want to attack the problem from other angles, such as the correlation between his findings and the evidence for long-distance trade (Fulford, 1987) or kiln technology.

Other experiments have elucidated aspects of site-formation processes or the very complex distribution of pottery around an occupied site. Microregional studies of medieval sites in France have given detailed pictures of field use in a period and area where we can hardly doubt that manuring took place (Zadora Rio, 1987).

Quantification can also be used to characterize sites. An experiment in the Belezma Survey was based on an attempt to differentiate between different types of site on the basis of their pottery assemblages (Ait Kaci *et al.*, 1993). Survey revealed sites with an overwhelming pottery density. The assemblages comprised a very low number of pottery types: a small range of late Roman fine wares (local African Red Slip), storage wares and handmade pottery. After an impressionistic assessment that sites in the mountains tended to have higher quantities of handmade pottery than the *opus africanum* farms of the plains, three 100m2 sample units were systematically collected from each site, and, in the field, the proportions between the different types were established, allowing a clear distinction between those in which handmade pottery accounted for over 50 per cent of the assemblage, and those it which it represented only 10 per cent (note that the sample in all cases was over 500 sherds).

All of these experiments have combined qualitative and quantitative approaches, rather than simply relying on overall densities. Further, they have all been problem-oriented. In the end, however, the most important quantitative information in a survey comes from the sites themselves: how many, how big, what kind, where, when. If small early medieval sites constitute only 1 per cent of our database then we will need to find 500 sites to have a reasonable probability of discovering five of them. To say anything useful about them we should have more. The same observation applies, of course, to other badly represented periods – the Bronze Age on Melos, the Iron Age in the Albegna Valley. Adequate numbers of the less common sites can only be achieved by close walking of many, many fields. I would thus like to return to my original point: how can we study a landscape without an adequate sample of it? By adequate, I mean site numbers large enough to lend our conclusions some reasonable degree of probability.

At this point I would like to return to the Hope Simpson-Cherry debate (1983, 1984). Hope Simpson, in his rebuttal, seems to be arguing that extensive survey retains some validity even if the information derived from intensive survey gives a more accurate picture of the relative proportions of different types of site. I am arguing that intensive survey which does not quantify off-site material retains validity even if the information derived from off-site material will give a more accurate picture of the whole distribution of pottery in a landscape. Here it is useful to return to Flannery, in this case his views on the 'Teotihuacan problem' (1976; Barker, 1991: 4). Missing Teotihuacan (or the principal population centre on your island) because your sampling strategy did not happen to hit it is not good archaeology. Extensive survey (interviewing farmers, driving around in cars, investigating prime sites) is necessary to make sure this does not happen, as well as to increase the sample of easily recognizable sites (villas, castles etc.). Of course, non-urban sites found this way are not counted when population statistics for the region are made (Redman, 1987: 251). Urban sites make an enormous difference, but are unlikely to be treated statistically in the same way as farms.

A finer lens can then be applied to the landscape in the form of intensive survey, with appropriate intervals between walkers off-site (10–20m) and on (1–5m). This should give a good picture of the numbers and variation in the population of sites *if* an adequate amount of territory is covered. At this level measurement and quantification take place on the scale of the objects we are examining – how big are the sites, what is their chronological range and their typology? The study of ancient demography is based on this information. However subjective the quantification of sites at this level may be, it has a far clearer correlation to human populations than the quantification of sherds.

The landscape can then be examined with any variety of even finer lenses, depending on the research interests of the team: manuring, rubbish disposal, relationship between nucleated settlements and their hinterlands, chronological and typological variation, the economic relationship between sites, manuring and the traces of centuriation (Favory *et al.*, 1994) or any one of the numerous questions that could, or should, be asked of survey evidence. Quantitative methods can be combined with the whole battery of geophysical approaches, as well as with excavation. The one proviso is that unless we collect enough basic data in the form of sites, the significant phenomena we are studying, the finest lenses will reveal single ecological niches rather than whole landscapes.

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