"ARCHAEOLOGICAL SCIENCE : SCIENCE AND ARCHAEOLOGY OR A SCIENCE OF ARCHAEOLOGY?"

John Bintliff
School of Archaeological Science
Bradford University

It is customary in many standard archaeological textbooks, in summarising the history of the subject, to point to a very crucial new phase originating in the decade after the last war. We are told that partly as a result of technological achievements that were a by-product of the war, scientific hardware began to impinge very significantly upon archaeological investigations. To the survey of field remains came geophysics, to the analysis of recovered artefacts a whole range of physical, chemical and simpler optical techniques, to the business so dear to the heart of generations of archaeologists – constructing time/space grids – came absolute dating, most notably C14, and to the storage and processing of excavation data, procedures of an automatic nature employing computers with complex statistical packages. Thus began our era of 'Scientific Archaeology'.

Such an analysis implies that whatever archaeology was before this time, it was not scientific. Moreover, it implies that the kinds of things archaeologists do today that do not directly involve machines to analyse or compute, are also unscientific. But what archaeologist in a report, extended essay or textbook has not striven, and felt he achieved a scientific approach to the collection, processing and interpretation of those matters strictly archaeological – strata, structures, artefacts, archaeological cultures?

With very few exceptions, and these largely in environmental science, practitioners of the post-war 'scientific' archaeology have been trained scientists assisting archaeologists, each community doing its own thing; even at the stage of publication the scientist so defined is generally segregated to an appendix if not to a separate publication. Such scientists then are understood to be so-termed by their methodology and expertise in the core scientific community. Not so our archaeologists who claim to behave scientifically – it is palpably a self-nomination, a claim to be recognised as really deserving inclusion into the core of science. Clearly we require clarification of these contradictions, and such a task is all the more pressing for us here at Bradford, since we are involved in the formative years of a novel degree course, one which purports to be training a new hybrid being, a genuine archaeological scientist, quote unquote.

Because, by and large, we are breeding from pure strains, since our teaching staff are either physicists, or chemists, or mathematicians, anthropologists or archaeologists, it might be thought a matter of some doubt as to how our varied inroads will amalgamate into a single if complex trained mind, at the end of the four year course. Provisionally, however, we are able to report unqualified success in merging both previous experience and present education in both the sciences and the humanities, producing a not obviously schizophrenic arts/science hybrid. It is our view that someone trained both in archaeology and applied science is far more capable of answering problems of relevance to archaeology than a pure scientist dabbling in archaeology.

However, this does not in any way resolve the overall problem of defining the frontiers of science in regard to archaeological activity *sensu stricto*. Let us work from the known to the unknown. Let us accept, then, that those physicists and other trained technical specialists who lend their time and equipment to archaeological problems are acknowledged by all to be certainly scientists. This means that they can be expected to conform rigorously to recognised standards of procedure at every stage of their investigations. The key element is undoubtedly that of scientific methodology. We are brought up to expect of science that data collection and sample preparation are to be strictly controlled, that experiments are to be continually repeated and deviations explained, that only after exhaustive negative testing of alternative interpretations can a provisional interpretation of results be offered – and that tentatively, with customary allowance for sample size, bias, significance levels. Throughout there is an unmistakeable emphasis on quantification, multiple hypotheses, but most particularly on taking the critical reader through each stage of the author's reasoning from observations to conclusions.

I am certainly well aware that such high standards are rather a scientific ideal than universal practice amongst the core scientific community, but it is surely true to say that one judges the value and reliability of a scientific publication by the degree to which it fulfils these requirements.

Now if this core scientific community so defined uses its specific approach and technical facilities for analysing archaeological data – be it artefacts, subsurface features, data sets, or whatever, – it just continues doing what it has always done, is neither more nor less scientific. Let me stress heavily then: simple by sending off material recovered during archaeological activity, or getting a laboratory with its staff brought to the site of such activity, in no way forces on some dramatic metamorphosis of that

archaeological activity which then becomes *itself* scientific. The archaeologist concerned may well, and generally does, continue to excavate and interpret the results of his endeavours in ways traditional to the archaeological fraternity. Let me state one conclusion then: that the postwar revolution is largely an archaeology *plus* science revolution, it has not brought into being a science of archaeology, nor have *most* archaeologists become any more scientific themselves, because of it.

But still, – is the *traditional* activity of archaeologists worthy of the term 'science' in critical comparison with core science as defined above? This question, it seems to me, can only be answered by separating off and treating differently the two major divisions of archaeological activity: the physical recovery and preliminary sorting of data in the field, and the processing of that data for interpretative purposes. If a trained physicist, or mathematician, shall we say, were to analyse the activities involved in these two divisions of archaeology, I think they would conclude, – and some of them already have done, that there is a very great divide between the methodology employed at the data recovery stage and that in use for higher level processing and interpretation. On an excavation, for example, he could be shown the various stages of a logical data collection programme, coupled with meticulous quantitative recording that should enable every stage of the site's dissection to be followed both on site and in the final publication. Claims made for the recognition of major levels and phases, varied structures and features and the basic interrelations of these elements with each other and with associated artefacts, are traceable on site and in the reports to detailed and measurable observations.

In these important respects we must give recognition to the broad similarities between this approach and that assigned to core science. The relationship is not exact, could be and is actively being improved upon on the archaeological side, particularly as regards sampling strategies. On the negative side, it is rare to find an adequate treatment of alternative viewpoints on the site's stratigraphy and structural history. Our core scientist will likewise be amazed to discover that despite many thousands of published excavations, archaeologists for the most part do not concern themselves with a control sample such as a village of the recent past or present day whose abandonment, burial and disintegration would offer a continual reference point. The Overton Down earthwork and similar control experiments are still treated as an extraordinary novelty.

All this aside, we might afford our recovery stage of archaeology a partial measure of, and undoubted trajectory towards, the methodology of true science.

But turning to that other aspect of archaeological activity, a very striking contrast is revealed. The further processing and interpretation of excavated data is most frequently an *art* that rests on intuition and sweeping qualitative evaluations. Our core scientist would generally find no step by step progression from observation to conclusion, demolishing alternative explanations on the way, rather a dramatic and imaginative taking off from the realm of factual description of the excavated material to pseudo-historical reconstructions of events and lifestyles. In this action-packed narrative the reader cannot trace logical paths, and will seek in vain for the testing of multiple hypotheses; the use of significance tests has little meaning where interpretation rests so largely on insights that lack a quantitative or even quantifiable background . The approach adopted can be illustrated in the words first of Sir Leonard Woolley (1930). "The prime duty of the field archaeologist is to select and set in order material, with not all of which he can himself deal at first hand .. Should he not then stop at this? It might be urged that the man who is admirably equipped to observe and record does not necessarily possess the powers of synthesis and interpretation, the creative spirit and the literary gift which will make of him an historian ... As his work in the field goes on however, the excavator is constantly subject to impressions too subjective and too intangible to be communicated, and out of these, by no exact logical process, there arise theories which he can state, can perhaps support, but cannot prove: their truth will depend ultimately on his own calibre, but in any case, they have their value as summing up experiences which no student of his objects and his notes can ever share."

.. then of Sir Mortimer Wheeler (1956)

"The archaeologist ... is primarily a fact-finder, but his facts are the material records of human achievement; he is also ... a humanist, and his secondary task is that of revivifying or humanizing his materials with a controlled imagination that inevitably partakes of the qualities of art and even of philosophy."

It has justly been said of such activity that the reliability of one reconstruction of the past over another cannot possible be estimated objectively from the inadequate attitude to scientific method employed; rather one has to work simply from the supposed academic status and reputation of the author in order to evaluate the likelihood of his reconstruction being 'correct'. As a leading Dutch archaeologist, H.T. Waterbolk (1974) has recently commented: "It is evident that we in Europe undertake a great quantity of work without pausing to consider method and theory. A theory, however poorly founded, is accepted and considered as exact

insofar as colleagues of great renown approve of it." One might object that such reconstructions of the past are usually prefaced by 'it seems likely that', 'we might consider', but it seems clear that *for working purposes* the most popular theories are given the status in archaeology of recurrently demonstrated laws in true science. As Wilson has pointed out: "the difficulty of testing hypotheses in the social sciences has led to an abbreviation of the scientific method in which this step is simply omitted. Plausible hypotheses are merely set down as facts."

In order to teach European Prehistory with current textbooks one has either to entirely suspend disbelief at the endless series of attractive stories that unfold one after another, or spend two-thirds of teaching time trying to extract, to isolate, that small portion of reality that can be justified in a methodical way from detailed observations at the data recovery level.

Firstly, even at the level of sorting out the typology of artefacts, the standard archaeological approach of intuitive but supposedly objective classification has been shown to produce widely differing systems for the same data set for each expert opinion sought. And when artefact groups are combined into the basic building-blocks of interpretation – the archaeological culture, the fundamental uncertainties and unsupported assumptions associated with that term should cause total bewilderment in our scientist considering the importance attached to it in archaeological interpretation. For instance, the general assumption that artefactual cultures = peoples has never been proved and has frequently been disproved. In the Archeulean 'culture', e.g., we are asked to think ethnically about a phenomenon that covered two and a half continents and lasted around one and a half million years: in the five separate Mousterian 'cultures' we are told to envisage five tribes playing the equivalent of musical chairs with each other's caves in south-west France for 40,000 years without mingling; with later manifestations such as the Beaker 'culture' or the Corded Ware/ Battle-Axe 'culture', despite the growing realisation that the relevant artefacts are incapable of standing for a coherent unit Clarke (1968), Shennon (1977)– we still read remarkable fantasies of nomad elites playing migrant ping-pong from one end of Europe to the other. The sober analyses of Collis (1977) and Champion (1975) on current syntheses of the British Iron Age reveal how paper-thin the logic of cultural interpretations remains even today for that epoch. The major opposition between those identifying recurrent continental invasions and those advocating limited influence by diffusion and trade as key elements, rests upon contrasted playing-card mountains of hypotheses, that at no point can be related to a scientific methodology of analysing invasion as opposed to diffusion phenomena as demonstrated recurrently in control samples from anthropology and history.

Such examples could be multiplied indefinitely, but pale before the more imposing super-playing-card mountain ranges that dominate the major reconstructions of later European prehistory. In those recent classic syntheses, e.g., that tell the tale of ancient Europe, or more particularly perhaps of the Bronze Age in Eastern and Central Europe, we read of the surges of steppe nomads, the wealth and power of vast kingdoms, collossal conflicts of East and West, the enlightening visits of Near Eastern sophisticates who raise the barbarians at every critical juncture to a higher degree of achievement, and so forth. Alas, – as our scientist thumbs through the raw data he will find no trace of rigorous argument from such narratives back to adequate supportive material; loophole after loophole he uncovers, and no end to the doubtful assumptions – sadly he would consign the stories to imaginative hypotheses to await serious treatment when our discipline has matured a respectable methodology. In these textbooks, and in another recent classic that aims to inform the general public and the student about Europe before Civilisation, we are also asked to accept without recourse to the discussion and rejection of alternatives, that progress in Europe's past has been largely due to the constant improving activity of an elite minority of gifted people, the upper class in social and political terms, or in more recent jargon – the 'Chieftains'. Despite the insufficient data presented in justification of this extraordinary dogma, which avoids a scientific presentation and argument, this tendentious piece of Conservative propaganda (and note that one author has stood for that august party!) has slipped comfortably into archaeological orthodoxy as well-nigh proven history.

My conclusion is, inevitably, that at the level of interpretation archaeology cannot be considered to deserve the term science. But how has this dichotomy within the subject arisen? Without a doubt because of archaeology's dominant background within the Arts/Humanities. From this origin comes the tacit acceptance that one can teach basic skills in excavation to conform to a general code of procedure, but matters of interpretation must rest upon the natural gifts of the archaeologist. Thus we find ourselves in a curious situation: on the one hand, the archaeological community castigates anyone contemplating excavation but lacking a thorough training in techniques of conducting such an investigation, i.e. a person who does not proceed along struct codes shared by the establishment of professional archaeologists. But on the other hand, given that fundamental expertise and degree of conformity, there is no formal training or even widely recognised critical methodology for the business of interpreting excavated data. Whereas there is general acceptance of archaeology textbooks such as Webster, Coles and Barker, which are in reality almost

exclusively concerned with excavation procedures – there is widespread suspicion if not rejection of reference works dealing with interpretation and reconstruction of the past (e.g. Clarke, Binford and their school). Anyone with basic excavation skills can devote a few months to some neglected region of the world, or a specific artefact type and become the leading expert for such matters, producing reams of articles and solid tomes of cultural history that cannot be objectively judged as statements of fact or estimated probability. Archaeology at the level of synthesis is essentially a superficial discipline; one expands knowledge by covering new areas or time periods, rarely by deepening one's approach. Hence specialists in different eras or countries infrequently, often never, attempt to compare and contrast each other's methodologies and overall conclusions. Laymen rarely dare to reinterpret features of *excavations,* but with broader reconstructions of the past is it not very significant how easily the alternative views of the completely untrained Van Daniken and other members of the 'astro-archaeology' set are confused, if not preferred, by the general public for genuine archaeological *interpretations?*

However, to set against this gloomy description I can remind you that from the early 1960's isolated voices were being raised from within archaeology, pinpointing these specific inadequacies in the subject, and arguing passionately that we do something radical about them. The voices rose in number, and cohered into something calling itself the new Archaeology Of its numerous battle-crisis we might pay most attention to the call for across-the-board application of scientific method to every stage of archaeological activity. This has become inseparably allied to the pressure to extend quantification and statistical tests to archaeological interpretation. The preliminary blasts of the New Archaeology frequently misfired, alienating traditional archaeologists by indiscriminate jargon and obscurantism. But in the later works, e.g., of David Clarke, there was a successful move towards a more accessible statement of the new position, while a whole series of books and papers from the States helped to make the aims and achievements of the New Archaeology better appreciated in Britain. Clarke's school are now providing us with stimulating publications aiming at a quantified logic of archaeological interpretation, of which probably the best known are those on spatial archaeology by Ian Hodder.

In order, then, for Archaeology to merit consideration as on the way to becoming a science, very considerable changes have to occur in the practice and naturally at the teaching stage of the subject. *Every* stage of archaeological investigation must be as rationally argued as that which precedes it; explicit models must be set forth for testing – and wherever

possible quantified for comparative purposes; conclusions concerning the interpretation of patterns in time and space must constantly be related to control samples of respectable size from anthropology and history – a task only just beginning; multiple hypotheses and an holistic approach to causation, allied to a systems approach, should be consistently employed.

The teaching of principles of archaeological interpretation must become as large a part of a degree in archaeology as that of basic recovery techniques, and equal rigour of objectivity and logic be advocated. It is here that we can hope to achieve the training of a true 'Archaeological Scientist', whose activity can bear critical scrutiny as a scientific approach from those within the accepted science fraternity. A traditional archaeologist with an extra hat, is no longer enough. Better to push forward to train Leonardo da Vincis, than Jekyll and Hydes.

References

From *Digging Up The Past,* 1930, quoted by G. Daniel in *The Origins and Growth of Archaeology,* 1967, p.20.

From *Archaeology From The Earth,* 1956, pp.228-9.

'L' Archeologie en Europe. Une reaction contre la "New Archaeology", *Helinium*, 1974, vol.14, p.137.

Wilson 1952, pp.26-7, quoted in L.R. and S.R. Binford, *New Perspectives in Archaeology,* p.14, 1968.

Clarke, D.L., *Analytical Archaeology,* 1968, p.272; Shennan, S.J., The Appearance of the Bell Beaker Assemblage in Central Europe, pp.51-70 in *Beakers in Britain and Europe* edited by R. Mercer, 1977.

J. Collis, An Approach to the Iron Age, pp.1-7 in *The Iron Age in Britain – a review,* edited by J. Collis, 1977.

T. Chamption, Britain in the European Iron Age, *Archaeologia Atlantica,* 1975, vol.1, pp.127-145.