was found. Forty years later, vast amounts of data have been published by this and other imitative committees and individuals; the largest group of such data is at this very moment wending its way into print. In the original committee’s first report, it was observed that ‘in all scientific work, many years are occupied with the collecting of facts, after which inductive reasoning weaves these facts into generalisations’ (Keiller et al. 1941, 70). Well, the facts are here. Now the analyses can proceed. Or can they? I would argue strongly that this is not the case. Why should this be?

The answer, I suggest, lies in the nature of research. At the time of the formation of the South Western Sub-Committee the use of inductive logic was very much in vogue. Too much hypothesising or theorising, and one risked an academic rap on the knuckles. The value of deductive reasoning, that is the conscious framing of hypotheses that can be tested by the collection of appropriate data, has been emphasised by some archaeologists in recent years. Extremes, however, should be avoided. Productive research is neither obsessive fact-gathering, nor needless question-asking. Rather, there should be a continual process of questioning, data collection, revision of questions and perhaps generation of new ones, more data collection, and so on. It is this kind of process that has been conspicuously absent from so much of the archaeological petrology conducted in this country.

Specifically what seems to have happened is that the means to the original question, that is the petrological and archaeological sources of stone implements, rapidly became the ends. One reason for this is undoubtedly the circumstance that the question (concerned with prehistoric trade and economy) was, to put it bluntly, not well considered. Knowing that an axe comes from the Lake District is not sufficient equipment for the understanding of the exchange mechanisms that took it to Wiltshire; any more than the knowledge that a tyre in Solihull is made of rubber from India, can lead to an explanation of the rise and fall of the British Empire. One wishes to know why one stone resource was favoured over others; how easy the objects were to produce; how robust the products; and how well they worked or took a new edge. How clearly could potential ‘buyers’ distinguish between tools from different sources? More importantly, it has to be recognised that exchange cannot be treated in isolation from its social context. Competitive traders with axe-laden canoes are all very well; but there are endless other ways in which the axes could have been moved, and the information on which to base any rational selection of possibilities cannot possibly come from the scrutiny of stone objects alone. In fact, identifying and understanding exchange systems is not a petrological problem. In this sense, the committee’s original goal was too ambitious. On the other hand, I would maintain that the form of data collection was not ambitious enough. What data are relevant? What are the questions that implement petrology committees should be asking?

One of the forces behind the growth of interest in petrolog-
ical analysis of stone artefacts in the 1920s and 1930s was undoubtedly a reaction against the ill-considered application of typology that was rife in lithic studies at that time. While others could wrangle over the precise palaeo-analyses that were based on the shapes of particular stone artefacts, Graves (1936), detractors could seek 'objective' data in scientific characterisation. This detraction has found a widening voice in the more recent and fashionable rejection of the culture model, so dependent as it was on the morphological classification of artefacts. The 1960s emphasis on economic explanations for what archaeologists could see in the past meant that many of the newly analytical techniques were applied to supposedly 'economically' data. Trade and exchange were seen as cornerstones of economies, and traded artefacts were identifiable with apparent ease.

After decades of 'collecting facts', the data on 'traded implements' could at last be exploited. First Graham Clark (1965), then more recently Bill Cummins (1980) and Elliot et al. (1978) have turned, with others, to the various regional committees' records to examine patterns of exchange in the Neolithic. However, these, and similar studies of pottery, do little more than play with a link between a site or artefact and a geological outcrop. The mere identification of traded pottery or stone tools on the basis of distinctive mineralogy constitutes an extremely narrow approach to material studies. We are asking no more questions about the objects themselves than were posed by the supposedly inferior culture or stage models.

Much work is currently in progress world-wide on many varied aspects of lithics and ceramics. For necessary reasons, most effort on the former is still directed towards the formal studies of manufacture and function, an unobtainable goal without laborious experimental and micro-morphological analysis. Neither the use nor the shaping of pots, however, are fields alien to our own technologies and domestic environments. It is thus perhaps not surprising that ceramic studies are ahead of lithic ones when it comes to understanding the use to which geological deposits were subjected. Of products from the various sources. Given, as has always been recognised, that there are problems with the nature of the sample studied, such comparisons can only reasonably be made between sources emitting a comparable output as seen through the eyepiece of a microscope. It is instructive to review the successive reports published by the oldest committee, to assess the progress made in understanding the use to which geological deposits were subjected. Between 1941 and 1972 (references in Evans 1974) there was inevitably considerable revision of the details of the petrological groupings, for it is only relevant to look at those groups which have remained unchanged. Of these, in 1941 and 1972, two were reported to have each provided more than 5% of all the objects examined: Group I, consisting entirely emerging as the oldest; and Group VI, similarly consistently registering some 10%.

The only substantial use made of petrological data by archaeologists has been to compare the relative quantities of products from the various sources. Given, as has always been recognised, that there are problems with the nature of the sample studied, such comparisons can only reasonably be made between sources emitting a comparable output. It seems that, in the South-West, the comparison could have been made as well in 1941 as 30 years and over 1200 petrological thin sections later.

For the simple comparison of relative distributions of varied products, the detailed information in the implement petrology tables is too detailed. Such inquiries can be pursued with significantly less cost and effort than is necessary for the compilation of these records. We need to look not only at thin sections of artefacts, but at the artefacts themselves, and at used and potentially usable rock sources. We have become mesmerised by microscopes. The prehistoric stoneworker did not have a petrological laboratory; but we can be sure he understood his resources. Mac-Strathern has described how informants in New Guinea would classify rocks primarily by raw material, using 16 different...
names, according to texture, veining and other properties (Strathern 1965). John Burton has recently been taken to an axe quarry by an old man who could describe the differing properties of twelve sub-categories of 'axe-rock', all in one quarry and in one geological stratum (Burton n.d.).

Nearer home, we are normally content to describe the flint in all the mines under three, technologically meaningless, names (i.e. top-, wall- and floorstone); names that not only apply properly to but one of the mining centres, but which even here, conceal a rich and complex variety of flints that was fully described for us in the 19th century.

I have been accused recently of wrongly ignoring Group I celts (Selkirk 1981). This group thus not only makes an appropriate one to illustrate my theme, but also enables me to explain why I thought fit to omit reference to it in my Shire book (Pitts 1980). The origin of Group I axes has always been sought in the far south-west of England, although the precise location of the quarry area has not been traced. In 1974, Cummins published an extremely interesting map that demonstrated relative peaks in the surface density of Group I celts away from the presumed source area (Cummins 1974). The largest of these, in East Anglia, would disappear if flint celts were included in the calculations. Nevertheless, there is apparently an absolute peak in that area, so the observation still stands. These aberrations from a normal fall-off curve in quantities of celts with distance from the presumed source raise interesting questions. Despite a common assumption to the contrary, there is no evidence for any form of direct trading in the neolithic. What we know of neolithic society makes as realistic a working hypothesis that most exchange took place as part of communication acts with wider significance than the simple transfer of material objects; and that much long distance movement of artefacts would have been the product of repeated exchange of the same items, which would gradually travel further from their point of origin as they passed from hand to hand. This model has a number of implications. For example, the tougher and more hard-wearing a celt would be, the longer it would stay in circulation and thus, other things being equal, the further it would travel from its source. Broadly, this seems to be borne out in the archaeological record. Brittle flint celts have much more restricted distributions than the tougher celts made from igneous rock. Distant density peaks of the kind mapped by Cummins would not, however, be readily predicted by this model. They suggest a more complex form of exchange, with redistribution centres away from the source, as Cummins has claimed. The origin and development of such centres, sometimes associated with early neolithic causewayed enclosures, are matters of no little importance. Cummins' thesis thus demands serious consideration.

Another prediction of the down-the-line exchange model is that, as a result of continual use and reworking, celts will generally be smaller the further from the source they are found. How do the sizes of Group I celts vary? Fig. 1 illustrates the varying weights of these celts over southern England. It is apparent that there is not a regular fall-off in size with distance from Penzance. Further, given the small sample size, the congruity between areas with relatively large numbers of celts is impressive. Something 'funny' is definitely going on. The next question to ask is are we dealing with one source and a number of redistribution centres, or are we confusing more than one source? Looking especially at the large celts in the north, one is loath to dismiss too readily the possible use of erratic rock. Absent from this map is the Yorkshire coast, where Group I celts are locally so common that they have for long been known as 'Bridlington axes'. Indeed, Cummins and Moore (1973, 223) have indicated that the distinctive 'Bridlington' shape is not represented in the south-west, and suggested that this indicates local reworking. But why

![Fig. 1. Regional variation in the weight of complete celts allocated to Group I (expressed in grams). This schematic map was drawn using the average weight of celts in 29 major museum collections, the respective museums serving as datum points.](image)
not local manufacture? The published descriptions of Group I petrology seem to allow for Norwegian erratics in the east Yorkshire boulder clays to be considered as a potential source for this Group. In this respect, it may be significant that, as Stephen Pierpoint (1980, 190) has recently remarked, Group I clefts in Yorkshire are commonest on boulder clay soils. There is clearly need for research here. This research should consider cleft shape and function, and possible geological sources. Both of these aspects require new work: neither the archaeological nor the geological information is available. I stress the absence of suitable petrological data. Until remarkably recently, field geologists have concentrated strongly on mapping solid deposits. Data on drift are consequently thin, as it were, on the erratics. if present, for working into implements.

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References


PREHISTORY IN THE JORDAN DESERT (Black Desert Survey: studies of prehistoric settlement in eastern Jordan)

by Alison Betts

Eastern Jordan is crossed by an almost impassable natural barrier, the lava desert of the Harrat er-Rajil. Its black and forbidding boulder-strewn landscape has long discouraged travellers and encouraged bandits, yet despite the hostile appearance of the region, it has supported a nomadic population since early prehistoric times. Until the late 19th and early 20th century game was abundant in the area. Herds of gazelle, oryx, onager and flocks of ostrich roamed across the whole of Arabia, a rich hunting ground for prehistoric man. Today, walls sunk by the Government and the possibility of spring grazing for their flocks attract bedouin for at least part of the year. A three-year project under the auspices of the British Institute at Amman for Archaeology and History is now under way to attempt to establish an outline of the prehistory of the basalt region.

Traces of prehistoric occupation are to be found almost everywhere in the area. Rocks strewn across the desert provided an endless supply of building materials for simple windbreaks, huts, corrals, and most interesting of all - an extremely elaborate system of traps designed to lead whole herds of animals into small enclosures where hunters, concealed behind low walls, could finish them off at close range. These are chert outcrops for raw material and one small obsidian source, although it is not yet clear whether it was ever exploited.