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DISCIPLINARY FAULT LINES: SCIENCE AND SOCIAL ARCHAEOLOGY

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ABSTRACT

Noel Gale's (2001) dismissive response to an earlier study on the Mediterranean metals trade (Knapp 2000) is challenged, and brief consideration is given to the main points of contention: source(s) of copper oxhide ingots, interpreting lead isotope analyses, and the possible mixing and recycling of metals. The critical 'fault lines' between scientific and archaeological approaches to interpreting the past are discussed, and a further call for cooperation and collaboration is issued.

KEYWORDS: science-based archaeology, oxhide ingots, lead isotope analysis, metals trade, Bronze Age, recycling/mixing of metals.

INTRODUCTION

In a predictably harsh response to an article recently published in the European Journal of Archaeology (Knapp 2000), Noel Gale (2001) has issued a series of revised, updated, recently changed, previously unpublished and occasionally unethical analytical or intellectual datum points in the attempt to pose a 'scientific' challenge to theoretical viewpoints and social approaches in archaeology. Responding point-by-point in order to correct and clarify all the inaccuracies, misunderstandings and half-truths that permeate Gales' response would be an exercise in futility. Indeed, as Gale himself indicated in his opening sentence (2001, 113): "Yet another article largely based on late Bronze Age Mediterranean copper oxhide ingots and lead isotope analysis may well

seem rather too much, and gilding the lily with a vengeance...." To be sure, one wonders why Gale's response was ever published, since all we have gained is a predictably cliched, often arrogant appeal for a quantitative, scientific approach; a scathing dismissal of social archaeology; and, in particular, a hyper-defensive reaction to the critique of the uses of lead isotope analysis in archaeology, or at least of the way that technique has been applied in the past. In this study, I shall not attempt to explicate yet again some of the problems with the lead isotope technique in general, and with Gale's erratic interpretations of those data in particular. Instead I shall outline the main points of contention between us — sourcing copper oxhide ingots, interpreting lead isotope analyses, the possible mixing and recycling of metals — and discuss what I see as the critical 'fault lines' between scientific and archaeological approaches to interpreting the past.

RECAPITULATING THE ISSUES

The main thrust of my study was to illustrate how, using the example I knew best, the interaction between archaeologists and science-based archaeology has been less successful than it might have been. Indeed, outstanding examples of co-operation and real advances in knowledge have resulted, even in the ancient Mediterranean and using lead isotope analysis (e.g., Trincherini *et al.* 2001). My main concern was to consider how the stated aims of science-based archaeology might be relevant and applicable to contemporary archaeology, specifically to a social archaeology. My primary contention was that scientific analyses do not translate easily into a blueprint for the interpretation of human behaviour and that, as a corollary, such analyses cannot adjudicate 'objectively' between alternative cultural or social interpretations (as many scientists still seem to think). Human behaviour and social realities are indisputably open-ended, nuanced and subject to multiple understandings and diverse interpretations. The past has many parameters and multiple points of view have been engaged in the attempt to represent that past. In my opinion (see also Knapp 1996), we have all — archaeologists, historians, anthropologists, and archaeological scientists — created multiple pasts, reflecting the questions we ask or the answers we expect or wish to see. In this case, data (archaeological or scientific) are arguably open-ended, subject to multiple (social or analytical) interpretations, and require evaluation by close collaboration and interdisciplinary discourse rather than through contestation and defamation.

As a step toward building such collaboration and promoting such discourse, let us examine briefly the main points of difference (rather than the details) between Gale's view and my own view on lead isotope analysis and the Mediterranean metals trade. In so doing, I limit myself strictly to the issues that Gale raised, not those that I might wish to pursue or query in addition.

POINTS OF CONTENTION

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

Gale maintains that the ore deposits of the Apliki region were the source of all copper oxhide ingots produced after 1250 BC (although in 1997 he had stated - and quotes himself [2001, 114, emphasis added] as so doing - that "*all oxhide ingots* so far analysed, dating to the fourteenth

(actually thirteenth) century BC" were made of copper consistent with production from the Apliki mining region and especially from the Apliki mine. For brevity's sake, let us leave aside the chronological discrepancies and the major significance attached to one mine in that earlier study, and instead simply accept this Apliki scenario as Gale proposes. He goes on to claim that all oxhide ingots found on Sardinia (post-dating 1250 BC) were made of copper consistent with production from ores of the type found at Apliki, and presumes that I do not accept his claim. In fact I called the claim into question as much on procedural and methodological grounds as on my belief in its veracity or its socio-economic and cultural likelihood.

Moreover, Gale should be aware that, when the Oxford group's first major study appeared on the possible Cypriot origin of these very same oxhide ingots from Sardinia (Gale and Stos-Gale 1987), despite its controversial character I wrote a lengthy article revamping all previously held views on the subject (including my own) and attempted to develop a possible politico-economic scenario in which we might understand this (at the time) revolutionary 'scientific' development (Knapp 1990). Following Gale's own stipulation (2001, 122), therefore, that archaeological data and independent archaeological thinking should come into play in "interpreting how the provenancing information yielded by lead isotopes alone can be interpreted within an archaeological framework", I re-examined my 'preconceptions' and produced a (theoretical) study in line with the scientific 'facts'. If I have now given up constructing a new scenario each time the 'facts' change, at least I should not be charged with purposefully ignoring analytical data, academic publications or the scientific point of view. Moreover, in my opinion, the question of the centrality of the Apliki mine or 'mining region' to the Late Bronze Age economy of Cyprus remains very much an open question, to be vindicated, modified or discarded as further analytical and archaeological data accumulate.

ii) Interpreting Lead Isotope Analyses

A close reading of Gale's (2001, 118-19) arguments on the changing nature of interpreting the relevant analytical data indicates how a single Cypriot 'field' for lead isotope data was (1) established, (2) modified, using multivariate statistics, to propose distinctions between five specific mines on Cyprus), (3) modified again, discounting the use of multivariate statistics and proposing 'isotopic regions' for the different Cypriot mines, until finally (4) we are told (Gale 2001, 118): "For provenance purposes comparisons must be made on a point-by-point basis between the lead isotope composition of an artefact and of the individual ore samples, eschewing statistics". The Oxford group now have analytical data on hundreds of ore samples and metal objects (including oxhide ingots), and it is difficult to understand what Gale is proposing here. 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? How does one actually compare these data on a 'point-by-point' basis? One glance at the intricate and complex lead isotope diagrams used by Gale (2001, 121, Fig. 1; 124 Fig. 2) to illustrate his points offers a striking example of 'why archaeologists don't care about archaeometry' (Dunnell 1993).

On the one hand, Gale (2001, 119) clearly accepts that there are still multiple,

unknown ore sources that contributed to the production of (pre-1250 BC) copper oxhide ingots. On the other hand, it should be clear that we cannot possibly test all the potential ore sources in and around the Mediterranean region, and so it seems unlikely that this issue will ever be resolved to everyone's satisfaction. As an archaeologist such an irresolvable conundrum is exactly what I expect. Concerning the proper use of statistics in lead isotope analyses, our position - developed as part of long-term project dealing with Mediterranean Bronze Age trade and working in collaboration with the Oxford group - has been stated clearly (Cherry and Knapp 1991; Knapp and Cherry 1994, 32-40) and I see no reason to modify that position. Above all, and following Gale's own early caveats about the proper use of lead isotope analysis, it must be acknowledged and accepted that this technique cannot be used, ever, to assign specific provenance to artefacts, but instead only to exclude possible provenances. No matter how high the (qualitative) likelihood, we cannot assign provenance on the (quantitative) results of lead isotope analyses. I see no way around our specific differences in this volatile arena, and believe it is incumbent upon archaeological scientists involved in lead isotope studies to show why and how we should accept an approach based on the lack of statistical treatment, a 'point-by-point' comparative procedure, and the positive assignation of provenance when scientists and archaeologists alike have always accepted that this technique should only be used in a negative manner, to *discount* possible ore sources.

iii) Mixing and Recycling

This equally controversial issue has challenged the proponents of lead isotope technique from the outset, and it is to the credit of all practitioners, and in particular the Oxford group, that they have responded by developing a range of procedures - statistical and methodological- to silence their detractors. In presenting my own observations on these matters (Knapp 2000, 38-47), I did not voice support for either mixing or recycling vis-a-vis the oxhide ingots, but rather sought to encapsulate the differing opinions and perspectives advanced over the past two decades, and to highlight some of the problems that still remained. Like those whom Gale cites in support of his own position (in particular Pernicka 1995; Karageorghis and Kassianidou 1999), I have always found problematic the hypothesis of recycling artefacts to produce oxhide ingots. The mixing or pooling of copper ore sources to produce ingots, however, and the practice of recycling artefacts or scraps of ore to produce new artefacts, seem undeniable; numerous references attesting to those practices in antiquity were cited in my original article and there is no need to repeat them or the associated arguments here. My ongoing problem with Gale's approach to these issues is the manner in which he expresses his arguments: archaeology is qualitative and cannot resolve such questions; science is quantitative, and can provide the answer:

> "Often a quantitative examination [of the lead isotope technique; of mixing and recycling] would show that the qualitative idea does not in fact withstand comparison with numerical data" (Gale 2001, 12).
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> "It is true that the qualitative consideration of lead isotopes in the abstract cannot resolve this question [of pooling ores], but equally true that quantitative consideration of the relevant numerical lead isotope data *can* resolve the issues, as outlined in this article" (Gale 2001, 122, original emphasis).

Gale (2001, 120) maintains there is no evidence that primary smelted ores (black copper - whether in the form of bun ingots, slab ingots or plano-concave ingots) from different inland origins on Cyprus might have been re-smelted, refined and cast into oxhide ingots for further distribution. The *experimental* evidence he cites in defence of his position is irrelevant: in our excavations at Politiko *Phorades* (Knapp *et al.* 1998, 1999, in preparation) we have uncovered the first extensive evidence of primary Late Bronze Age copper smelting ever excavated on Cyprus, and the (quantitative) archaeometallurgical analyses currently underway will help to provide new answers to at least some of these questions, in particular the possibility of pooling ore sources in the production of copper oxhide ingots (pre-1250 BC).

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In considering each of these issues, the critical fault line between scientific and archaeological approaches to interpreting the past becomes painfully evident. Gale (2001, 122) is quite direct on the matter: "At this [quantitative vs qualitative] level of discussion and interpretation of the lead isotope data, archaeological data has [sic] no standing". In other words, Gale has no doubt that lead isotope analysis, in the guise of science, has resolved at least one of the many problems associated with the origin of the copper oxhide ingots, and that his 'facts' are correct. In total contrast, I have no doubt that ongoing archaeological excavations and archaeometallurgical analyses will turn up new questions and even more difficult interpretative issues associated with the origin of the copper oxhide ingots (not least as a result of our own fieldwork and laboratory analyses), and that the current 'facts' as I see them will be proved incorrect (or in need of serious modification). Therein, perhaps, lies the crux of the deep differences that separate archaeological and scientific thinking on cultural and behavioural issues.

As an archaeologist long involved in collaborations with scientists, statisticians and theoreticians alike, I view with concern if not disdain many of the polemical position statements and commentaries that have appeared in the past several years. I regard archaeology as a soft, social and human science, one rooted in historical methodology and characterised by a fully viable, independent and autonomous body of knowledge that has benefited immeasurably from its (usually) open-minded, open-ended and interdisciplinary outlook. The intra-disciplinary paroxysms that have rocked archaeology over the past 40 years have resulted only in strengthening the epistemological, philosophical, theoretical and empirical bases upon which this field is based. The current (younger) generation of archaeologists, for the most part, routinely and confidently has no hesitation in employing a battery of techniques and approaches from diverse fields to treat empirical data, develop their own social theory, and establish more nuanced and holistic interpretations of the human past. Science-based archaeology, and an increasing number of archaeologists equally well-trained in specific domains of science, are an integral part of this revamped, more dynamic archaeology of the 21st century. Sceptics like Jones (1988), Dunnell (1993) and Chippindale (1994) ignore this new reality at their own peril. Knowledge that

stems from both the hard and natural sciences has the potential to illuminate archaeological questions that range from the local (e.g., dating an artefact) to the global (e.g., the peopling of Australia or the New World).

As a scientist, Gale is unconcerned with social theory and clearly unaware of the many internecine debates within archaeology, and nor is there any particular reason he should be, in the normal course of his work. When that work involves archaeological data or behavioural interpretations, however, he must accept the need for interdisciplinary discourse and not just with those archaeologists who might share his point(s) of view. 'Science in archaeology' is just that: as (McGovern 1995, 79) noted in his recent review of scientific applications in archaeology, "This phrase implies that science of whatever variety (social, biological, physical, etc.) has found its way into archaeology, and it is for us [viz. archaeologists] to decide whether it is producing worthwhile results". Why Gale (2001) now chooses to qualify the scientific basis of lead isotope analysis (negative provenancing) or to dismiss the application of statistics in interpreting lead isotope data is not only puzzling but counterproductive. Perhaps these choices are bound up with the nature of the scientific endeavour, and the perceived need to be seen as advancing scientific knowledge or broadening scientific applications, without which the appallingly high costs of laboratory research (and academic reputations) cannot be maintained (Shapin 1996; Greenberg 2001). Make no mistake: behind many white-coated, high-tech, mass-accelerator-driven laboratories there stands a formidable legion of power, politics and personal enhancement.

What is so special about archaeology, and why must it be distinguished from the hard or natural sciences? Why should its science-based aspects be singled out for critique? One answer, as McGovern (1995, 80) pointed out, is that science-based archaeology suffers from what C.P. Snow (1963) defined as a 'two-culture', liminal, split personality, being neither science nor archaeology. Another, more charitable answer is that cultural interpretations are more abstract than scientific conclusions, and can never attain the same level of confidence. Moreover, it is surely the case, as John Terrell (1999, 671) recently noted, that "...scientists working outside of archaeology and anthropology may get what we write wrong ... because they have a different trope in mind, the trope of a 'crucial element".

Archaeological issues cannot be reduced to essential, alternative hypotheses whose superiority or likelihood can be established by experiments that produce an unequivocal solution. Archaeological narratives and social interpretations thus may seem, to non-archaeologists, simple-minded, contrived or designed to "fit in with some preconceived archaeological or social theoretic [sic] hypothesis" (Gale 2001, 125). The members of different scholarly communities frequently talk past one another, knowingly or unknowingly, because they have not been trained in the manifold and increasingly specialised techniques, approaches, viewpoints, even meta-narratives that characterise and define different disciplines. If I might conclude with another Terrell quote, this time from another intriguing essay entitled 'Anthropological Knowledge and Scientific Fact' (2001, 809):

I argue that real knowledge of how experts in different fields are prepared to understand the world is not trivial knowledge. Even under the best of circumstances, interdisciplinary collaboration can be incredibly difficult. Knowing how scientists miscommunicate is crucial knowledge if experts in different field are to work together and build stronger research collaboratories [sic] in the twenty-first century.

REFERENCES

Cherry, J. F. and Knapp, A. B. (1991) Quantitative provenance studies and Bronze Age trade in the Mediterranean: some preliminary reflections. In N.H. Gale (ed.), *Bronze Age Trade in the Mediterranean*, Studies in Mediterranean Archaeology 90, P. Astrom's Forlag, Goteborg, Sweden, 92-119.

Chippindale, C. (1994) Editorial. *Antiquity*, vol. 68, No. 259, 191-199.

Dunnell, R. C. (1993) Why archaeologists don't care about archaeometry. *Archeomaterials*, vol. 7, 161-165.

Gale, N. H. (2001) Archaeology, science-based archaeology and the Mediterranean Bronze Age metals trade: a contribution to the debate. *European Journal of Archaeology*, vol. 4, 113-130.

Gale, N. H. and Stos-Gale, Z. A. (1987) Oxhide ingots from Sardinia, Crete and Cyprus and the Bronze Age copper trade: new scientific evidence. In M.S. Balmuth (ed.), *Studies in Sardinian Archaeology 3: Nuragic Sardinia and the Mycenaean World*, British Archaeological Reports, International Series 387, Oxford, 135-178.

Greenberg, D. (2001) *Science, Money and Politics: Political Triumph and Ethical Erosion*, University of Chicago Press, Chicago.

Jones, R. F. J. (1988) Questions, answers and the consumer in archaeological science. In E. A. Slater and J. O Tate (eds.), *Science and Archaeology, Glasgow 1987*, British Archaeological Reports, British Series 196(i), Oxford, 286-296.

Karageorghis, V. and Kassianidou, V. (1999) Metalworking and recycling in Late Bronze Age Cyprus - the evidence from Kition. *Oxford Journal of Archaeology*, vol. 18, 171-188.

Knapp, A. B. (1990) Entrepreneurship, ethnicity, exchange: Mediterranean inter- island relations in the Late Bronze Age. *Annual of the British School at Athens*, vol. 85, 115-153.

Knapp, A. B. (1996) Archaeology without gravity: postmodernism and the past. *Journal of Archaeological Method and Theory*, vol. 3, 127-158.

Knapp, A. B. (2000) Archaeology, science-based archaeology and the Mediterranean Bronze Age metals trade. *European Journal of Archaeology*, vol. 3, 31-56.

Knapp, A. B. and Cherry, J. F. (1994) *Provenance Studies and Bronze Age Cyprus: Production, Exchange, and Politico-Economic Change*, Monographs in World Archaeology 21, Madison, Wisconsin, Prehistory Press.

Knapp, A. B., Donnelly, M. and Kassianidou, V. (1998) *Excavations At Politiko Phorades - 1997. Report of the Department of Antiquities, Cyprus,* 247-268.

Knapp, A. B., Kassianidou, V. and Donnelly, M. (1999) *Excavations At Politiko Phorades - 1998. Report of the Department of Antiquities, Cyprus*,125-146.

Knapp, A. B., Kassianidou, V., Donnelly, M., Duffy, P. and Noller, J. (in preparation) *The Excavations*

at Politiko Phorades (1996-2000): The Archaeology and Archaeometallurgy of Mining.

McGovern, P. E. (1995) Science in archaeology: a review. *American Journal of Archaeology*, vol. 99, 79-142.

Pernicka, E. (1995) Crisis or catharsis in lead isotope analysis? *Journal of Mediterranean Archaeology*, vol. 8, 59-64.

Shapin, S. (1996) *The Scientific Revolution*, University of Chicago Press, Chicago.

Snow, C. P. (1963) *The Two Cultures and the Scientific Revolution*, New York.

Terrell, J. E. (1999) Comment to M. Pluciennik, 'Archaeological narratives and other ways of telling.' *Current Anthropology*, vol. 40, 671 [653-678].

Terrell, J. E. (2001) Anthropological knowledge and scientific fact. *American Anthropologist*, vol. 102, 808-817.

Trincherini, P. R., Barbero, P., Quarti, P., Domergue, C. and Long, L. (2001) Where do the lead ingot-sof the Saintes-Maries-de-la-Mer wreck come from? Archaeology compared with physics. *Archaeometry*, vol. 43, 393-406.