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DISCIPLINARY FAULT LINES: SCIENCE AND SOCIAL ARCHAEOLOGY – ANOTHER VIEW

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ABSTRACT

Bernard Knapp's (2002) surprising response to an earlier exposé (Gale 2001) of misunderstandings in Knapp (2000) is challenged, further misunderstandings in Knapp (2002) are corrected, especially in regard to the non-existent opposition of science to social archaeology and the absence of 'fault lines' in Gale (2001), and about the source(s) of copper oxhide ingots, and the interpretation of lead isotope analyses. The call for cooperation and collaboration issued by Knapp (2002) is accepted.

KEYWORDS: science-aided archaeology, copper oxhide ingots, lead isotope analysis, Bronze Age, Mediterranean, recycling.

INTRODUCTION

Knapp (2002, 37-38), using surprisingly intemperate and unscholarly language in a hyper-defensive attempt to reject natural scientific data for the sources of copper for oxhide ingots, accuses Gale (2001) of attempting "to pose a 'scientific' challenge to theoretical viewpoints and social approaches in archaeology". This is emphatically denied, as is the accusation that Gale (2001) intended "to present a scathing dismissal of social archaeology" so long as social archaeology is of the nature of that undertaken by the like of Renfrew (1984). Further, what was gained by the publication of Gale (2001) was not as incorrectly summarised by Knapp (2002, 38), but rather a carefully argued point-by-point refutation of the catalogue of errors and mis-representations which formed the larger part of Knapp (2000). Knapp (2000) was in large part a polemical and intellectually dishonest attempt to discredit earlier papers from Oxford which had as their objective the use of lead isotope analyses (LIA) to establish the source(s) of copper used

to make the copper oxhide ingots. The Oxford endeavour was a natural development, urged by an archaeologist (Branigan 1982), since no progress whatsoever in solving this question had been achieved either by archaeology, or by other methods of natural science such as metallography or chemical analysis. Part of it was in collaboration with the expert in Sardinian archaeology Fulvia Lo Schiavo (1990, 7-12).

The response of Gale (2001) to Knapp (2000) was more or less mandatory, as recognised by the editors of E.J.A., if this important field of enquiry was not to be plunged back into the obfuscation which had so nearly resulted from the earlier interventions of Budd and his colleagues (e.g. Budd et al. 1995). However it is quite unnecessary to repeat here all the necessary corrections made by Gale (2001) to the inaccuracies, misunderstandings and falsifications contained in Knapp (2000). Interested and impartial readers will easily see where the truth lies by a close comparison of Gale (2001) with Knapp (2000). Almost unbelievably, serious further errors are contained in Knapp (2002), which force this response to that paper. It seems legitimate to question the motives behind Knapp's continuing but isolated attacks (Knapp 1999, 2000, 2002) on the Oxford work on the copper oxhide ingots. Knapp (2002, 42) implies that the scientific endeavour may be biased by the need to maintain "...the appallingly high costs of laboratory research (and academic reputations)...". David Clarke, 30 years ago, had already identified the tendency by archaeologists to develop authoritarian specialists within their ranks, summarised by Clarke (1973, 6-7) in the words:

"This process is also marked by the emergence of competitive individualism and authority, since the individual's living depends on the reputation he achieves as a focus in the media or by innovation and intensive work in a specialist field. The politics and sociology of the disciplinary environment increasingly develop this 'authoritarian' state in which each expert has a specialist territory such that criticisms of territorial observations are treated as attacks upon personalities."

Unfortunately this tendency has been reinforced recently in Britain by the fatuous introduction by the Government of the Research Assessment exercise, upon success in which a large amount of the financial support of individual universities is made to depend, whether in the sciences or the humanities. It may therefore not be irrelevant to note that Knapp has devoted much time and many publications to the Bronze Age archaeology of Cyprus, and especially the organisation of its copper industry (e.g. Knapp 1990, 1996), thereby coming within the scope of Clarke's remarks quoted above. Yet it seems unfortunate that Knapp continually fails to recognise that, as I shall try to show yet again below, the factually based Oxford position on the source(s) of copper for the oxhide ingots and the copper based artefacts from Bronze Age Cypriot sites does not materially differ from his own views about these matters.

SCIENCE and SOCIAL ARCHAEOLOGY

Here is perhaps not the place to dwell overmuch on the relations between science and archaeology, and certainly not on the jejune comments of Jones (1988), Dunnell (1993) or Chippindale (1994), against whose wilder views Knapp (2002, 41) himself warns us. Nor do I disagree that the ways of thinking *within* science are distinctive, do not conform with a common sense view of the world, and are misunderstood by most philosophers of science. Careful discussions, by practising scientists, of such questions have been given by Wolpert (1992) and by Feynman (1992). The nonsense introduced by devotees of the strong programme of the sociology of science, in their ill fated attempts to claim that science is essentially a social construct, has been demolished by Wolpert (1992), whilst the misguided attempts by (chiefly French) sociologists and philosophers to enhance their writ-

ings by the importation of misunderstood or irrelevant science was delightfully exposed by Sokal (Sokal 1996; Sokal and Bricmont 1998).

More relevant to Knapp (2002, 41) is his assumption that Gale (2000) demonstrates "the critical fault line between scientific and archaeological approaches to interpreting the past" and evinces "the deep differences that separate archaeological and scientific thinking on cultural and behavioural issues". Nothing is further from the truth. As stated above, I do not oppose the endeavours of social archaeology as understood by Renfrew, nor in principle any other archaeological way of interpreting the past. But social archaeology, or other approaches of theoretical archaeology, has at base to be rooted in, and interpret, the data gathered by archaeological excavation, survey, and post fieldwork analysis, including various types of scientific analysis of artefacts. Some 30 years ago Clarke (1973) clearly recognised this, and accepted that new methodologies sometimes produce surprising results, when he wrote:

> *"New Observations*
> The array of new and old methodologies have also combined over the same twenty years to produce a multitude of 'surprising' new observations and to detect previously unrecognized sources of variability." and "Once again, epistemological adaptation to the empirical content of the new observations is of no less significance than the explanatory and conceptual adaptation now required to understand them. Even those most complete and finished accomplishments of the old edifice – the explanations of the development of modern man, domestication, metallurgy, urbanization and civilization – may in perspective emerge as semantic snares and metaphysical mirages."

It is this situation which has arisen from the neutral application of LIA to attempting to discover the source(s) of copper used to make the copper oxhide ingots. This approach has led to the, at first sight surprising, conclusion that all post-1250 BC copper oxhide ingots, including those found in Sardinia, were made using copper metal smelted from ores from the Apliki mine and its immediate region in Cyprus. This finding does not depend on any archaeological facts beyond those indicating the rough dates of the ingots and the places of their discovery. This finding, of itself, in no way applies science to interpreting the past, and in no way applies science to hypotheses about cultural or behavioural issues. The "fault line between scientific and archaeological approaches to interpreting the past", averred by Knapp (2002, 41) to be painfully evident in Gale (2001), has no real existence; it collapses into one of the straw men so favoured by Knapp in his attacks on other scholars.

Clarke (1973) was very clear when, in discussing archaeological method, he wrote of:

> "... the essential set of predepositional, postdepositional, retrieval, analytical and interpretive models and theory which all archaeologists intuitively employ in the interpretive leaps from the excavated data to the written report, covering the interpretive process from the grave (or settlement) to publication."

Some will regard this as an outdated view, and will say that archaeological data are 'theory-laden' (Shanks and Tilley 1987, 9), but I think Clarke's view sufficiently accurate for the ingots under discussion. For the oxhide ingots the use of the method of LIA to establish the source(s) of their copper is a post retrieval application of natural science. The scientific conclusions about the copper source(s) which emerge can then to be subjected to archaeological modelling and interpretation, to make interpretations of a cultural, behavioural or socio-economic nature. Natural science stops at the point of establishing the source(s) of copper, whereupon archaeology takes over. Challenges to the source(s) of copper must come from detailed challenges to the natural scientific analyses on which they are based. The cop-

per oxhide ingots have been subjected to excellent and thorough archaeological/typological examinations (Buchholz 1959, 1966, 1967; Bass 1967), but these and other archaeological studies did not throw any light on the source(s) of the copper used in their manufacture. It is for this reason that the methods of the natural sciences, and in particular comparative lead isotope analyses of copper ores and ingots, were used.

Knapp (2002, 38) singles out for special praise the work of Trincherini et al. (2001), which applied lead isotope analyses to finding the source of lead for Roman lead ingots from two shipwrecks in the Western Mediterranean and involved two archaeologists. In view of Knapp's praise for their work, it is comforting for Oxford's position that Trincherini et al. conclude that their archaeological arguments have to be abandoned in favour of the contrary indications of lead isotope analyses, writing (Trincherini *et al.* 2001, 405):

> "But here archaeology is obliged to bow before the data of physics".

Can Knapp follow the logic of his praise for Trincherini et al's work by accepting that, also in the case of the post 1250 BC copper oxhide ingots, archaeological pre-conceptions should bow before the data of physics? Can he perhaps understand that Gale (2001, 125), when he writes that "...it is entirely unacceptable to massage the (scientific) data or interpret it so as to fit in with some preconceived archaeological or social theoretical hypothesis.", intends to convey that the copper source(s) indicated by lead isotope analyses cannot be thrown aside on the basis of archaeological preconceptions based on no evidence? Can Knapp understand that the discovery of new facts from the application of scientific analysis to artefacts does not constitute using natural science to interpret human behaviour or to adjudicate between alternative cultural or social interpretations, but merely provides new facts which can then be used in such archaeological interpretations?

DETAILED POINTS OF CONTENTION

(i) The Source(s) of Mediterranean Copper Oxhide Ingots

Knapp (2002, 38-39) correctly summarises our view, based on lead isotope analyses, that all so far analysed copper oxhide ingots postdating 1250 BC, whether found in Sardinia, mainland Greece, Crete, Cyprus, Bulgaria, Turkey, the Cape Gelidonya shipwreck or elsewhere are made of copper coming from the Apliki copper mine in Cyprus or its environs. Examination of the isotopic evidence by other experts in Mainz, Freiberg, Heidelberg, Bradford, has led them also to accept that the scientific evidence points to this Cypriot origin (Begemann *et al.* 2001; Scaife *et al.* 1999, 131). What Knapp (2002) conveniently omits is that, in his earlier papers (Knapp 1999, 2000), he states unequivocally that we had claimed that all post 1250 BC oxhide ingots were made of copper from the relatively small archaeological site of Apliki-*Karamallos,* which, if true, would have rather different social archaeological implications. Gale (2001) showed unequivocally that we had never made such a claim; Knapp had confused (deliberately?) the large Apliki copper mining region with the much smaller archaeological site.

It is equally misleading, and intellectually dishonest, for Knapp (2002, 39) to imply that we claim a "centrality of the Apliki mine or 'mining region' to the Late Bronze Age economy of Cyprus". This is another of Knapp's straw men, for which he seeks to gain credence by repetition or assertion rather in the manner of Humpty Dumpty or the White Queen (Carroll 1962, 75, 62). We have never made such an assertion; the Apliki mining region is central only as a copper source for the post 1250 BC oxhide ingots. Our existing lead isotope analyses had already shown that a number of other copper mines and smelting sites around the Troodos, of which Knapp's discovery of Politiko-*Phorades* is but one, also supplied copper for Late Cypriot tools, weapons, etc (Stos-Gale and Gale 1994; Stos-Gale *et al.* 1998; Gale 2001, 114-116).

(ii) Interpreting Lead Isotope Analyses

Knapp (2002, 40-41) employs the same straw man technique, using partial quotation out of context, in asserting that Gale (2001, 121-122) opposes quantitative scientific arguments to qualitative archaeological ones, to the detriment of the latter. Careful study of Gale (2001, 121, Fig. 1) shows that he did no such thing; his discussion was intended to show that, based on a quantitative discussion of present LIA for Cypriot ores, mixing of those ores cannot produce the observed LIA for the Apliki region ores or the post-1250 BC oxhide ingots. This does not stand in opposition to archaeological arguments since, as the silence of Knapp on this point suggests, there is currently no archaeological evidence about the hypothesis that ores may have been mixed in smelting copper in Bronze Age Cyprus. Knapp's suggestion that further work on Politiko-*Phorades* will help to provide new answers to the pooling of ore sources in the production of pre-1250 BC copper oxhide ingots (Knapp 2002, 41) seems unlikely, in view of the fact that the current LIA of copper slags from Politiko-*Phorades* (Gale 2001, 121, Fig. 1) are widely different from the LIA of such earlier ingots (see for the Minoan examples Gale and Stos-Gale 1986, and for those from Uluburun see Gale 1999).

More serious still is Knapp's astonishing current insistence (Knapp 2002, 39) that it is mandatory to use multivariate statistical analysis for interpreting the large amount of lead isotope data that needs assessment. Indeed he goes so far as to write (Knapp 2002, 39), in criticising Gale: "In a situation ideally suited for multivariate statistics, in fact one where it becomes impossible otherwise to cope with the amount of data that require assessment, why forsake statistical analysis?" Yet in his previous paper (Knapp 2000, 38), Knapp had written:

> "DFA (discriminant function analysis) or indeed any sophisticated multivariate treatments are now regarded as inappropriate or unnecesary for assessing data generated by lead isotope analysis (Scaife et al. 1996). Moreover, recent work by Baxter and Gale (1998) unambiguously demonstrates that points within lead isotopic 'fields' are not necessarily normally distributed."

In other words, in 2000 Knapp knew well why interpretative methodology had moved on from Knapp and Cherry (1994), and why it is incorrect to use multivariate statistics to interpret lead isotope data, but in 2002 he decides to upbraid Oxford for not using it – might this be a case of deliberate intellectual dishonesty? It was the work of the Bradford school (Scaife *et al.* 1996, 1999), and that with Baxter (an expert on multivariate statistics, see Baxter and Gale (1998)), which suggested that departures of lead isotope fields from normality might imperil numerical probability assignments of data for artefacts to particular ore fields, and that therefore multivariate statistics had to be abandoned. Even before this Knapp had approvingly quoted Pernicka's (1993, 259) view that the simplicity of point-by-point comparison between ores and artefacts in paired two-dimensional scatter plots leads to the conclusion (Knapp and Cherry 1994, 35) that: "There is, in fact, hardly any need for multivariate statistical methods at all". The point-by-point comparison basis (explained by Gale and Stos-Gale 2000, 522-525, Fig. 17.7) is, with attention, comparatively easy to use, and is that also adopted by Begemann, Pernicka, Hauptmann, and their colleagues, and also by Trincherini et al. (2001). How is that simple two-dimensional lead isotope scatter plots become, for Knapp (2002, 39), "intricate and complex lead isotope diagrams"? How is it that Knapp knew and insisted in 2000 that multivariate statistical analysis was inappropriate for lead isotope interpretation, but has conveniently forgotten it in 2002 in order to criticize us for not using it? Knapp (2002) might perhaps (but did not) have appealed to Sayre et al. (2001), as the only group currently adhering to the use of multivariate statistical analysis of LIA, but their Figures reveal how they could far more simply have

obtained the same conclusions from point-by-point comparison.

As to Knapp's insistence that lead isotope analyses can only give negative evidence, and can only exclude ore sources from which the metal artefacts in question cannot have been made, such a dogmatic view was excluded by Gale and Stos-Gale (2000, 522-535), was not a principle followed by the originators of lead isotope provenancing (Brill and Wampler 1967, Grögler *et al.* 1966) and is not the practise of other laboratories (e.g. Pernicka and Wagner 1982; Farquhar and Fletcher 1984; Pinarelli et al. 1995; Sayre *et al.* 2001; Hauptmann *et al.* 1999, 2003), including the study of Trincherini *et al.* (2001) particularly praised by Knapp (2001, 38). All of these laboratories not only use the exclusion principle adumbrated by Knapp (2001, 40), but also point to overlapping lead isotope data as between particular metal artefacts and particular ore deposits as revealing the metal ore deposits actually exploited. They also (except Sayre) use the point-by-point comparison approach found so difficult by Knapp (2002, 39), but which is in practise very simple and has been explained in detail by Gale & Stos-Gale (2000, 522-525, Fig. 17.7) and by other laboratories.

COLLABORATION

Knapp (2002, 37) issues a further call for cooperation and collaboration, by implication on the copper oxhide ingot question. For my part I would be glad to see such cooperation between Glasgow and Oxford, and announce my readiness to respond to his call.

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