

ARCHAEOLOGICAL SCIENCE

Despair, education and celebration?

Andrew Meirion Jones

I would like to congratulate Lidén and Eriksson on their thought-provoking paper. However I should also say that their paper left me with a profound feeling of sadness and despair. I felt despair as – having personally engaged with these issues a decade ago – it seems little has changed in the intervening years.

I like Lidén and Eriksson's description of "the filter", and can report that this doesn't only exist in Stockholm, or Sweden, as I have also experienced "the filter" in British archaeology in conversations with colleagues whom I otherwise respect who claim no knowledge or interest in archaeological science. Equally, I have been alarmed to notice the operation of the filter in scientific analyses in my own study area, Neolithic Britain. From the 2000s onwards the study of the British Neolithic has been characterized by an explosion of scientific analyses, but many of these have expressed a minimal awareness of current archaeological debates, or no apparent awareness of the character of archaeological data. To take one example, discussed by Lidén and Eriksson as exhibit D, the GC/MS analysis of British Neolithic pottery often appears to occur with little correspondence to the context of vessels analysed, and little discussion of the vessel component (i.e. rim, body, base) analysed. In rendering the archaeological material as a scientific sample it has been transformed into scientific "data". It is often difficult to trace a given GC/MS result back to individual vessels in pottery reports, so disengaged has this data become from its parent source. This "data" can,

however, then be employed to make broad statements concerning major social changes in Neolithic Britain; all this without the detailed evaluation of materials and their contexts expected of mainstream archaeology.

Lidén and Eriksson provide a useful list of sins against archaeology, and misuse of archaeological science. The question is, what can be done about this?

I particularly welcome Lidén and Eriksson's point that collaboration and cooperation is the key to a solution, and it's a shame that this was only briefly discussed in the opening and closing sections of the paper, as this deserves more consideration. For me, archaeology is absolutely a polyglot, collaborative and messy discipline (in all senses of the word); this is what makes it so much fun! Collaboration exists in many archaeological projects at various scales. My recent work excavating around rock art sites in western Scotland, while ostensibly concerned with theoretical issues relating to landscape inhabitation, visual culture and questions of ontology, required a suite of archaeological scientists to help me along, including palynologists, soil sedimentologists, lithic analysts, conservation scientists, and radiocarbon dating. This was a relatively small project. Now scale this up and think of the Stonehenge Riverside Project, which was essentially driven by questions relating to materiality, landscape perception and prehistoric ritual and religion. This project has required the help of numerous archaeological scientists, from geophysicists and palynologists, to lithic analysts and faunal analysts, osteoarchaeological analysis of human bone, sedimentological analysis, geochemical analysis of stone, GC/MS analysis of pottery, stable isotope analysis, strontium isotope analysis, Bayesian analysis of radiocarbon dates, to name but a few. All archaeological projects, whatever their theoretical presuppositions, require the input of archaeological science; this is one of the fundamental facts that Ian Hodder established when excavating at Çatal Höyük, Turkey.

If we can all agree that archaeology is a collaborative enterprise, why do we still face "the filter"? In other words, why do we still observe distinctions between archaeologists and archaeological scientists? Is it because the contemporary discipline is so diverse that no single person can possibly master all aspects of the discipline, therefore they tend to diverge to the mainly archaeological or mainly scientific (as Hermann and Wagner suggest in Lidén and Eriksson's opening quotation). Yes, this might be part of the problem, but for me the problem has to be tackled at its root: education. I don't have any insight into the Swedish education system and the archaeology degree, but in the UK school leavers (18 years old) will tend to have studied either a suite of science or a suite of humanities-based subjects at Advanced level, "A level". These

individuals will then enter university to study archaeology, and many universities offer degree tracks in science or humanities-based archaeology, Bachelor of Science (BSc) or Bachelor of Arts (BA). In practice most of these groups of students will be taught together; they are not kept apart, though there may be a weighting towards science or humanities in the specific modules or courses these students opt to study. The fact that these two groups of students are taught together ought to ameliorate the problem. However, we could be more imaginative in the way in which we teach our subject. In my own university students are taught the practical aspects of the subject in the first year, and are introduced to the theoretical aspects of the subject in the second year of the degree, thereby neatly disengaging practice from theory (a disengagement that we then spend the rest of their careers trying to patch together again). This arrangement is common to British universities. The scientific aspects of the discipline will also tend to be taught as distinct elements of the degree as a whole, often chosen optionally by students; again, this will also reinforce a distinction between “science” and the remainder of archaeology.

I strongly adhere to the Popper statement that Lidén and Eriksson end their paper with: “We are not students of some subject matter, but students of problems. And problems may cut across the borders of any subject matter or discipline.” Now imagine if we designed university teaching along these lines! For example, we could have courses that focus on particular issues, such as courses on the Mesolithic-Neolithic transition that not only focused on the archaeological evidence for this, but also focused on techniques of isotopic analysis for analysing dietary change; alongside this, students would be introduced to debates within economic history, which led to the characterization of prehistoric peoples in economic terms, i.e. as “hunter-gatherers” vs. “farmers”. What a wonderful course this would be, and no longer would students be able to claim ignorance of scientific analytical techniques or of the nature of the archaeological evidence and its associated theoretical baggage.

The example I’ve just given would be a fairly specialized kind of course on a specific issue. Alternatively, we might imagine a more general artefact analysis course of the kind taught at most universities worldwide, but one that incorporated the latest theoretical discussions of materials and materiality alongside archaeometric analyses, such courses would remove at a stroke the daft distinctions that have long plagued the discipline, between artefacts as symbols and artefacts as things. Students would realize from the outset that the proper study of archaeological artefacts fundamentally treats them as materials, and that we have a variety of approaches – both scientific analysis and theoretical

analysis – for dealing with archaeological materials. Such courses could sit alongside more traditional methods of teaching so that students did not miss out on in-depth knowledge of either archaeological scientific techniques or of specific subject areas within archaeology.

I now want to shift away from discussing education to considering the practice of archaeology and archaeological science. Lidén and Eriksson discuss a series of case studies of the use and abuse of archaeological science. Of these, Exhibits A and B stand out. In Exhibit A we have research from outside the discipline using poor archaeological knowledge as a cover, or fig leaf, for an otherwise mundane piece of research. In Exhibit B we have an archaeological scientist extrapolating the data thoughtlessly to make bold and unsupportable claims.

In the first instance I am not sure we can do much about this, as the culprit was a geneticist outside the discipline. No amount of opprobrium from archaeologists will affect this researcher. I have almost become immune to what I think of as “Savannah sickness” – the tendency of researchers in evolutionary psychology to attribute all human evolutionary behaviour to our mythical origins as a species on the African Savannah. I’ve read many books dealing with evolutionary psychology, and almost all of them refer to the Savannah at some stage as a catch-all explanation for a particular aspect of human behaviour, but not one ever mentions a shred of archaeological, anthropological or even evolutionary biological evidence to back up their statements. Now as soon as I see the word “Savannah” I mentally throw the book across the room and classify the research as null and void, as poor scholarship. I suggest that exhibit A should be consigned to the same place in our minds. They have signally failed in the most basic aspect of scientific endeavour: getting your facts right.

Exhibit B is more of a problem as the research was conducted within the discipline of archaeology, but the scientific results were used to excessively extrapolate the interpretation. Here I think there is more that we can do. As a discipline we should not focus only on the results of scientific analyses, and assume that these are “only archaeological scientists who do not understand the archaeological source material”. This approach privileges the validity of the scientific analysis as a body of data over other archaeological and anthropological data; this suggests that as a discipline we are too humble over the knowledge claims of our archaeological field data and associated theoretical analysis. Instead, as a discipline, we need to take a *symmetrical* approach and assess the scientific analytical results equally alongside the theoretical interpretation. It should no longer be possible to produce “good science” but “bad interpretation”. Instead, to be worthy of publication, scientific results

ought to be able to be assessed as “excellent” according to the criteria of both their scientific results and their interpretation. If, as argued above, archaeology is primarily a collaborative discipline, it ought not to be possible for any practising archaeologist to claim ignorance in one area of the discipline or the other. Such decisions regarding quality ought to be rigorously made by reviewers and editors of archaeological science and archaeological journals.

The point that Lidén and Eriksson discuss in Exhibit C, regarding radiocarbon dating, interests me. They discuss this in the context of the development of new techniques for dating cremated bone. They note that archaeologists, when they receive a date back from the laboratory, tend to accept or reject a date if it fits the expected date range. Like Lidén and Eriksson, I have always been puzzled by this process of rejection or acceptance based on the expected date amongst archaeologists. Lidén and Eriksson argue that this is a problem that suggests a lack of understanding by archaeologists, and in this context they are probably correct. However, I wonder if this is less of a problem, and more cause for celebration? Curiously the summary rejection of results does not occur in other areas of archaeological science; it only seems to be common practice when dealing with radiocarbon dates. What this suggests to me is that radiocarbon dating has been around so long, and has had such well-reported problems, that archaeologists feel confident evaluating these scientific analytical results alongside the data derived from field archaeology and conventional methods of relative dating, such as typology and seriation. Essentially, what archaeologists are doing when they evaluate radiocarbon dates is the process Alison Wylie (1993) describes as tacking: archaeologists tack back and forth between pieces of evidence seeking a connective and cohesive whole to their interpretations. If a piece of evidence – such as a radiocarbon date – does not fit the picture being generated by this process of interpretation it is rejected.

I recognize that the particular example that Lidén and Eriksson discuss here – radiocarbon dates from cremated bone – constitutes a classic example of bad practice, in which archaeologists do not appreciate the complexities involved in producing dates from cremated bone. However, I do think the fact that radiocarbon dates are routinely produced, evaluated and accepted or rejected suggests a broad confidence in the discipline about this particular branch of archaeological science. I would hope to see in the future a similar process occurring with the results of various other analytical techniques in archaeological science. For example, that archaeologists in the future may be so used to dealing with GC/IRMS analyses of pottery that they question the easy interpretation that pots held milk, vegetable waxes, or whatever. This would herald an

increased confidence in archaeologists' abilities to evaluate this kind of data alongside other aspects such as the type of pot, and its context. Of course, I concur with Lidén and Eriksson that these processes of evaluation should take place through close collaboration with archaeological scientists, and should not be based on prejudice or ignorance.

I began my response to Lidén and Eriksson with a declaration of sadness and despair. I would like to end my response on an upbeat note. While many of the examples of bad practice that Lidén and Eriksson draw our attention to relate to recent publications – which should give us cause for concern – nevertheless it is my belief that things are changing for the better. I have been particularly impressed, and humbled, by a number of recent doctoral and master's level conferences I have attended in the UK, which have been notable for the confidence with which a new generation of scholars is deftly weaving together detailed analytical science with sophisticated theoretical approaches. It is my belief, and hope, that many of the debates that occurred around the epistemological status of archaeology during the 1990s (i.e. questions asking whether archaeology is an objective science or a subjective arts-based subject) are now beginning to bear fruit.

Andrew Meirion Jones
University of Southampton
Highfield
Southampton
SO17 1BFUK
United Kingdom

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