A FUTURE FOR MEDIEVAL POTTERY STUDIES?

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After fifty years of study led by such figures as Dunning, Jope, Barton, Hurst and Moorhouse, the writer is led to ask whether medieval pottery studies have developed beyond the accumulation of an increasing quantity of raw data and whether basic research questions have yet been adequately defined or projects evolved which are likely to elucidate them. The status of the subject appears low among archaeologists in general because, from the outside at least, it appears bedevilled by idiosyncracy, the cult of personality and inherent irrationality, and because tangible results, which are arrived at with great expense, themselves appear to have only limited significance beyond the coterie of ceramic specialists. Comments by historians and their use or non-use of pottery evidence as a tool in economic synthesis do not inspire confidence that the subject has any real standing outside the narrow circle who indulge in it. It is, therefore, quite understandable that the DOE and its equivalents in Wales, Scotland and Northern Ireland, is actively seeking to reduce expenditure specifically on pottery processing and the production of reports and archives. Compare this with the climate in which the same body established the Medieval Pottery Research Group and it will be clear that we have failed to establish the credibility of our subject, both as a study in itself and as a major research tool for the understanding of excavated sites. The aim of the present paper is to suggest that this poor rating is largely self-inflicted and stems from theoretical and methodological ineptitude on the part of its protagonists - us! It will attempt to define the problems facing medieval pottery research both in terms of their practical and structural limitations and also as a product of an underlying theoretical vacuity.

The practical problem

Most pottery workers have little choice in the selection of the material evidence with which they deal. The site to be excavated, the collection and recording methods used and the problems posed are normally devised by others. Whether or not the site is excavated in the first place usually depends on persuading the funding bodies of its importance, now often couched in terms of national priorities, which, with the exception of kiln sites, almost never involve pottery in the fundamental research design. As most medieval pottery produced and used was local in origin, the establishment of local or regional sequences is now very difficult to argue for. In any case this type of objective was always seen as a bi-product of excavation rather than as a reason for it. Thus, the main energies of the pottery specialist are directed towards processing, publishing and archiving the finds from specific excavations. There is usually no money to pay for regional syntheses or for pure research, which is often relegated to a spare time activity. So, only by inference and considerable personal sacrifice are pottery studies themselves advanced. The isolation of individual workers caused by the geographical dispersion of excavation units and by current policies which favour publication site by site, rather than local or regional syntheses, has led to a great deal of idiosyncracy both in terminology and method. Some individuals have been slow to throw off the image of tribal dating magician and for some the ability to date a sherd to the nearest quarter century has become a definition of competence. Local names for forms, fabrics and decorative features abound with little attempt at structured analysis. All in all the picture is one of disarray, low status and unsatisfactory results.

The theoretical problem

Underlying many of the perceived inadequacies of dating, sourcing and interpreting groups of pottery from excavations is a circular thought process. Pottery is collected by the excavator because it is expected to answer dating problems and to throw light upon the status of the site. geographical variations in occupation or activity between areas within sites, trading connections and fluctuations in economic well-being. The 'body of knowledge" used by workers to attempt to answer such questions consists of similar groups about which similar questions have been asked and which were themselves bi-products of an excavation process which had been undertaken for quite separate research objectives. Thus, there is very little independent data or theory which can be applied to excavated groups which has not itself been derived from such groups. This circular thinking has a number of important practical consequences. It has meant that, despite half a century of considerable progress in the accumulation of evidence, there is still no fundamental text book on the subject, no agreed body of theory and practise and few syntheses. This state of affairs may be likened to that of eighteenth century botany before the classifying innovations of Linnaeus or mid-nineteenth century geology before the great surveys of that period. That is, a vast amount of information, poorly organised with an absent or incoherent theoretical basis.

In addition, many of us are guilty of a particularist attitude to archaeological evidence which itself militates against a soundly based discipline. Each potsherd is conceived of as being unique, as was its maker, and as an art-historical object which is important in itself. In this attitude we are bolstered by our Arts education, museum background or institutional links and by the popular notions of what archaeology is about - have you found anything? what is it worth? The implication is that it is the thing itself we are after and not information about past societies which might lead to their better understanding. A typical "serious" press report reads "Rare find at Xchester". We learn of a rare Roman statue, the only one of its kind discovered so far from Britain. The implication is easily given that the object of excavations at Xchester is the recovery of objects of this type. If we ask ourselves how far a find of this unique type advances Roman studies we soon realise that it is really of very small significance. We already know that the Roman Empire produced finds of this sort and that Britain was in receipt of traded goods from the Mediterranean area. Thus we have learned very little that was not already known. This propensity to focus on the unique or exotic bedevils medieval pottery studies in which, for example, study of imported pottery, such as Saintonge polychrome and its distribution has consumed a much greater effort than that exerted over the very much more common locally produced wares. How many of us can resist attributing a false importance to a new find of imported ware? Isn't this because the ability to name it confers upon us that kind of importance which we would like to be applied to our studies as a whole? To name is to have power over. Whatever the precise motivation, particularism of this kind is a serious barrier to the establishment of pottery studies as an empirically based discipline and will always tend to deflect the worker from the real business of understanding the mass of information which large scale excavations now provide.

The task we are setting ourselves is as if a geologist, with only a crude knowledge of mineralogy and no historical or structural background were to be asked to produce an account of the geological sequence in Britain on the basis, solely, of a number of thin sections. This lack of structured academic basis for medieval pottery studies, this lack of discipline, indeed little discipline at all, provides funding bodies with little reason to throw away good money after bad and retards any real progress in the understanding of the subject.

These theoretical weaknesses can be best summarised in what might be described as the four maxims of pottery handling on the majority of British excavations:-

1. All pottery must be collected from stratified levels.

2. All pottery must be collected by excavated context.

3. All pottery must be either published or reported on in detail in the excavation archive.

4. All pottery must be kept.

If a botanist required that all excavated pollen grains, or seeds should be retained or a geologist that all the products of his fieldwork should be stored in perpetuity, few would doubt the absurdity of such a position. Unless we take positive steps to establish our own subject on a sound empirical footing we shall continue to retreat before financial stringency and better based archaeological methodologies.

Solutions ?

It would be wrong to suggest that many of the ways forward are not already being investigated. The very formation of the Medieval Pottery Research Group, and in particular the establishment of a network of regional groups which meet regularly to discuss common problems and material, must in the long run have done much to reduce the isolation of the individual worker and to ensure a far greater comparability of methods and results. The regular production of Medieval Ceramics, where theoretical and synthesising articles have been the norm, has provided an important counter-balance to the individual site report and a vehicle for new ideas and summaries of regional and national trends. The first issue contained, for example, an important article by Alan Vince, in which he suggested that the significance of pottery as measured between levels on a site or between sites, might be gauged by comparing the amount of pottery recovered with the quantity of animal bone or soil excavated from the same deposits. The attempt to place pottery studies on a more objective and empirical footing which is represented in articles such as this can be seen much more widely in evidence in Ceramics and Trade in which pottery descriptions and quantification have clearly moved a great deal forward since the foundation of the Group. The Guidelines themselves provide a useful summary of received wisdom and the best prevailing post-excavation practises. The Glossary, once generally accepted and used, should do much to standardise terminology, and, in fact, to produce a technical language. The Bibliography will attempt to organise knowledge of what has already been written about medieval pottery. It must be remembered, however, that most of these developments stem directly from a DOE initiative aimed at improving the quality of post-excavation pottery processing and publication. Although they will have important consequences in the field of theory, they make no direct contribution to the establishment of an hierarchy of pure research objectives or to the clarification of an underlying theory or set of theories. They are still within the circle.

In order to break out of this situation, a number of basic questions need to be asked. First, what kind of study is medieval pottery research? How, by what kind of thought process, does it achieve its results? Does it proceed by pure logic, intuition, experiment or what? Secondly, what is the nature of its Universe of Discourse? What is it about? Which questions are fundamental to it? Thirdly, how can the study best be furthered?

What kind of study is it?

The writer would suggest that, as we are dealing with three dimensional objects which occupy space and time and are subject to observable variability, medieval pottery studies must be considered, first and foremost, as empirical in character. The appropriate thought process is 'Observe - theorise - test theories' and at the level of data collection should be 'Observe - sample - test sampling techniques'. Although our subject is complicated by the fact that most of its data derive from past societies, it is really a branch of historical anthropology or sociology.

What is it about?

It is not the study of medieval pottery from excavations, as much of the literature might suggest, but rather, the study of the production, distribution, function, use and loss of pottery during the medieval period. The basic questions would appear to be how and why medieval pottery was produced, used and lost and what was its role within the wider dynamic of society? In addition how far do answers to the above vary with place and time? Questions such as the dating of medieval pottery, its socio-economic interpretative value and its meaning for studies of trade and international relations can only be secondary ones. Our first duty is to attempt to understand the pottery itself.

How we get better results?

We need to organise and carry out excavations specifically designed to test theories of pottery production and consumption at different times and places. At the moment fieldwork directed mainly at pottery problems is almost unheard of, except, perhaps, for kiln sites. The study of medieval pottery is too important to be left to the vagaries of excavators. An analogy with environmental work on excavations might be appropriate here. Discussions with environmentalists will produce a range of grudges about how archaeologists conduct their fieldwork. It is often the 'ignorant' archaeologist who decides when a deposit is worth sampling. Quite frequently important areas of the site are ignored and efforts are concentrated on the wrong types of deposit. When samples are taken they have often been subject to considerable mechanical damage or disturbance by trowelling. Although contexts are carefully recorded, there is rarely any detailed knowledge of the geography of site distributions. All such complaints might just as well have been made by the post-excavation pottery specialist on most excavations. Some sites should be excavated by pottery workers themselves in order to test, for example, presuppositions about variations in pottery use related to status or activity. In addition sampling techniques should be proposed, tested and established, so that material may be compared from site to site and the extraordinary waste of collecting and keeping all finds may be avoided (Maxims 1 and 4). On some sites this may well mean that all pottery will have to be three-dimensionally recorded (Maxim 2).

Apart from excavations we need to instigate a range of experimental approaches which may help to elucidate pottery problems. In addition to experimental production of a range of medieval pottery types, we should attempt to use many of the vessel types which have been described. This would undoubtedly bring to the fore those problems of definition and function which are being considered by the Glossary. We should also consider the sowing of sites which we can subsequently monitor in detail, with pottery of a range of types and in a variety of conditions. This would allow such variables as dispersion, wear and break-up to be tested in a range of different circumstances such as ploughed fields, gardens, demolitions processes, road and track surfaces, ditch fills, river beds etc. etc. Similarly we should consider the detailed monitoring of all the pottery in use in a number of different households whose occupants vary in occupation, social class and geographical dispersion. Experiments such as these would provide a body of independent 'controlled' data with which to compare our medieval groups. Until we have put our subject on to something like a rational foundation, can we justify present policies? When we have done so, will those policies remain as they are today?

Finally, at the end of this harangue, the writer would like to suggest that, given the lack of a satisfactory research basis for our pottery studies, the Medieval Pottery Research Group might consider the establishment of a Research Committee in order to promote the cardinal aim inherent in its title.

* This paper is a shortened version of one prepared, but not read, at the Aberdeen Conference in 1983.