NEW ZEALAND ARCHAEOLOGICAL ASSOCIATION

ARCHAEOLOGY IN NEW ZEALAND

SOME SUMMER READING—THEORY AND PHILOSOPHY IN ARCHAEOLOGY[1]

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When the AINZ editor sent out a call for copy, I responded with the following essay, now aged for 15 years in my files. It seemed appropriate that it might join other retrospective pieces appearing during the celebration of the 50th year of NZAA. The essay was written following a summer (December ’88–February ’89) of catch-up reading on archaeological theory, and was the outcome of discussing my thoughts with Jim Allen, a visitor to the Department of Anthropology in the University of Auckland for a period starting in the March 1989. He prodded me into giving a noonday brown-bag seminar that would place my reaction to aspects of current archaeological theory in a coherent public format rather then hiding behind the veil of a silent cultural historian. I have never been sure about the outcome of the exercise, as discussion lapsed following brief commentary at the seminar’s end. Doubtless this presentation, plus the 1987 *Current Anthropology* paper with Patrick Kirch, put me firmly in the evolutionary theory camp among colleagues—if I was not already seen as residing there.

That might have been the end of the matter, had not one among the editors of my 1996 Festschrift *Oceanic Culture History* obtained a copy of the typed version of the essay that they forwarded to Michael Graves, who had been asked by the festschrift editors to appraise the methodological and theoretical frameworks evident in my publications. Michael came to the conclusion that many, if not most archaeologists, would probably be surprised at the depth and breath of the contribution of those writings to archaeological method and theory. He thought any surprise might be due to reluctance to cast myself in the role of a theorist in the field of archaeology, though it seemed to

[1] This is the text from a lunchtime archaeology seminar given on April 26, 1989, with some footnotes added more than a year (and many subsequent publications) later that has been further reworked as indicated in prologue.

him I had engaged in a relatively coherent and comprehensive application of a certain kind of method and theory over some 30 years. The conceptual framework Michael deduced from my publications he viewed as embracing three major components of evolutionary change. Though he could not know it from those sources alone, this conceptual framework is in fact of longer standing; going back to graduate school at Harvard, and an essay for a course in neo-evolution that served as forerunner to a typed introductory theory chapter for a PhD on the Largo-Gallina region in the Southwestern part of the USA, that subsequently had to be abandoned due to illness.

Because the essay below aided Graves in interpreting my efforts in method and theory in archaeology, it might be sensible that it was more widely available for others exploring the development of method and theory in Pacific archaeology.

Finally, in mid 1990, with a now typed version to hand thanks to Dorothy Brown, who has kindly retyped the present version, I sent a copy of the essay to Robert C. Dunnell for possible comment, as he was someone on whom I had drawn who was deeply interested in and informed about the use of evolutionary thinking in archaeology. Not surprisingly he replied that the essay found a "sympathetic ear", but suggested removing any description on my part of my writings as "atheoretical" [whatever the views of others] as that was patently "inaccurate" by reworking the initial few pages. This I have done in this version. He then suggested publishing the paper and sharing a copy with him, which I belatedly will do. What I found most interesting and encouraging was a central paragraph in his letter:

> Much of my career has been spent trying to unearth the algorithms used by "cultural historians" because it is easy to show some kind of algorithms must have been used to make sense – that they are commonly drawn from common sense or disciplinary lore is what makes them invisible. Strict induction is not possible (e.g., Dunnell 1981, Science, Social Science and Common Sense…). My purpose has not been, as I am sure you are aware, to deprecate cultural history, but to find out why, to the degree culture history works, that it does work. And the result is (e.g., Dunnell 1986, Fifty years of American Archaeology) mostly positive in regard to culture history (and mostly negative in regard to "classical" New Archaeology).

In that light my rejection of new archaeology for a contextual holistic archaeology with co-evolutionary underpinnings derived from cultural history seemed to have some merit. It still does.

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I enjoy reading about theory in anthropology and the social sciences, as well as in archaeology. Yet I find it very difficult to talk about, and almost impossible to write about it, so I hardly ever do. This bit of "kite flying" therefore is somewhat of an experiment, by trying to comment on some recent reading in the philosophy of science as that applies to archaeology and in attempting to understand how my particular interest in historical reconstruction (or culture history) fits into the present-day anthropological enterprise as seen by others.

It is well known that I call myself a cultural historian and often take the opportunity to stick that "offensive" word into things I write and into the titles of many things I have edited. The objective is simple; it means all New Archaeologists and those of other processual and post-processual persuasions can immediately classify and reject my work as purely descriptive. Or perhaps, following my most serious and persistent critic, John Terrell (1987: 447), describe such efforts as "19th century inductivism and atheoretical (even antitheoretical) skepticism of the like of Franz Boas", by "an archaeologist who engages in pre-Darwinian 'just-so' stories or 'narrative scenarios' of dubious veracity." For the New Archaeologists, culture historians like myself are believed to limit our archaeological results to "conventional culture history, to the identification and more or less adequate description of prehistoric types and to their placement in time and space" (Spaulding 1988: 267). We are thought not to engage in theory, we seem not to understand process, and we do not, except by accident, offer explanations of culture change. Therefore, no-one expects to find in the work of cultural historians such as myself, extended discussions of theory, comments on processual, post-processual, contextual, structural, Marxist, cultural idealist or any other of the current archaeologies. This, of course, is a stereotype, though it serves as a convenient cover that avoids being categorised as a proponent of one of the many different theoretically distinguished kinds of archaeology available. It does not mean, however, people like myself avoid theory when we employ process and offer explanations of culture change.

Before I begin this brief emergence from the closet in a burst of theorising, let me make my position quite clear. What I have to say on these matters is, as Schnädelbach said of Karl Popper's adoption of realism, "methodologically without significance" (Outhwaite 1987: 36). As Flannery's (1982) Old Timer in "The Golden Marshalltown" proclaims, one can readily get along without all this philosophising by archaeological philosophers of the '60s, just by doing "real" archaeology. Thus I find great sympathy with Gregory Johnson's (1987: 517) comment on Earle and Preucel's "Processual Archaeology and the Radical Critique", when he says "a dismaying proportion of the literature (since the 17th century) busies itself with (a) all the excellent reasons someone else cannot do what he is trying to do (epistemologically), or (b) all the excellent reasons someone else should not do what he is trying to do (ideologically)." Johnson himself favours a "show and tell" strategy as an operational framework for archaeological inquiry, and "feels uneasy that an abstract and theoretically critical approach has come to outweigh the 'do it' demonstrations in the literature."[2] That is my position exactly. I prefer to do it and let others like John Terrell (1988, 1990), Jack Golson (1986) or Harry Allen (1987), to cite recent examples in the Pacific literature, make of it what they will. John Edward Terrell is currently having a ball, as his recent articles in *Antiquity* and in *Advances* show.

Is my work culture history in the conventional sense of the New Archaeologist? One could certainly argue such a case for "The Cultural Sequence of the Auckland Province" (1962) and for other papers where I set out time, space and content sequences in little boxes. Harry Allen (1987) has persuasively done so. But what of *Archeology on the Island of Mo'orea* (1967) with no boxes at all, just a summary discussion attempting to infer stratification, segmentation, rank and social organisation from settlement pattern data. Or take *Archaeology in Western Samoa* (1969, 1974). To the dismay of those who have to teach or use it, there are no period or stagal boxes whatsoever (cf. Kirch 1988: 7–8, 14). It is, in fact, a demonstration of how to use a settlement pattern approach to reconstruct the history of an island group through a continuous narrative approach. It was deliberately designed theoretically and organisationally to be the exact opposite of Suggs' 1961 *The Archaeology of Nuku Hiva, Marquesas Islands, French Polynesia* where data is organised and summarised within a general evolutionary stagal sequence. Thus except for a typology of adzes, the material is set out in terms of reports on site surveys or individual site reports with everything in its stratigraphic context (not by stage or period), and is all integrated within narrative summaries that have continuity (without pauses and periods) and continuing serial change as their organising structure. Explanations are varied: environmental, ecological, historical (both origins and subsequent cultural contact), social and demographic. Finally, take the five volumes of the Makaha Valley Project. Again there are no boxes, or stagal summaries by period. The enterprise began under one conceptual framework, and in the final volume (Green 1980) I reinterpreted the whole enterprise under another. Moreover, I made sure the final volume never mentioned the words hypothesis or hypothesis testing then so fashionable in

[2] I note recently, and with some approval, that Hodder (1989: 347) himself also sees this as our most urgent task.

Hawaiian archaeology (cf. Cordy 1981). The concern in that final volume was the narrative explanation of how a hierarchical conically organised society became transformed into a class structured society of the Hawaiian *ahupua'a* type. Part of the explanation offered was in terms of changes in the mode of production and the relationships of that production. Marx was not mentioned—and fortunately no one noticed, probably because of the view that I am a "descriptive" culture historian, although one who often seems not to require that approach's fashionable time/space boxes. So what am I?

As David Aberle (1987) set out in his Distinguished Lecture to the American Anthropological Association, "What Kind of Science is Anthropology?" I am one of a small body of *anthropologists* interested in historical reconstruction (in the general sense, not just in terms of archaeology). As he notes, while there seems to be a growing emphasis on the value of history in anthropology, interest in historical reconstruction is currently minimal.[3] However, except among a number of ethnologists and social anthropologists (whom I will discuss below), reconstruction is respected in all the sub-fields of anthropology. For Aberle (1987: 551), as for Kroeber and myself, anthropology is "a historical science which puts it in the distinguished company of cosmology, geology, evolutionary biology and genetic linguistics." Fortunately, Aberle recognises that there are two kinds of science in existence today, one he calls Newtonian and the other, the thermodynamically-based style of science. Historical science fits in the thermodynamically-based kind which concerns itself with such issues as entropy, information and evolution. As he says, for the most part anthropology has not caught up with the new views in these fields.

Fortunately for me, my interests and reading outside archaeology do not make this a problem. I am aware of Prigogine (Prigogine and Stengers 1984) and his science of becoming, dissipative structures, self-organising entities, and open versus closed systems. I know that in astrophysics, physics, chemistry, biology, and culture there is a new way of viewing science which is rapidly gaining the forefront of attention.[4] This does not deal with universal laws of invariant expression that make powerful predictions about a deterministic universe. It is not concerned with finding and applying laws that hold in the past, in the present and in the future. Rather, it is concerned with irreversible, probabilistic, stochastic sequences. Complexity increases as a system grows, whether in size or heterogeneity or both. Evolution is defined

[3] See, however, Patterson (1989: 559-560), who recently documented a case for and need of rapprochement between history and the social sciences, including archaeology.

[4] See, for example, the recent book by S.M. Hawking, 1988.

as growth in complexity. Self-organisation is a fundamental part of nature; the universe as a whole is one gigantic self-organising system, so if we look at the history of the universe, as the physicist Davis says, we see a history of pulsating though not necessarily lineal "progression", starting from a featureless origin in a series of self-organising steps. There are two sources of constraints: one constitutes boundary conditions or natural selection, which selects against some elements in the system; and the second consists of historical conditions, limits on what may come into being, which may also be called initial or inherent conditions. Gould (1986) in his *Sigma Xi* "History Matters" Lecture, distinguished between Science A and Science B, and credited Darwin with inventing Science B, in which history or time must be taken into account. He also noted that Science A was always preferred to Science B as the superior model of science. This Gould doubted. Now we know that Science B is about to provide us with a more profound and historically based understanding of the universe.[5] As Prigogine and Stengers (1984: 213–232) argue, the usual way of thinking about physics and chemistry is in a timeless way. Time as motion. Now we know that it is more complicated and there are more kinds of time to be considered. Moreover, what I like doing, historical reconstruction in anthropology, fits in nicely with that programme.

As the physicist Davis was reported as saying in a recent *New Zealand Herald* interview by Gilbert Wong, we now know that randomness, or chaos, can result in a direction, and what we see in the biological and cultural world, a growth in complexity or in increasing levels of organisation is also present in the physical world. Direction is not a problem, despite the second law of thermodynamics, which says that in any system of molecules there is a natural tendency for disorder, or entropy, to increase; e.g., sugar in a cup of coffee. This is true of crystals and other closed systems as in the Newtonian universe, but it is not true of open systems which take in energy from outside sources to build and maintain internal order, and export disorder as a waste product.

This brings me to my first philosophical question; whether I accept the naturalist or anti-naturalist position in science. I have always thought of myself as an anti-naturalist. I read my Nagel (1961), *The Structure of Science*, and in general rejected it. He argued that the underlying structure of all science was the same, and the same rules and methods applied to all physics, chemistry, anthropology and history. He taught me that functionalism in anthropology was a disaster area (Nagel 1961: 520–535), a thoroughly "unscientific" enterprise. But like many others, I came to the conclusion the social

[5] See, for example, the recent book by S.J. Gould, 1989, *Wonderful Life, The Burgess Shale and the Nature of History*, especially pp. 277–291, where these matters are set out at greater length.

sciences and biological sciences were somehow different, as were geology and palaeontology in which I was also trained, along with biology. It was not until I learned more about realism, and Roy Bhasker's claims against positivism, that I dared hope this need not be the case. Now I know that a great many who have rejected a positivist account of science often correlated with the classical views of physics, believe that realism may extend not only across the social sciences, but also into the biological and physical sciences. I owe this understanding to William Outhwaite and his book (1987) *New Philosophies of Social Science: Realism, Hermeneutics and Critical Theory*. But my introduction to realism as the most likely current alternative comes from Chalmers (1982) in his book *What is this Thing Called Science?* in which the last two chapters deal with these matters.

I was further encouraged in this direction by Guy Gibbon's (1984) wonderful book, *Anthropological Archaeology*, which explored the naturalist and anti-naturalist alternatives; and positivism, realism and conventionalism in his final chapter on "Is a Science of Archaeology Impossible?" To give you some flavour of what I am talking about, I will skip the positivism section, about which you all know, and quote his statement on a realist's view, as well as a brief description of what he terms conventionalism:

> Both research programs assume as methodological principles that science is objective, empirically based, and rational, and both share a common interest in formulating general statements, building theories, accumulating knowledge, and following the general guidelines of a "logic" of science. But there are at least two major differences between realism and positivism. First, realists emphasize explanation as a more primary objective of science than prediction because they believe in the existence of underlying structures and mechanisms that work to produce necessary connections between observable phenomena. An essential activity of science, according to realists, is the accumulation of knowledge of the often unobservable underlying structures and mechanisms that causally generate observable phenomena. Second, realists explain events and entities by showing not only that they are instances of well-established regularities but that they are instances of necessary connections between phenomena as well.
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> Realists' emphasis on making causal explanations through the discovery of underlying structures, mechanisms, and "essences" has often meant the postulation of existence of types of entities and processes that we cannot see in the ordinary sense of that term. Unobservable theoretical entities such as a culture or a social structure are regarded as real in much the same way that we regard a stone or a house as a real. Realists, then, make ontological commitments to theoretical terms in a manner that positivists find mystical and metaphysical. However, re-

alists argue that this is an essential scientific strategy, as exemplified by such terms as *force* and *gravity,* for observable regularities can only be meaningfully explained by going beyond mere appearances to the underlying nature, mechanism, or essence of a situation.

Positivists, by analyzing only the superficial, apparent aspects of the natural and social worlds, fail to discover those deeper mechanisms and structures that cause observable natural and social phenomena. Most cross-cultural studies are misdirected and trivial for the same reason: they concentrate on the superficially observable rather than the deeper underlying substratum of social life. This same basic disagreement between realists and positivists is apparent too in their differing conceptions of the role of models in the research process and in their interpretation of scientific theories. For a realist, a model attempts to depict and transfer actual mechanisms from one better-known realm of study to another. The positivist, on the other hand, typically regards models only as heuristic aids that help one better grasp or represent a theory. Realists construct scientific theories consisting of general statements that describe the structure and mechanisms that causally generate the observable phenomena that are the subject of the study, while positivists adopt an instrumentalist interpretation of theories.

Even though most sociocultural anthropologists who believe that a science of anthropology is possible tend to be realists, a realist conception of science has been greatly overshadowed in anthropological archaeology by positivism. An example of a realist position in archaeology is Childe's Marxism. More recently, a realist structural archaeology has openly challenged positivist conceptions of archaeology in Europe (…). Within anthropological archaeology, James Deetz (1967, 1977) probably comes closest to exemplifying a realist perspective.

The Conventionalist View

Conventionalist is a general term for a diverse group of scholars who are joined more by their rejection of many features common to realism and positivism than by a shared set of views about the process of science. What they basically reject is the general realist and positivist conception that science is an objective, rational enquiry that aims at true explanatory and predictive knowledge of an external reality (…). In rejecting this general conception of science, conventionalists are united in rejecting (1) the idea that things exist in the external world independently of our beliefs and theories about them, (2) the idea that scientific statements and theories must be or even can be objectively tested and compared by experiment and the observation of empirical evidence, and (3) the idea that there are external and universal standards of scientificity that are independent of particular substantive theories and explanations. (Gibbon 1984: 391-393)

This confirmed what I already knew from Marilee Salmon's book (1982) *Philosophy and Archaeology.* As Spaulding (1988: 267) comments, "one result of the heady promise of the New Archaeology was a certain amount of over-stimulation of some of its adherents… It is my impression that these youthful excesses have pretty much passed away owing to the re-emergence of ordinary prudence and to the helpful ministrations of some professional practitioners of philosophy of science (Marilee H. Salmon's [1982] book summaries the situation neatly)." Salmon shows that if we wish an archaeology of science it is unlikely to come from positivist hypothetical-deductive methods of confirmation which she rejects, but that a statistical–relevance model, which can accommodate functional explanations, might well be appropriate. However, the greatest use of her book for me was the development of analogical arguments to establish the prior probabilities of hypotheses or models that we may wish to subject to testing. I know many of you think my paper on Lapita models (Green 1982) is about various Lapita models and their utility in explanation of data about that cultural complex, but as Matt Spriggs (1987: 282) has noted in his *Journal of the Polynesian Society* review of John Terrell's book, it is actually about prior plausibility, the philosophical question of which models it is reasonable to examine when exploring any theoretical proposition and subjecting it to the supporting evidence derived from one's knowledge of the world.

I now feel more sure than previously that positivist accounts of science are faulty after reading *Archaeology and the Methodology of Science* (1988) by my long-time colleague Jane Kelley and the philosopher Marsha Hanen. I recommend the book to every archaeologist who wants to know about Kuhn, Popper, Hempel/Oppenheim and the D-N (deductive–nomological), I-S (inductive–statistical) and D-S (deductive–statistical) models, or Braithwaite and his more relaxed notions of 'Scientific Explanation.'[6] Importantly, Kelley and Hanen (1988: 166–167) don't like the recent tendency to realism in archaeology, and take a fairly anti-realist position (1988: 216, 269–271). They say there is no reason for archaeologists to suppose that they must accept realism as the only framework within which to make sense of science, or even that archaeologists must have a firm view about how the controversy over scientific realism is to be settled, in order to make systematic or theoretical progress in archaeology.[7] However, from my reading of Outhwaite and Chalmers, both enthusiasts for the "realism" position, I am not sure that realism isn't the right way to go, especially if I want to talk to a fair number of my social

6 See the review by Gibbon (1990), where he more forcefully makes this same recommendation.

7. Gibbon's (1990: 188-189) discussion of this point in his review is most informative.

anthropological, sociological and social science colleagues.[8] What I am comforted by in all this philosophical reading is that I no longer need worry about my own or others' rejection of the New Archaeology and its positivist underpinnings, nor do I need to retreat into the anti-scientific position of the conventionalists, or the more extreme positions of various post-processualists (O'Meara 1989).

This brings us to Albert Spaulding's (1988) Distinguished Lecture to the American Anthropological Association, "Archaeology and Anthropology." Here he says archaeology is not a science, nor is it history or an humanity; instead it is a technique or group of techniques, a way of recovering knowledge about past human activities through the material remains of those activities. One way of using this data (but not the only way) is to provide information leading towards a reconstructed and dated ethnography of a prehistoric community. This kind of application of the archaeological method he sees as both science and anthropology. Spaulding defines science along quite conventional lines. He sees Radcliffe-Brown's social anthropology as quite conventional science. Yet he says Leach, like Evans-Pritchard after the mid-1950s, now rejects Radcliffe-Brown and thinks of social anthropology as an art rather than science and takes his place on the idealist side of the "empirical/ idealist dichotomy." Still, Spaulding (1988: 266) doesn't find Leach really anti-scientific in any fundamental way. Finally (1988: 266) he is:

> troubled by the implicit or explicit assumption here and elsewhere that social anthropology is a self-contained topic, first because he judges it to be untrue, and second, because archaeological research on prehistoric communities is not likely to produce any useful information on social structure itself. [Here I would argue, he may be wrong]. Indeed, if social systems are self-contained, there can be no indirect information on their character. The conviction that prehistoric ethnography cannot contribute anything to social anthropology (and vice versa) [he believes] is the prime reason for the creation of departments of archaeology.

He then goes on to Hodder, whose statements he finds alarming. Yet, by applying Spaulding's use of hermeneutic principles to these, he finds under the surface a more familiar sort of archaeology, one which is a manifestation of scientific research on the data of anthropology. He (1988: 268) thus describes Hodder as coruscating on thin ice, but not unscientific.

8. See Patterson (1990: 193) on the point that all three post-processual archaeologies which he identifies hold realist positions, largely through assertion, although these are potentially contradictory to their adoption of hermeneutic principles!

Spaulding's darkest thoughts are for those who take a conventionalist position, at least as I understand it. He believes a scientific cultural anthropology is quite possible, but he is concerned for its future. To quote, "If we are convinced that the wave of the future is the development of the constellation of interests pointed to by such labels as epistemological relativism, postmodern anthropology, hermeneutic anthropology, and others, then the future of a scientific cultural anthropology is dim indeed. In fact even its past vanishes" (Spaulding 1988: 269). They reject the scientific model with its appeal to objective truth and explanation by covering generalisation, for a model of literary criticism together with echoes of German historical philosophy (hermeneutics and *Verstehen* philosophy come to mind). Spaulding (1988: 270) therefore concludes "a scientific archaeology cannot live with a hermeneutic anthropology and yet [he] can't visualise a hermeneutic archaeology". Nor does he believe that scientific cultural anthropology can co-exist with the postmodern brand.

Thus in *total agreement* with the postmodernists, Spaulding comes to the regrettable but obvious conclusion that there can be *no* easy accommodation of the scientific and hermeneutic intellectual frames, because the latter is fatally damaged by its denial of objective truths and the possibility of scientific anthropology.[9] For him it is not anthropology in any reasonable sense of the term, and this leaves it in outer darkness, though as Levi-Strauss has clearly demonstrated, it is an excellent platform for literary endeavour and social philosophy.

Earle and Preucel, in their 1987 paper, "Processual Archaeology and the Radical Critique", by which they mean Hodder's ideas of contextual archaeology built on the idealist notions of Collingwood, Giddens and Bourdieu, plus what they call structural Marxism, come roughly to the same conclusion. As they say, after rejecting scientific objectivity as a false and misleading goal for archaeology, the problem rests ultimately in the cognitive, relativist position adopted by both contextualists and structural Marxists, and is debilitated by the lack of an explicit methodology. Radical analysis of the past seems to amount to "thick descriptions" incapable of independent replication.

With this background, I present the following table, taken from Susan Kent in the (1987) book she edited on *Method and Theory for Activity Area Research, an Ethnographical Approach*. The book contains some important papers, but my interest focuses here on her chapter, "Parts as a Whole: A

9. See O'Meara (1989) for pointed discussion of the various postmodern rejections of the possibility of anthropology being an empirical science. Archaeology and anthropology can and do have an empirical basis which is what allows them to be scientific and not mere inventions of the investigator.

Critique of Theory in Archaeology", and her Table 11.1 (Kent 1987: 517) in that chapter.

Table 1. The four most common theoretical orientations in archaeology as seen by Kent (1987: 517)

Theoretical	**Epistemology**	**Critical Factor**	**Focus**	**Emphasis**
Cultural materialism/ ecology	Positivism/ empiricism	Environment/ economy	Explanation	Economy/ behavior-ecology
Cultural idealism	Idealism	Cognition/ meaning	Understanding	Cognition/ symbolism-psychology
Marxism	Rationalism	Economics/ politics	Explanation	Stratification-power/economics
Structuralism	Realism	Pattern/ relationships-interrelationships	Understanding	Underlying models of cultural material/culture-behavior

As can be seen, there are few surprises. The four most common theoretical orientations are indeed present. We have Binfordian positivistic, empirical New Archaeology; Hodderian cultural idealism; Sahlinian historical-structural realism; and any kind of Marxism from the vulgar to critical theory. Only the designation rationalism remains unexplained. By this Kent means that the theory of knowledge for Marxism partakes of a belief in natural and universal power struggles, an orientation based on Western industrial societies and never convincingly demonstrated as appropriate for studying non-Western ones, at least in her opinion.

Her views are catholic. As she says, judging from the present, the past was complex enough and our current knowledge of it incomplete enough to warrant more than one theoretical orientation in which to view and understand the past. However, Kent also thinks some orientations are more robust and productive than others. Up to this point, she and I are in strong agreement.

If we are to be catholic, however, what approach is missing? By now, most of you know me well enough to guess. In a word, evolution. As the 1987 book edited by Michael Schmid and Franz M. Wuketits makes only too clear, evolutionary theory in the social sciences is alive and well. It is not only possible, but may be theoretically justified, as papers in the book by Schmid and Wuketits indicate.[10] Thus I will end with an excerpt from a favourite archaeological theorist peering into archaeology as it will be in the 21st century.[11]

10. Its recent justification in archaeology is attempted by Mithen (1989).

11. See Hill (1989) for an alternative position of the future in archaeology.

Outwardly, archaeology is a robust and healthy discipline. There are more archaeologists employed today than ever before; there is more money devoted to archaeological fieldwork and analysis than there was only a decade ago. Archaeologists have borrowed advances in the natural sciences and technology to great advantage.

These gains are real and substantial. But intellectually, archaeology is in deep trouble, trouble that has been brewing for more than a century and has now reached crisis proportions. Beyond this grave threat are defects in archaeological theory and method. (Dunnell 1989: 63)

There have been various reactions to this problem. Many archaeologists committed to a scientific approach have retreated into subdisciplines like geoarchaeology, zooarchaeology, and archaeometry, where non-archaeological scholarship supplies the essential theory. Others have retreated into interpretive "schools" of archaeological theory. We now have ecological, economic, and population pressure approaches, to name a few, but no compelling reason to prefer one over another. For the most part, these different schools continue the traditional aspiration (pretense?) to science, but in the past decade some (the structuralist and symbolic approaches) have abandoned scientific methodology altogether. However, we want to characterize its parts, archaeology in the 1980s is in greater disarray than ever before, and its century-old commitment to science is weakening. How we now deal with this problem – the lack of a scientific basis for interpreting the archaeological record – will decide the course of archaeology in the next century.

The future of archaeology may lie with evolutionary theory. Archaeologists and anthropologists have now taken the first steps in reworking evolutionary theory by redefining our concept of culture as a trait transmission process analogous to genetics. The major features of biological evolution derive from the integration of Darwinism and genetics in the 1930s, genetics supplying the mechanism of trait transmission lacking in Darwin's formulation. In the case of human beings, however, most significant trait transmission is effected not genetically but culturally. While culture traditionally was taken to be a *configuration* of traits (beliefs, language, technology), in evolutionary theory it becomes a *mechanism* by which traits are passed on from generation to generation.

We cannot, however, use this new approach simply to reinterpret the results of earlier studies. This is because archaeologists have traditionally pursued a typological view of the archaeological record, classifying artifacts and assemblages into types or kinds. Since these typologies are based on the similarities among artifacts, they tend to

> suppress the recording of variations in favor of recording similarities. Because mechanisms of evolution, such as natural selection, operate on variation, our archaeological "facts" – the descriptions of artifacts and assemblages that we have developed – are flawed. Redescribing the archaeological record is not only an unsettling prospect, it is a daunting physical task. It must be done, however, if we are to have a scientific archaeology grounded in evolutionary theory. (Dunnell 1989: 64)

To echo Hill (1989: 20), "I believe we will begin to see increased effort put into refining our evolutionary theories, and attempts to test them." New understanding in archaeology will follow the tenets of Science B, use narrative (verbal) models of explanation, and deal more effectively with the contingent aspects of historical development. The law and order mode within a covering generalisation that forms the approach of a Science A type processual archaeology based in predictive cause and effect explanations will give way to a Science B variety more in keeping with evolutionary models. Archaeologists (and historians) will recognise their physical records do not really allow reconstructions of the short term "standard" ethnographic type, but usually document types of behaviour accumulated over much longer intervals, and thus it will be reconstructions of the long duration type, and not "palaeo-ethnographies" that will prevail as the objective of most research in the field of historical reconstruction to which archaeology is firmly wedded.

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