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Richard Bradley and Mark Edmonds

Excerpt

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I

NEOLITHIC BRITAIN AND THE STUDY OF EXCHANGE SYSTEMS

I

Making the connections

Introduction

The best way to introduce this book is by telling a story.

The basic elements are familiar, for the story is Italo Calvino's version of *The Count of Monte Cristo* (Calvino 1969). The scene is Château d'If, and the protagonists are the prisoners, Edmond Dantès and the Abbé Faria. Both of them wish to escape, but their approaches are subtly different from one another. Faria digs his way out of their cell, but all his plans go wrong and his tunnels lead him further into the fortress. On the basis of Faria's mistakes, his fellow prisoner constructs a map of the castle. If Faria is so energetic that he comes near to the perfect escape, Dantès spends his time imagining the perfect prison – it is a prison from which escape is entirely impossible.

His reasons are very simple. If he can conceive the perfect prison, it will either be the same as the real one – in which case there is no point in trying to leave it – or it will be one from which it is even harder to escape than the Château d'If. In that case all is not lost: 'We have only to identify the point where the imagined fortress does not coincide with the real one, then find it' (p. 152).

Thus the prisoners have two methods at their disposal. Faria digs blindly in the hope that he will escape, and Dantès draws together the results of the Abbé's tunnelling in order to create a model of the fortress as a whole. Those basic approaches epitomise the methods used in prehistoric archaeology on either side of the Atlantic.

No doubt the contrast is overdrawn, but since the 1960s research in the United States has been led by an explicit body of theory: questions of explanation have had pride of place, and the archaeological 'record' has been approached through a series of hypotheses, some of them derived from research in other fields (e.g. Binford 1987). In Europe, on the other hand, a tradition of detailed description has been difficult to shed, and here some of the more sophisticated interpretations of the past have developed out of a process of pattern recognition. Nowhere is this clearer than in the study of exchange, where work in the two continents has drawn on quite different traditions of research.

The material studied in this book is no exception. It demonstrates quite clearly how tenacious an empirical approach can be. The non-flint axes made during the Neolithic period in Britain have been examined by petrologists using essentially the same techniques for half a century: a programme of research which must be unmatched for sheer persistence (Keiller, Piggott and Wallis 1941; Clough and Cummins 1988). Yet their results were hardly used in studies of prehistoric exchange systems until about ten years ago (Cummins 1979; Hodder and Lane 1982). In effect, the first forty years

of research were devoted to detailed *description*. Neolithic specialists accepted that axes had travelled over considerable distances, but it seemed enough to map the extent of their distribution and to observe any changes in that evidence over time. For the most part, interpretation of these patterns was regarded as self-evident. To all appearances, it was also a secondary consideration.

This approach to the material record is deeply entrenched in Europe. It is the archaeology of the Abbé Faria, tunnelling energetically and getting nowhere. We can compare the exhaustive characterisation of stone axes with the lists of metal analyses compiled by the Stuttgart laboratory (e.g. Junghans *et al.* 1960), or with the great catalogues of metalwork published in the series *Prähistorische Bronzefunde*. Each is a testimony to the energy and devotion of its authors, yet there are few signs that these studies have had much impact on our understanding of broader patterns of production and exchange. Either these compilations are viewed as worthwhile ends in themselves, or their sheer bulk is so inhibiting that they remain on the library shelf: lists of artefacts sundered from their material and historical contexts. Despite their thorough documentation, they rarely attract the attention of archaeological theorists.

To some extent the situation has changed over the last few years, and many prehistorians working in Europe have acknowledged the impact of processual archaeology. Its attitudes have more in common with the position of Edmond Dantès, yet there is a danger of taking the comparison too far. Where European prehistorians had placed the major emphasis on cataloguing and characterisation, the new generation of scholars turned to quantitative modelling. Exchange systems were studied by formal methods, some of them derived from geography or economics, and most of the developments to establish themselves securely concerned methods rather than theory. There were important changes in the ways in which data were collected and analysed, yet there is a sense in which the impact of the New Archaeology has been surprisingly limited.

Its ideas were adopted as part of a wider reaction against the excessive empiricism of the post-war years, and to some extent its reception was coloured by thinking in social anthropology. Anthropologists working in the United States had more in common with archaeologists and even taught in the same university departments. Certain ideas were shared between the disciplines; there was the same emphasis on cultural adaptation, on social types and progressive evolution, and in some cases the new interest in exchange was informed by ideas from formalist economics. This contrasts with European anthropology, where the dominant influence was probably that of Lévi-Strauss. Research workers took more interest in understanding societies *from the inside* and had turned their attention to the symbolic dimensions of human behaviour. The differences are epitomised by the title of Marshall Sahlins' famous polemic (Sahlins 1976). European research placed more emphasis on *culture*, whilst the New Archaeologists of the 1960s and 1970s were interested in *practical reason*. Exchange systems are a case in point. Processual archaeology emphasised the importance of precise *measurement*, whilst European social anthropologists showed a greater concern with *meaning*. In this situation, the techniques pioneered by American archaeologists took on a life of their own. There was little common ground with

European social theory, and no sooner had the precepts of the New Archaeology been absorbed by prehistorians than Edmund Leach denounced it as the offshoot of an obsolete anthropology (Leach 1973).

In effect, the methods of the New Archaeology were introduced into an intellectual climate in Europe in which a number of its assumptions were no longer so acceptable. Although archaeology and anthropology could be taught together in European universities, there were areas in which the subjects had diverged to an alarming extent. Among these was the study of exchange. As we shall see, the New Archaeology ensured that data were interrogated and ordered with a growing sophistication, so that now a whole battery of analytical techniques were available. But at the same time, these methods had become increasingly divorced from anthropological studies. To adapt a famous saying of Oscar Wilde, archaeology and anthropology had become two subjects divided by a common interest.

Many writers have sensed a new rapprochement between these disciplines (e.g. Rowlands 1982; Hodder 1982a). In this chapter we argue that after twenty years of productive research, formal methods for the analysis of exchange systems are starting to reveal their limitations. It is to social anthropology, and particularly the tradition associated with Marcel Mauss, that we must look for further progress. The first part of this chapter traces the development of exchange studies in processual archaeology and the difficulties that gradually became apparent in that work. The second section shows how some of the same issues have been approached by social anthropologists and advocates a new synthesis of these two fields of research. In the remaining chapters we try to practise what we preach. We turn to one of the largest bodies of data in prehistoric archaeology and offer a new study of the 'axe trade' in Neolithic Britain. In Calvino's story Edmond Dantès' escape from the Château d'If depends on his ability to put Faria's observations to good use. With fifty years of petrological analysis at our disposal, we shall try to do the same.

Exchange systems in prehistoric archaeology

The petrographic analysis of stone artefacts is so well established that it is difficult for two British authors to appreciate the impact of characterisation studies on prehistoric archaeology. By linking specific products with identifiable sources these studies provided one method of evaluating the diffusionist interpretation of European prehistory. They reduced the importance of purely stylistic comparisons (Dixon, Cann and Renfrew 1968) and, used together with radiocarbon dating, they prepared the way for a reading of the evidence that emphasised *local* processes rather than the migration of people (Renfrew 1969). At the same time, characterisation studies were a prerequisite for the development of more subtle regional analyses, providing one method of charting the character and scale of resource use. Such work has taken two general forms: analyses which use the overall distribution of a particular raw material to define an analytical region, and those where the distribution of different raw materials helps to identify 'interaction zones' (Pires-Ferreira 1976; Renfrew and Dixon 1976; Plog 1977).

For the most part studies of stone axes in Britain have operated at the broader

geographical scale and have been concerned with describing their distribution across the country as a whole (Clough and Cummins 1979). Only recently has much interest been shown in exploring the relationship between the products of *different sources* (Chappell 1987). This change of attitude is due to the work of Colin Renfrew and his colleagues who studied the distribution of obsidian in the Aegean and the Near East (Renfrew, Cann and Dixon 1968; Renfrew 1969), although similar work was carried out over the same period in the New World (Flannery 1972; Beale 1973). Renfrew's initial objective was to reassess the geographical connections that formed the basis of the diffusionist model, but in doing this it became important to identify the kinds of process that were responsible for the spatial patterning seen in the archaeological record. It was no longer enough to describe the distribution of particular artefacts or raw materials; it was important to identify the processes behind different dispersal patterns.

Such studies were important because they raised the possibility that detailed analysis of the movement of objects from one area to another – or, more precisely, studies of the frequency with which they were used and discarded – would shed light on the institutions involved in promoting and maintaining contacts. Many of the early analyses took a similar form and focussed on the changing number of objects in relation to their distance from the source, a method with an established pedigree in geography and economic anthropology. They highlighted a recurrent pattern in the movement of goods outward from their area of origin. Almost without exception, their numbers decreased with distance from the source, a pattern of use and discard which Renfrew was to formulate in his 'Law of Monotonic Decrement':

In circumstances of uniform loss or deposition and in the absence of highly organised directional (i.e. preferential nonhomogeneous) exchange, the curve of frequency or abundance of occurrence of an exchanged commodity against effective distance from a localised source will be a monotonic, decreasing one. (Renfrew 1977, 72)

This was an analytical device which was designed to isolate anomalies and also to distinguish between what Renfrew called *contact* and *supply zones*. One aspect of this work was to prove particularly influential, for he suggested that an exponential fall-off curve could be explained by a model of 'down-the-line exchange' or balanced reciprocity. This was a pioneering attempt to forge a quantitative link between a specific anthropological model of exchange and its material residue. Moreover, it followed from that argument that significant departures from the basic fall-off curve might reflect the intervention of different agencies in the movement of goods between their source and their ultimate destination (Renfrew 1975). In other words, formalist analytical techniques could be used to identify 'types' of exchange that substantivist anthropologists placed along an evolutionary continuum (Dalton 1977). These ranged from 'reciprocal' and 'prestige-chain exchange' to 'freelance trade', 'directional trade' and even *laissez-faire* capitalism. Each was held to have specific implications for the organisation and complexity of the societies in which they operated. Thus in Sahlins' typology reciprocity could be linked to segmentary

societies, whilst redistribution would be correlated with chiefdoms or states (Sahlins 1972). In a similar vein, Ian Hodder showed how regression analysis of different fall-off patterns could distinguish between the movement of low-value, often bulky goods which had travelled a short distance, and the movement of higher-value goods, whose distributions extended much further (Hodder 1974; Hodder and Orton 1976, 124).

These were exciting developments of the original model since they held out the prospect of extrapolating from distribution patterns to social organisation through a fuller understanding of how the exchange of goods had been organised. Inevitably, as more applications of these methods became available, empirical and theoretical problems began to be identified. Many of these surfaced in two volumes dealing explicitly with approaches to prehistoric exchange, both of which drew heavily on the conceptual framework mapped out in earlier studies (Earle and Ericson 1977; Ericson and Earle 1982). At an empirical level it seemed as if the predictive power of Renfrew's two-dimensional model could be enhanced through the incorporation of additional factors that might have influenced the regional distribution of material (Wright and Zeder 1977). These included the nature of transportation and access to and from the area of origin, the character and chronology of the sites used in the analysis, the density of population in different regions, and the availability of alternative resources (Wright 1969; Ericson 1977; Singer and Ericson 1977; Ammerman, Matessi and Cavalli-Sforza 1978; Finbow and Bolognese 1980). The last factor was to be explored in a study of Neolithic axes in Britain, which employed a 'gravity model' based on the 'attractiveness' of artefacts from different sources (Chappell 1986; cf. Hallam, Warren and Renfrew 1976; Hodder 1978). As Torrence points out, however, such an approach only offers a basis 'for describing and comparing distributions which are already reasonably well documented' (1986, 27). Valuable as this may be, it does not provide sufficient grounds for inferring the character of the processes that lay behind those patterns.

Stone artefacts are especially well suited to such studies, and this may be why the early analyses of obsidian distribution have proved of such lasting value. We must also consider the relationship between consumption and exchange in relation to the physical properties of different raw materials. The problem is partly one of archaeological visibility. In propounding the Law of Monotonic Decrement Renfrew had referred to 'circumstances of uniform loss or deposition', but not all the artefacts that were exchanged would have entered the archaeological record. Metals were frequently recycled and the products of different ore sources might be mixed together – a problem which confronted the Stuttgart analyses mentioned earlier. Ceramics could also be recycled, as broken pots were used to temper new ones, but stone is virtually indestructible. In the case of lithic artefacts complete recycling is not an option; although objects may be extensively reworked, a raw material like obsidian will retain its essential characteristics. Moreover, the production, re-use and maintenance of lithic artefacts leaves a direct trace in the form of debitage, and this often provides a basis for inferences concerning source, technology and chronology. Indeed, Ammerman and Andrefsky (1982) argued that any study of the regional distribution of lithic materials should incorporate an assessment of the reduction sequences

employed at different locations. On this basis it would be possible to establish how far the level of consumption on any site reflected its involvement in artefact production.

There are further problems in using the evidence of consumption to infer the character of exchange. There has been some progress in measuring the proportion of imported materials 'dropping out' of the system, but as evidence of how they had circulated this is tantalisingly indirect. Most studies have been concerned with objects or materials that were 'routinely discarded' when their use-life was over, yet, as we shall see, this approach makes questionable assumptions about the nature of material culture and the roles that it plays in social life. The problem becomes more obvious where particular objects might have been associated with personal identity or prestige, or where they had been deployed in specialised transactions (e.g. Moholy-Nagy 1976). In such cases they may have entered the archaeological record as formal deposits, for example grave goods or votive offerings. Their distributions could differ markedly from those of objects that were discarded with less formality. For example, Sidrys demonstrated that different amounts of obsidian were entering Classic Maya sites according to their status in the settlement pattern (Sidrys 1976, 1977). Two separate regression lines could be calculated to summarise the movement of this material away from its source; the density of obsidian in the excavated deposits on major centres was about six times that encountered at minor sites. This might have happened because the movement of obsidian was articulated through these locations, but it seems more likely that it reflects two different patterns of consumption.

A more serious problem emerged in Renfrew's later work, for it became apparent that a number of quite different agencies might have been responsible for the same types of patterning in the archaeological record (Renfrew 1977). The two types of regression identified by Hodder still retained some validity, but simulation studies and ethnographic research were starting to show that essentially similar fall-off curves could be created by a range of quite distinct processes (Ericson and Earle 1982). Still more discouraging, fall-off curves very much like those believed to identify particular kinds of exchange could all be created by the random movement of goods outwards from their source (Elliott, Ellman and Hodder 1978). By extension, even where distinctive spatial patterning could be recognised, it was by no means certain that this reflected one kind of socioeconomic relationship. For example, centrally administered redistribution might result in the same spatial patterning as the operation of a market economy.

Faced with such problems, several authors concluded that formal models would be most useful where they could be assessed in relation to additional sources of information. Nevertheless their discussions continued to work from the assumption that there might be a direct relationship between different levels of social complexity and different forms of exchange. Many of these arguments feature in Ericson and Earle's edited volume *Contexts for Prehistoric Exchange* (1982), although the significance attached to the word 'context' varies considerably between different authors. Earle, for example, talks of a 'more detailed contextual analysis that considers the broader economic, social, political and ideological forms in which exchange is embedded' (Earle 1982, 7–8; cf. Spence 1982). This statement echoes a strong

substantivist vein among the contributors. Other papers place more emphasis on methodology, and here 'context' is used as a synonym for provenance. Despite the range of different viewpoints, many of the authors acknowledged problems created by the massive scale at which earlier studies had been undertaken, and looked for more detailed patterning at the local level. Finbow and Bolognese (1982) suggested that the solution to some of these problems might be provided by trend surface analysis, whilst other contributors showed that patterning at a local level could even contradict the trends detected on a broader scale of analysis (Bettinger 1982; Earle and D'Altroy 1982). In other words, Renfrew's model might work well at a regional level but did not account for variations which stemmed from more local factors, such as site specialisation or the presence of sociopolitical boundaries (Bettinger 1982). The argument was underlined by McBryde's ethnohistoric studies of Aboriginal exchange, which were published at about the same time (McBryde 1979; McBryde and Harrison 1981).

Another approach has been to investigate production sites. We find the same attempts to identify consistent relationships between different forms of social organisation and the character and organisation of production (Singer and Ericson 1977; Sheets 1978; Ericson and Purdy 1984; Torrence 1986). This kind of reasoning characterises Ericson's discussion of obsidian exchange in California (Ericson 1982), but it is most clearly articulated in Torrence's analysis of the obsidian quarries of Melos in the Aegean (Torrence 1986). Her work focusses on the lithic reduction sequences practised at the stone source, with special attention to questions of standardisation and specialisation, as measured by technological features such as error rates. Torrence draws on ethnographic and historical accounts of stoneworking in different socioeconomic settings in order to define a continuum of productive efficiency. This she describes as 'a framework for measuring exchange'. Different points along this continuum have different social correlates and reflect the existence of quite different forms of exchange. On that basis she rejects the hypothesis that Melian obsidian had been extracted and worked by specialists. Research by Muller (1987) on Mississippian exchange systems has something in common with this study, for here again close scrutiny of the evidence for shell artefact production led to a reinterpretation of distribution patterns thought to identify specialist activity.

Muller's study is more wide ranging than Torrence's, and makes two observations that serve to highlight the true complexity of the issues. First, he comments on how the discovery of fine or exotic artefacts can lead to circular interpretations. Their presence in graves tempts archaeologists to infer that the deceased had enjoyed a special position in life. They then go on to suggest that high status depended on the ability to secure those objects; the character and role of exchange are inferred from exactly the same material. Secondly, Muller shows how the distinctiveness of the artefacts found in his study area has been exaggerated. This contrasts with the way in which *the same objects* were treated at Spiro, nearly 1000 km away. Here these artefacts are concentrated in a small region, where they are best represented in a restricted set of burial mounds. Both observations point to the difficulty of drawing inferences about the value of certain artefacts from the circumstances attending their production. In this case the value accorded to shell artefacts may have changed as they circulated in

contexts some distance from their area of origin. In much the same way, Ammerman and Andrefsky's discussion (1982) of obsidian exchange in Calabria shows how the significance ascribed to particular objects may change according to their context.

Problems in the study of exchange

Many of these studies have been stimulating, but they do have their limitations. So many problems have arisen that we must question the assumption that the refinement of formal methods of analysis can ever provide a satisfactory 'framework for measuring exchange'. There is the critical problem of equifinality. There seems little virtue in pursuing quantitative analyses of artefact distributions if we lack any grounds for inferring the nature of the processes responsible for their creation. All that we shall achieve by that work are more precise and all-encompassing *descriptions*. These remain important for comparing different exchange systems with one another, and for tracing their changing configurations over time, but at a still more detailed level their role will be quite limited. The problem is no longer one of scale, for the most serious difficulties result from the assumptions that lie behind such formal analyses. As Hodder notes, most studies have been predicated on the idea that progress can be made by *assuming* that people in the past considered costs and benefits along formal economic lines (Hodder 1982b). Torrence's study is a case in point, for it is only by making this assumption that she is able to apply the same scale of measurement to people as different from one another as hunter-gatherers procuring workable stone for their own use, and the makers of gunflints for sale in the modern world market (Torrence 1986). We do not deny the value of heuristic models in thinking about the archaeological record. The problems arise because we equate predictions made on this basis with explanation or broader understanding:

To say that Y amount of pottery is found at a site because it is X distance from the source and because the relationship between X and Y fits a regression formula is hardly an adequate explanation of the exchange process. In the same vein, concepts from modern economic theory may be adequate for describing and 'predicting' the past, but since we cannot be sure that scarcity, maximisation and surplus are relevant concepts for past societies, attempts at explaining why a particular formal pattern is found are liable to be of limited value.

(Hodder 1982b, 202)

There are other problems with the idea that particular dispersal patterns should reflect the character of past societies. It would be easy to focus on the extremes encompassed by different case studies and to extrapolate between them in order to locate those societies that fall outside their remit. We could arrange the various systems of production and consumption according to their scale and complexity as if this mirrored a real sequence of development. Dalton came close to doing so in his contribution to *Exchange Systems in Prehistory*, whilst Earle and D'Altroy hint at an evolutionary sequence in their discussion of 'staple' and 'wealth finance' systems (Dalton 1977; Earle and D'Altroy 1982). Marxist scholars are no less vulnerable on

this account, and they too have looked for links between specific modes of production and particular types of social formation, although in non-capitalist societies these are based on a different conception of exchange (e.g. Friedman and Rowlands 1977).

Such approaches do have their attractions, but we hesitate to follow that course since it brushes aside the problems raised by particular analyses in favour of a general model so far removed from the data that it loses any analytical power. It would make it difficult, and possibly unnecessary, to explore the conditions under which particular networks developed or the purposes that they served. At the same time, social typologies have had an unhappy history. Feinman and Neitzel, for example, have shown that schemes of this kind establish no more than a one-dimensional index. Once we extend the scope of our analysis, the sharp outlines disappear. Those cross-cultural correspondences that appear to hold do so at such a level of generality that they tell us nothing about the conditions in which particular practices were employed (Feinman and Neitzel 1984). Such approaches leave little room for competition or conflicts of interest.

If it is difficult to extrapolate from spatial patterns, it is still harder to infer social institutions from the evidence of production sites. Yet in a number of cases this is exactly what has been done. Thus for Torrence, and for Singer and Ericson, production sites or quarries provide a unique context in which to study 'a complete exchange system' (Torrence 1986, 91; Singer and Ericson 1977). Similar ideas have been propounded by Earle (1982). We agree that the archaeological potential of production sites has very rarely been realised, but take issue with the idea that they can be studied in isolation. There is no intrinsic reason why the relationships between production, distribution and consumption should be either predictable or direct. It is the nature of their articulation under specific historical conditions that needs to be explored, and this cannot be assumed a priori. Otherwise studies of artefact production merely substitute one partial analysis for another.

In effect, the last two decades have seen the development of a battery of sophisticated techniques for studying prehistoric exchange. Most of these have been concerned with examining broad patterns of artefact dispersal. This work has been supplemented by a more limited number of studies of production sites, but these share the same disadvantage, that the character of the entire system is being researched through just one of its component parts. Moreover, with a few notable exceptions, research in this field has emphasised the frequency or *scale* of consumption, rather than the *character* and *context* of artefact use and deposition. The life history of different artefacts seems predestined, from the moment they are made until they enter the archaeological record. As a result we sustain an extraordinary loss of detail. The summary character of these models does little justice to the richness of the material that they address. If we are to catch something of what is lost, we must go beyond the methods of processual archaeology and explore the potential of other sources.

Alternative perspectives

A useful starting point for this discussion is a review article by Ian Hodder (1982b). He argues that if archaeologists are to study exchange in its social context, 'as part of a