2

Positivism and the 'new archaeology'

'Recipes for the Good Society used to run, in caricature, something like this -
(1) Take about 2,000 hom. sap., dissect each into essence and accidents and discard the accidents.
(2) Place essences in a large casserole, add socialising syrup and stew until conflict disappears.
(3) Serve with a pinch of salt.' (Hollis 1977, p. 1)

'The wish to establish a natural science of society . . . probably remains, in the English speaking world at least, the dominant standpoint today . . . But those who still wait for a Newton are not only waiting for a train that won't arrive, they're in the wrong station altogether.' (Giddens 1976, p. 13)

Introduction

How should archaeologists come to have knowledge of the past? What does this knowledge involve? What constitutes an explanation of what archaeologists find? This chapter considers the answer to these questions accepted by the 'new' archaeology; it considers epistemological issues raised by a study of the past in the archaeological literature post-dating 1960. New archaeology has embraced explicitly and implicitly a positivist model of how to explain the past and we examine the treatment of the social world as an extension of the natural, the reduction of practice lo behaviour, the separation of'reality', the facts, from concepts and theories. We criticize testing, validation and the refutation of theory as a way of connecting theory and the facts, emphasizing all observation as theory-laden. The new archaeology polemically opposed itself to traditional 'normative' archaeology as a social *science* and we begin the chapter with a consideration of this change and why it took place. The tone of this chapter is again critical. We argue that existing positions in the archaeological literature are inadequate at an epistemological level and fail to offer much insight for a study of either past or present social processes and their relationship to material culture.

The new paradigm - or traditional archaeology resurrected?

In the early 1960s what subsequently became known as the 'new archaeology' was born. Initially this was an American development originating with the work of Binford and a famous paper 'Archaeology as anthropology' (Binford 1962) and followed by a series of influential articles (Binford 1972). The British expression of the new framework was soon articulated in the works of Clarke (1968) and Renfrew (1972). It is difficult to assign any precise dates but by around 1972 a new hegemony had been established in

archaeology and new archaeology which had once been unorthodox became accepted b\ many Naturally there were some who dissented (Bayard 1969, Hawkes 1968, Trigger 1968, 1970) but these failed to offer any clearly articulated or acceptable alternatives, at least to those inspired with the revolutionary fervour of the new archaeology

Probably all archaeologists would now agree that there have been major changes within the discipline but whether these amount to a revolution or anything really substantively new is a matter of personal belief or conviction and it is unlikely w hether it can be established in any conclusive manner Although profound disagreement is registered in this chapter and in the book as a whole with \irtually e\ery major tenet forming the 'project' of the new archaeology' we w ish to state that we do believe it to be a very significant development and to be of paramount importance This has nothing to do with *content,* with what archaeologists have either said or done in specific detail It rather concerns the act of saving itself

Renfrew (1982a) has aptly described the period from 1900 to 1960 as the 'long sleep' of archaeology, a period in which the aims, procedures and nature of archaeology as a form of intellectual inquiry into the past were not explicitly discussed except in the work of a few scholars, most notably Childe and Taylor, and certainly did not promote man\ major debates The significant feature of the new archaeology and one that remains of abiding importance is that a debate and discussion began, not among one or two, but within a whole community of scholars of what archaeology might be, what it could and could not do, how the past might be conceptualized and whether an objective knowledge of it was possible and in what way In short it not only became respectable to theorize but this was seen as being essential for any development to take place A large literature on theory developed, entire books being devoted to the subject, whereas few had existed before To theorize was no longer regarded by mam as an unusual activity, an adjunct to the real business of doing archaeolog\, but an integral component of archaeological work If anything can be said to be truly different or revolutionary about the new archaeology it is this However, the content of w hat was said is another matter

Kuhn's influential work (1970) on scientific revolutions has been used by archaeologists to promote the view that a paradigm change has taken place and citation analyses have been used to lend empirical support and legitimate these claims (Sterud 1978, Zubrow 1972, 1980) To specify exactly what has changed and why it has changed has pro\ed difficult in practice ^Meltzer 1979) A whole host of specifics can be named such as the use of quantitative techniques, the notion of s\sternness or a deductive emphasis in research but these can all be either traced back to the archaeology which went before or can only be assigned 'newness' as a result of confused reasoning For example, an emphasis on deduction in research has been opposed to a former inductive procedure (Hill 1972) However, this supposes that one could do one without the other

Naturally all scholars like to think that advances are being made, and have been made, in their field, but it seems to ha\e been crucially necessary to new archaeologists to mark out their work as being radicall\ different from what had gone before and legitimate it as (I) actually being new (n) as representing progress Clarke (1973j identified the emergence of disciplinary consciousness as being the most promising feature of the new archaeology, the 'prize' being the possibility of radically expanding

disciplinary horizons, developing alternatives to existing practices, and in so doing controlling the 'direction and destiny' of the discipline (ibid , p 7) To Clarke this implied that the 'innocence' of traditional archaeology had been finally dispelled The reasons for the change in archaeology have been related to a crisis in archaeological thinking, but what exactly did this crisis consist in and wh\ should it occur in the 1960s'[1] Was the perceived crisis merely a chance development^ Hill (T972, p. 61) suggested that the crisis resulted from the failure of traditional theories and methods to solve archaeological problems Precisely the opposite view was taken by Leone (1972, p. 21) who related the problem to the very success of traditional archaeology Leone took the ma|or goal of traditional archaeology as being to provide an outline of what had happened in the past As this outline was now available what were archaeologists to spend their time doing-' Trigger (**1981**) has attributed changes in archaeological thinking to wider social changes and attitudes over which archaeologists themselves have no control. On this account changes in archaeological theory and practice merely mirror the wider social context Most recently, according to Trigger, the idea that technical progress would solve all social problems has been replaced with a profound pessimism and despair more or less directly expressed in Renfrew's use of catastrophe theory to explain social change (Renfrew 1978aj Whether or not Renfrew's outlook on life is indeed despairingly pessimistic is not for us to sa\ but Trigger's formulation is hardly adequate It relies on a conception of society as being a normative consensus between individuals and thus the theories of archaeologists, perforce, reflect the general outlook on life held at any one time Taken lo its logical extreme Trigger's position credits archaeologists with a non-active intelligence in which they are only capable of reflecting the social conditions of their existence rather than challenging them or attempting to change them

The contradictory views of Hill and Leone with regard to the reasons for the crisis in archaeology might, of course, be resolved by simply stating that some archaeologists believed that ihe\ had accomplished all they could by utilizing traditional theories and methodologies while to others there was a feeling that nothing of interest or importance could be established We would suggest that perhaps a more plausible reason for the development of the new archaeology is fundamentally to do with a drive for prestige and power, but on a disciplinary basis rather than in terms of individuals *per se* During the last two decades archaeologists have distanced their work from history, conceived as being particularistic and ideographic in character, and have characterized it as a nomothetic generalizing 'hard' science Science, with a capital S, is the key word for understanding recent developments in archaeolog\. Why did the majority of archaeologists want to don the antiseptic white coat' This would seem to involve the acceptance of the myth of the supremacy of science as the ultimate mode of human understanding, the scientist as an heroic figure dispelling myths with incisive rationality. Given the increasing dominance of science and technology in contemporary society, to be cast in this image was to gain intellectual respectability and power, the power to be gained by producing or purporting to produce objective knowledge relevant to the modern world (Fritz and Plog 1970, p 412), relevance being conceived in terms of both ethical and political neutrality and thus inherently conservative (Tilley 1985, Chapter 3 this volume). The new archaeology began optimistically; archaeology, it was asserted, could

be anthropology but the limitations of traditional archaeology with respect to being able to achieve a penetrating understanding of the past were recreated as a result of the advocacy of positivist science. Drunk on Hempelian whisky and functionalist cognac the new archaeology has regressed to being able to say little more about the symbolic and social other than that which can be reduced to the effects of the technological and the economic, the initial rungs of Hawkes's ladder (1954) beyond which traditional archaeologists did not care to venture, except in rare moments of secondary speculation. It is towards a critique of the nature of the epistemology and methodology adopted in the new archaeology that the remainder of this chapter is devoted.

Grounding knowledge claims: positivism

New archaeologists not only opposed scientific aspirations to an historical understanding, but deduction to induction, positivism to empiricism. Appeals were made to the positivist philosophy of science as the royal road to success. The goal was no longer to describe the past but to explain it and explanatory structure became a central concern. Almost immediately a split developed between those archaeologists who considered their task to be the formulation and testing of laws and those who favoured functionalist explanations in systemic terms (for a critique of the latter see Hodder 1982a). This schism was first explicitly identified by Flannery who made a derisive attack on the 'law and order' archaeologists. Since the 1970s the two lines of approach have continued to be taken but the general failure to formulate any laws going beyond the trivial has resulted in a situation in which few archaeologists are now willing to commit themselves to such an approach. Common to both groups is an emphasis on the need to make generalizations and the belief that deductive testing against the archaeological record is the only way to ensure the objectivity and validity of statements made about the past.

Both those who advocated the use and formulation of laws and the functionalist systemic theorists singularly confused positivism and empiricism. Ironically they replaced empiricism with positivism, one of the most stringently empiricist philosophies to exist, and such was the general confusion that this was not recognized. A commonly accepted philosophical description of positivism is that it is systematic empiricism. Furthermore, those archaeologists who considered themselves to have totally rejected empiricist positions based their work on a very narrow reading of the philosophical literature. First, they only referred to the philosophy of science (understandable in that they wanted to become scientists), secondly, it was more or less assumed that a general consensus existed in the philosophy of science and the work of a few positivist philosophers of science, especially that of Hempel, was transferred wholesale with little or no critical consideration. Positivist doctrines were transferred to archaeology at a time when many philosophers were rejecting virtually every major tenet on which positivism was based. The unfortunate spectacle is one of archaeology embracing thoroughly discredited and outmoded ideas as the framework for its own advance. Curiously enough this trend has continued and the papers presented at the recent Southampton conference (Renfrew, Rowlands and Segraves (eds.) 1982) by those professional philosophers called in to 'advise' archaeologists, with the exception of Gellner's paper (Gellner 1982), all took a positivist, if diluted positivist line. Similarly the only semi-professional philosopher to

have written a book with relation to archaeology (Salmon 1982) has retained a positivist position. This does not mean that positivism is alive and well as a viable philosophy but rather illustrates the adage that old traditions die hard. This is particularly unfortunate if it really is the case that archaeologists will believe anything told to them by a philosopher (Flannery 1982, p. 277). A current frustration with philosophy, and theory more generally, is represented by Flannery's recent paper (ibid.) and remarks made by Schiffer (1981). Flannery recommends a return to the common-sense real business of archaeology, i.e. a solid empirical culture history, while Schiffer suggests that archaeologists distance themselves from philosophical concerns which have no direct practical (i.e. methodological) implications for carrying out research.

Precisely the converse viewpoint is taken here. Ignoring philosophical and theoretical concerns is no way out. Such an approach, urging us simply to press on with the study of data without worrying about the niceties of theory, presumably inviting us to respond directly to that data, assumes that the lack of any systematic approach or procedure is somehow a miraculous guarantee of objectivity. Such a common-sense approach systematically evades any confrontation with its own premises, safeguards any methodology which is currently available and, in this manner, produces the very opposite of objective problem-free research. Empirical research presented as the obvious stuff of common sense is never called upon to guarantee its consistency, silences and contradictions and hence is entirely unsatisfactory. If philosophy has been of little use to archaeology this is because of the systematic abuses archaeologists have made of it and as a result of dabbling on the fringes of the philosophical literature. If philosophy is to be of value this will not result from calling in philosophers to tell us what to do or how to proceed. Archaeologists must themselves confront the literature, enter debates and establish positions. These will, of necessity, be philosophical positions which transcend the pragmatic concerns of the discipline but will be relevant to it. The only alternative would seem to be a blind unsystematic groping towards a study of the past, or for archaeologists to continue to find themselves subject to the comments made, quite correctly, by Morgan (1973) in relation to *Explanation in Archaeology* (Watson, LeBlanc and Redman 1971) (EA): 'In short EA seems in places reminiscent of a religious revivalist, appealing to scripture to establish his points, while surrounding his doctrine with flowery phrases and redefinitions to make that doctrine more palatable' (Morgan 1973, p. 273).

Even if it could be established - which it cannot - that philosophical issues are entirely irrelevant to archaeology, the difference between a philosophically and theoretically informed statement, and one not so arrived at, is that in the former case we do, in fact, have at least some reason to believe that we have a sound basis for what we are saying. Divorcing theory from practice is one of the fundamental features of positivism and, in itself, can only be defended or refuted on an informed philosophical basis.

We will now turn to a precise delimitation of what positivism may be taken to be, and the grounds on which it can be rejected. In constructing this critique we have found the following sources of particular use (Benton 1977; Giddens 1977; Harre and Madden 1975; Hindess 1977; Keat and Urry 1975). Two ways of proceeding are possible. The first is to consider the work of those philosophers who have actually been prepared to

label their work 'positivist' There are increasingly few of these since positivism is nov more or less a term of abuse rather than a living philosophical tradition The other, take here, is to identify specific propositions as being positivist We will be considering fo the most part the philosophical arguments themselves rather than their direct archaeo logical adaptations since am use to be made of positivism in archaeology depends o whether these positions are, themselves, sound

Naturalism

The thesis of naturalism depends on four interrelated beliefs

(1) human beings are principally physical and biological entities Concomitantly what people do and produce is, in essence, no different from the processes in the physical world in which natural scientists have their interest,

(n) all science forms a unity so that the principles relevant to the formulation and evaluation of statements are isomorphic in both the social and the natural sciences,

(in) the natural sciences provide the social sciences with a model for their procedure,

(IV) the most certain knowledge is mathematical and deterministic in conception

Archaeologists have explicitly or implicitly subscribed to all four of these doctrines In fact the entire 'project' of the new archaeology is based on them and there is no nee to go quotation mongering to establish this The eighteenth-century sociologist, Charle Fourier, was so dazzled by Newton's achievements in physics, which to him consiste in the discovery of a single universal principle, that of gravitation, that he imitate Newton by proposing that social life was governed by a principle of passionate attrac tion Most archaeologists would dismiss Fourier's proposition as ridiculous but, i essentials, it is no different and no more fantastic than to insist that to have any, validit as a serious type of inquiry archaeology must ape the natural sciences (read physics rather than consider social theories and model itself on the social sciences

The thesis of naturalism can be attacked on a number of related grounds First, a indicated above, there is no logical reason to accept thesis (in) but, equally, it cannot b rejected on purely rational grounds and, therefore, the statement is vacuous Theses (1 (n) and (iv) are more crucial and will be considered together Naturalism denies tha human beings are in any way unique and claims that their actions can be explained i precisely the same manner as physical regularities in the natural world Now it migh appear open to debate whether or not a person or a society is a natural entity After a people do possess physical bodies and are subject to the same physical forces in th world as, say, a boulder, a tree, or to use a favoured palaeoeconomic example songbirds There is undoubtedly a kernel of truth in such a position but it does not tak us very far People are not natural entities if we accept the primacy of sentience, inten tionality, linguistic and symbolic communication We clearly need to distinguis between physical bodily movement which can be accommodated in terms of a naturalis thesis and human actions which cannot be readily assimilated as They involve intentions choices, dispositions and motivations The social world is not a mirror of the natura

world but a world that is always already structured and constitutes a totality which derives its nature and form from the interpretative procedures of its members. Natural phenomena, unlike social phenomena, have no inherent meaning or cognitive structure which needs to be taken account of in explanation.

Wittgenstein asks: 'What is left over if you subtract the fact that my arm goes up from the fact that I raise my arm?' (Wittgenstein 1953, I sec. 621;. The implication of this question is that there is something far more important in the *action* of raising an arm than mere bodily movement, e.g. a reflex action. We subsume action not in terms of physical processes but in terms of the meanings to which it is directed. Meaning here is a crucial term. A reflex action is meaningless in that no human purpose or intentionality has caused, or can be related to the movement, but there are clear reasons why someone might raise his or her arm, e.g. to make a signal to someone. Meanings are necessarily and not contingently connected to human actions and their products. Social action, as opposed to movement, goes beyond itself. Intentionality is a crucial concept which distinguishes mental from physical phenomena. It involves a conception of people who can make distinctions, understand and follow rules, impose normative constraints on their conduct, judge reflexively or monitor their actions and be capable of deliberation or choice. This is a very different perspective from the usual view of behaviour espoused in much of the archaeological literature where actions are deemed to be propelled by various external stimuli, needs and role expectations (e.g., Plog 1974, pp. 49-53; Schiffer 1976; Jarman *et al.* (eds.) 1982). People possess the ability to act in and on the natural world and to systematically transform it and create their own world or social construction of reality. The superficial resemblances set up, in particular, by the palaeocconomic school (Higgs (ed.) 1972, 1975; Jarman *et al.* (eds.) 1982) and in forms of neoevolutionary theory (Dunnell 1978a, 1980; Wenke 1981) between human and animal behaviour, conceals fundamental and non-reducible differences such that there is a categorical distinction between concepts such as production and foraging, ethnicity and ecological niche, property and territorality. One set of concepts does nothing to elucidate the other since they belong to fundamentally different frames of reference. Human culture is not a part of nature but a transformation of it.

Mathematization is usually an irrelevant diversion in an attempt to understand the social world. Little has been achieved in archaeology beyond the questioning of earlier work on the basis of its alleged quantitative inferiority: 'it is as if one had to board an atomic submarine for a new discovery of America, a discovery which has to be verified simply because Columbus's "Santa Maria" was technically imperfect' (Wiatr 1969, p. 23). Cooke and Renfrew (1979) develop a model to simulate the emergence of civilization. Societies are treated as systems with human beings as their 'components'. The model is opera tionalized by using the six subsystems Renfrew (1972) defines for the Aegean and, at this stage, the human 'components' fall irrevocably out of view. A further step is to eliminate the subsystems in favour of numerical variables between 0 and 1. Now it is only possible to ascribe a numerical variable, as in all mathematical approaches, on the basis of very clear definitions which are provided. For example the projective subsystem becomes *'either* the number of abstract concepts in use in the society relating to measure *or* the number of man-hours per head per year spent in

religious observance or in facilitating them (e g. building temples (Cooke and Renfrew 1979, p. 331). The authors explicitly state that the model was only a crude and preliminary attempt but the specification of mathematical numerical variables can never be profitable. An analogous situation might be to ask someone to place a precise numerical variable (or even a value range), as to whether they would prefer their nose or right leg to be amputated In essence the thesis of naturalism collapses with the counter thesis of the irreducibility of the social. Concomitantly, if the social is irreducible the statements made about the physical and natural world must take a different form and there is no need to model social processes in terms of natural processes

Phenomenalism and the demarcation of science/non-science

The doctrine of phenomenalism and what can be taken to constitute scientific as opposed to non-scientific work remain crucial to any understanding of positivism, probably more so than the thesis of naturalism. This is so because it is possible to reject naturalism while retaining a phenomenalist thesis and separating science/non-science on the basis of various criteria. In this section we will consider issues involved in explanatory structure alluded to above but not explicitly discussed

Phenomenalism is the thesis that the only feature that can be known for certain about either the natural or the social world is what is given to human beings, as subject-observers, in the senses Anything that goes beyond sense-perception is non-observable and therefore unknowable It is the empiricist belief par excellence and remains at the heart of positivism Associated with this thesis is the belief that it is possible to perform objective tests against a solid bedrock of fact and to confirm or refute statements in this manner In an archaeological context Renfrew (1982a, p 143) has reaffirmed the crucial importance of this 'old relationship' between theory and data- 'the hypothetico-deductive approach rightly lays stress on the passage from theory to data, by means of deduced hypotheses and of hypothesis testing' Since the advent of the new archaeology testing statements against the archaeological record has remained the key-note of what is regarded as an objective and, therefore, truly worthy scientific enterprise Put very simply anything, i.e any statement which cannot be tested, must remain a meaningless statement because there is no way of evaluating it

No particular stress or importance is placed on where the statements come from, or the theories. They could be the result of a careful consideration of the data or previous work or, quite equally, arise from dreams or hallucinations. The first point to note, then, is that the process of arriving at these theories or statements to be tested is totally denigrated. This is of no importance and there is a one-sided stress on the testing process wherein the essence of truth is thought to arise Secondly, it is assumed that there actually is a hard bedrock of facts or data to test against, independent of an act of subjective definition This is the proposition that all archaeologists would be able to reach agreement, in any particular case, as to what the hard facts to be tested against actually are and this will now be examined

First, are the empirical facts non-subjectively defined? Consider the diagram below (Fig. 2.1). This is a rather famous example from gestalt psychology Is it a young or old woman? Can we perceive what it is irrespective of an act of interpretation? The answer

is no: we can either perceive it as an old or a young woman. We cannot perceive what it is supposed to be apart from a subjective interpretation of the observed evidence before us, i.e. the lines that make up the diagram. The manner in which this diagram is *described* is vitally crucial to the act of seeing what it is. The reality is not independent of a description of that reality. Hanson (1958) has argued, on the basis of such examples, that the idea of a theory-neutral observation language is untenable. What scientists sec is essentially related to their theories and beliefs about the way the world is. What we observe depends on these background assumptions so there can be no objective testing without circularity. Furthermore it is not possible in a positivist framework to judge between competing theories or statements, because different observers cannot agree on what they actually see as a result of the theory-laden nature of observational statements. Kuhn (1970) has widened this position to a discussion of paradigm change within science so that at different stages of scientific activity individuals literally see things in different ways. They may generally agree on what they are seeing but this is a consensus position which has little to do with a non-subjectively defined reality. Priestley 'saw' dephlogisticated air while Lavoisier, adopting quite different assumptions, 'saw' oxygen.

Now it could be claimed that examples such as this or the young-old woman diagram are unfair. The diagram is deliberately designed to trick or deceive. It runs counter to our general intuitions and the real world of archaeological data is simply not like that. We can agree on what we see, or what the facts are supposed to be, and make our tests against them. This is a common-sense and well-established procedure. Well, the archaeologist advocating positivist science will be hoist with his or her own petard. First, the claim as to whether theories actually are independent of observations can only

tig. 2.1 Appearance and interpretation.

be grounded on a subjective claim - a statement of belief that things really are like that There is no reason to accept this Secondly, science has nothing to do with common sense. It is precisely the opposite - science calls into question common-sense notions

Consider the concept of explanation Passmore (,19621 has demonstrated hov\, under a variety of circumstances, a piece of information can be offered as an explanation He adopts the pragmatic standpoint that ultimately what constitutes an adequate explanation depends on 'what I know and what I want to know' Above all an explanation must be intuitively satisfying depending on criteria of clarity, soundness, intelligibility and precision, criteria which it is almost impossible to realistically define. According to Passmore historians use almost as liberal criteria as, to use that old cliche, the man or woman in the street On the basis of the manner in which positivist philosophers of science have characterized their activities, natural scientists adopt an extremely specialized procedure, the deductive-nomological model of explanation' A if B (law an atemporal, aspatial statement). Empirical case B happened (antecedent condition) Therefore A occurred (explanadum event) (Hempel 1965) A variant on this model is the inductive-statistical mode in which laws arc replaced by probability statements to the effect that if A then usually B with, ideally, a specified probability of how many times B is likely to occur if A. As discussed above some new archaeologists set out to find laws in order to make their work conform to the deductive nomological model but discovered none which were neither tautologies nor of the utmost triviality. No doubt this has promoted some of the current disillusion with the value of philosophy to the discipline The failure to find laws has led to an emphasis on generalization as a substitute Renfrew (1982b, p 10) has stated that laws have always been his bete noir However, in common with most archaeologists, he has retained the wish to make generalizations and for archaeology to be a generalizing science It remains unspecified and unclear what status these generalizations are actually supposed to have and how general a statement must be before it counts as a generalization: two cases? three? fifty?. If the generalizations made arc not laws they cannot be expected to be applicable in any one particular case so why are these generalizations of use to us? Why must the business of doing science necessarily be equated with the ability, or the will lo generalize? This appears to be a procedural rule founded on the basis that generalizing, rather than considering all the particularity of the individual case, is a superior kind of activity. There seems to be no compelling reason why we should accept this.

We will now return to problems of testing theory or hypotheses against the data from a slightly different angle within positivism So far, the only specific criticism we have made in relation to deductive nomological or inductive statistical explanation is that, in practice, archaeologists have been unable to make their work conform to these rigid self-imposed models. This may be a sufficient reason for rejecting them but it is not a necessary condition

Historically, there has been a very strong link between the positivist tradition of philosophizing and, in particular, the logical positivism of the Vienna circle espoused in the work of Carnap, Feigl, Frank, Godel, Neurath and Reichenbach, and the British empiricist tradition represented by Locke and Hume (Hempel belonged to a Berlin group of philosophers while Popper was never a member of the Vienna circle and has

always distanced his views from theirs). Hempel's explication of the nature of explanatory structure is founded on the Humean 'regularity' view of causation according to which two events, C and E are related as cause and effect, if, and only if, they are members, respectively, of classes C and E of observable events. Thus each member of entity E regularly follows and is contiguous with a particular member of entity C. An observer experiencing E will, then, be led to expect the presence of C (Sayre 1976, p. 65). Causation, in such a perspective, is identified with regular succession coupled with psychological association. It is the psychological association made by the observer between C and E that constitutes, to Hume, the 'cement of the universe' (see Mackie 1974 for a detailed discussion). Put crudely it is the philosopher's job to provide the logical cement that links C and E. Hempel's work is one way of applying the cement. However, that this cement is required in the first place is purely a product of the empiricist's radical scepticism as to the manner in which knowledge claims can be made. It depends on a theory of perception in which the observer experiences the world as a series of independent and unconnected sense impressions and must connect these back together in terms of a logical cement between perceived regularities. This logical cement is a deductive syllogism, in the form presented by Hempel, but to Hume it was a process of induction. Real structural relationships are denied and exchanged for logical relations. We will return to this point in Chapter 5. It will suffice to state here that if we deny the belief that we experience the world solely in terms of disconnected atomistic particularities, but that real relations of structural necessity exist, there is no requirement for us to lay on thick layers of logical cement.

The logical positivists of the Vienna circle codified a distinction between analytic and synthetic statements. Synthetic statements refer to relationships existing between entities of which we acquire simple sense-perception, subject to the view criticized above, that we sec the world independent of theories about it. Analytic statements are purely products of logic and only tautologies as they rest on definitional clauses, e.g. a bachelor is an unmarried man because to be unmarried and to be a man is to be a bachelor. Such statements are only correct in terms of formal logic and tell us nothing about the world. Analytic *terms* relate to entities which cannot be perceived, e.g. volume, mass, force, atoms. A scientific theory almost always involves reference to such terms. To have validity, according to positivists, it must be subject to axiomatizations. Axiomatizations of a theory must include various explicit definitions for the theoretical terms of the sort Tx = Ox where T is a theoretical term and O is an observation term and the link between them is provided by correspondence rules.

Braithwaite (1968, p. 51) likened these rules to a zip which pulled together theoretical and observation statements but, true to the empiricist tradition, the theoretical language, unless it was purely tautologous and thus useless, was entirely dependent or parasitic on the observation language. The two are not on an equal footing and, furthermore, if it is conceded as even members of the Vienna circle now admit, that observation statements are not theory-free (e.g., Feigl 1970) the corollary is that theoretical and observational statements cannot at all be clearly separated. Concomitantly the latter provide, on positivist grounds, neither a conclusive affirmation or refutation of the former. Additionally W. V. O. Quine's (1961) paper 'Two dogmas of empiricism' has

demolished even the difference held to exist between analytic and synthetic statements by calling into question the very concept of meaning itself construed as some kind of mystical mental property held to exist independently of the will of a speaker to assent or dissent from sentences in natural language. Quine (1960) describes science as being analogous to a force field whose boundaries (i.e. the boundary conditions) are radically underdetermined by experience, such that there is a considerable latitude of choice as to exactly which statements should be re-evaluated in the light of any single contrary experience Thus any formal criteria for theory choice - verification, confirmation, falsification - become discredited Positivist science is no more, no less, than a form of controlled subjectivity, the controls being that there is some sort of logic or rationality involved, but exactly where this resides except on an intra-community subjective basis is rather difficult to specify. At this point it should be noted that this, kind of criterion for evaluating knowledge claims was precisely that which new archaeologists objected lo in the traditional archaeology - the more famous, and inevitably the older, the archaeologist, the more reason there was to accept his or her knowledge claims as being valid, as providing a reasonable accommodation to the 'facts'.

We will return to the theory/data relationship in another and less abstract way. We have a theory and wish to test it against the data Further, we make the assumption that the data is in some way independent of our theorization. The test is negative: is our theory falsified? Alternate el> the test is positive Do we then have any more confidence in the theory? This depends on whether we adopt a verificationist or a falsificationist strategy for hypothesis confirmation. In the early work of the Vienna circle the verification principle was upheld, i.e , statements made about the world must be empirically verifiable in relation to sense-perception data in order lo have any meaning A logical extension of this was that only scientific procedures framed in this manner could be granted meaning. Any other statements made about the world by poets, for example, or in aesthetics lacked any meaning. Concomitantly the rest of philosophy, apart from logical positivism, was also written off as meaningless activity A concession was that statements made by others might be granted emotive meaning but little more An immediate problem arose a fundamental logical inconsistency. The verificationist principle itself could not be tested nor could it be granted the status of an analytic truth Consequently, on the very grounds of logical positivism it was itself meaningless. In other words logical positivism rested on precisely the kinds of metaphysical claims about the world it shrugged off as being invalid

The phenomenalist thesis commits positivism to the idea that there is an objective world to test or verify an hypothesis against (but see the Popper variant below). Unfortunately for archaeology, or any other area of human inquiry, the content of what is supposed to be given to the senses of the subject-observer is not independent of an operation of mind and can hardly be relied upon to point to itself. The similarities or differences between two 'given[1] objects of experience must necessarily be described but it is impossible to ever give a complete description In other words even act of dealing with 'givens' in experience is dependent on a whole set of procedures not themselves given to experience and therefore not subject to meaningful discourse If all observation is to a certain extent theoretical - objects of experience are constituted by an act of knowledge

on the part of a 'knowing' subject (in our case the archaeologist) who decides what the givens are - it is illogical to maintain that theories can be *independently* tested against observations

Popper (*1959,* 1963, 1974) has persistently claimed that he is not a positivist Indeed, he has regarded his work as a critique of positivism However, he asserts the naturalist thesis of the unity of science and maintains there is a clear demarcation between scien tific and non scientific activity and that science provides the most reliable and significant knowledge to which human beings could hope to aspire As with the positivists of the Vienna circle. Popper's criterion of scientific activity is that statements should be subject to testing In all these respects Popper's work remains part of the same tradition The major points of contrast are:

(i) the substitution of a doctrine of falsification for one of verifiability Nothing for certain can be known about the world. All we can hope to do is to disprove statements by selling out to falsify them by empirical testing,

(ii) observation statements are in no way certain, they art theory laden,

(iii) he has defended metaphysics and openly acknowledges that his philosophy is based on metaphysical arguments

Surprisingly, archaeologists have not made much explicit reference to Popper's w ork (but see Salmon 1975, Tringham 1978) but it May accord more with their actual practices than strict logical positivism or the Hempelian position Popper stresses an asymmetry between verification and falsification We can never verify a law because the next test May prove it to be wrong Scientists deal more with verisimilitude than truth (Popper 1959, p 135) Science is carried out by a process of deduction, by testing universal statements flaws) against singular statements 'to give a causal explanation of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal laws, together with certain singular statements, the initial conditions'(ibid, p. 59) As in Hempel's work explanation is of the particular by the general and a symmetry is held to exist between explanation and prediction Now, on Popper s account there is no compelling reason to believe there are, in fact, laws the argument rests on the assumption that laws exist because the world can be described in terms of essential uniformities a metaphysical assertion Science to Popper is descriptive and divorced from language so that the 'logic of discovery' does not require correspondence rules linking theoretical and observational terms Truth is considered to be a non empirical concept and timeless Corroboration is different from truth and we accept some hypothesis or statement as being provisionally 'true' in that it has not been proved to be false (ibid , p 275) One single contrary observation falsifies a hypothesis in a falsificationist strategy To Popper scientific knowledge is built on the shifting sandbanks of theory impregnated observations but empirical refutations provide the basis on which knowledge is based and is supposedly a progressive advance towards more and more certainty in what we say This idea of a progressing, but nevertheless uncertain knowledge is, in fact, only possible to defend if there were a limit to the number of conjectures and refutations to be made of a theory, but as these are infinite- there is no logical end point at w hich the testing procedure can stop - what basis is there

to believe that any number of tests would lead us nearer to the truth? In the actual prac tice of testing, falsification remains unreliable A cherished theory can always be 'saved' precisely because observation is theory-laden and Popper admits that it is The slate ment that all swans are white is falsified by the discovery of a single black swan, in theory, but then, of course, we must decide whether the black swan is to count as a swan or can be defined as such Furthermore any statement about the world or a series of statements, 1 e a theory, cannot be falsified in am simple manner since a falsifying test may not impugn the whole theory but, perhaps, an unexamined auxiliary statement In this case w e more or less have to make a choice w hether to reject the statement or theory or not In the black swan case we have to decide whether the empirical example refuting our statement is a swan If we want to defend a theory we can simply reject the test as inadequate in some way

Popper's response to strategies designed to save a theory from refutation, in the manner discussed above, is to claim that this is contrary to the ideals of scientists 'we decide that if our system is threatened we will never save it by am kind of conventionalist stratagem it must be left to the investigator to constantly guard against this temptation' (ibid , p 82) So, to be a scientist, in effect, has *nothing whatsoever to do with testing* to be a scientist is to accept some type of behavioural norm as to how to act Popper asserts (1966; that Marxists accept no such norm and always save their theories (see Cornforth 1968 for a rejoinder) For Popper, then, it *a,* the behaviour of Marxists that is at issue, not the conceptual structures *they* employ What is observed and tested depends on the scientist's training (Popper 1959, pp 99-104) Concomitantly, the results of tests and w hat then counts as know ledge depends on how scientists are trained al am one time Thev can then more or less agree on basic statements about the world, but if someone doesn't agree They can be dismissed as a pseudo scientist or a fool 'when all else fails the danger of an infinite regression in the testing and retesting of basic statements by the scientific community may be averted by the elementary rule that might is right' (Hindess 1977, p 175)

To maintain, as Popper does, that observation is theory-laden but that science still progresses *by* testing against empirical data is entirely contradictory The basic statements, I e the observations made on the world, are a function of training and are theory-laden Furthermore, a choice is always involved as to whether a test has really falsified a theory We test our hypotheses not against any solid bedrock of fact, as the strict logical positivists of the Vienna circle once claimed, but in terms of basic theory-laden terms and observations we have supposedly been trained to know But if theory is involved in the very act of observation then testing cannot be a rational procedure Even if after the test we have not falsified our theory there is no reason to believe that confidence has been increased since there are an infinite number of tests which could be made If we just single out a few 'key' hypotheses to lest then, in effect, our tests only serve to reproduce the knowledge 'given' to us in our conceptual system

In positivist philosophy, then, there are no coherent grounds for the belief that we can test against an independent non subjectively defined reality The testing process provides no more certainty than if we had not tested a proposition The grounds for distinguishing scientific from non-scientific activity are far from clear and there is no

reason to believe that science provides more objective or more certain knowledge than other modes of human understanding of the social world The beliefs held by archaeologists that they can perform objective tests of their theories and choose between these theories in the framework of positivist science are undermined, likewise the belief that their activities are of a radically different nature from those engaged in by historians Some of the arguments that have been made might well be accepted (Binford and Sabloff (1983) concur that knowledge is not to be attained by testing procedures) but with the residual retention of a phenomenalist thesis which we consider further below

We have noted that a commitment to phenomenalism or the belief that we can only gam know ledge through sense-perception is a metaphysical statement so that there is no automatic reason to accept it the question is can a more plausible counter-claim be made and on what basis? The delimitation of such a claim will be left until Chapter 5 and attention here will be concentrated on why phenomenalism provides an inadequate basis for founding knowledge claims To use an Althusserian turn of phrase, positivism is the empiricism of the object, thought to be present to the senses, capable of isolation as such, and constituting the correct unit of study Knowledge consists in both a radical distinction between concepts and phenomena (set the discussion of causation above), subject and object, and vet at the same time depends on a correspondence between them There is a realm of knowledge acquired by a 'knowing' subject and this knowledge consists in propositions and concepts about the world The realm of knowledge is opposed to the object world of phenomena passing to the subject-observer as sense data Distinctions are set up between concepts and reality and May be linked up by correspondence rules, for example But in positivism there can be no real difference between an object constituted in knowledge and the phenomena of experience Thev become more or less isomorphic This might be termed a form of subjective idealism' (Hindess 1977, p 114) in that the investigator, as 'knower', on a priori grounds already formulates and constitutes that which is to be known Whatever is not physically present in space or time, and whatever is general, can only be reached through inference and therefore remains uncertain However, given that observation is dependent on theory, objects of knowledge become constituted prior to the process of knowing them Coneomitantlv, knowledge is not discovered or produced from a realm but already given to (hat reality prior to any application of method Knowledge consists of little more than the description of that which has already been theoretically constituted In other words, what positivist science attempts to produce via the application of a scientific method ologv has already been constituted prior to the operation of the methodology through an operation of the mind there can be no 'logic of scientific discovery' since it has already been decided what there is to discover (see Chapter 3)

Conclusion

The fortunes of a positivist archaeology naturally depend on the philosophical tradition on which it draws, but positivism provides no coherent epistemology, no adequate ontology of the world, no means of conceptualizing the theory/data relationship which is acceptable no convincing account of explanatory structure, asserts a crude view of the unity of science, and the spectre of science which it presents simply does not accord with

actual practice, providing no basis (itself irrevocably value laden) for claims that science is a superior mode of activity the only really meaningful activity a scholar could indulge in It is a tragedy that most archaeologists feel a commitment to carry on this completely discredited tradition of research in one form or another In fact, if positivism was actually taken to its logical extreme we would have to deny the possibility of am knowledge of the past beyond pure subjectivism Positivism would return archae ology to exactly those 'normative' traditions and that radical scepticism displayed in Hawkes's ladder of inference from which it sought to escape Hawkes 1954 to a certain extent this has already happened

The failure of archaeologists to discover laws reduces the explanations archaeologists make, in the positivist dogma, to mere 'explanation sketches' Hempel 1959; Accord ing to Hempel this is because of empirical complexity more than any thing else but it still seems to make archeology a poor thinker's science vis *a vis* natural science if archae-ologists are to accept this imperialism Binford (1977) introduced the term middle range theory' into archeology, at the same time confessing that 'in the absence of progress toward usable theory, there is no new archeology, only an antitraditional archeology at best' (Binford 1977, p 9) Binford s solution to the lack of usable theory is to build it and his subsequent work (1978, 1981, 1983a section IV, 1983b) has been devoted to doing just this, but the theories are being built from the bottom up, to arrive at empirical 'facts' which are subsequently employed to invalidate the work of others This so-called middle range theorizing is enthusiastically advocated by some(e g Raab and Goodyear 1984, Willey and Sabloff 1980, pp 249ff) and appears to be rapidly developing to the status of a new panacea for archaeological ills Middle range theory is little more than middle range empiricism, and what is supposedly 'middle' about it is far from clear According to Raab and Goodyear

> One *outcome* of middle-range theorising can be the creation of a logical struc-ture in which low-order working-hypotheses tend to confirm or negate propositions in a middle stratum and the latter in turn reflect upon the validity of vet more generalized theories On the other hand a series of testable propositions can be derived from existing theories in ways suggested by Hempel (1965), Popper (1959; and others
>
> (Raab and Goodyear 1984, p 257, our emphasis)

Given the criticisms of positivist science presented above such a statement requires no comment other than to note that the term middle range theorizing' is virtually redun dant as a means of differentiating the type of research that Raab and Goodyear propose from that which has been carried out without the use of it The concept is, rather, anew fancy icing on the old empiricist cake

In some recent discussions, the terms 'objective' and 'science' take on an almost magical significance They are so vitally important to Binford, for example, that he con-stantly repeats them in his publications as a means of legitimating the worth of his research programme The commitment to phenomenalism, stridently displayed in his work in which the subject-observer must take his or her 'premises to experience and per-mit experience to pass judgment on their accuracy' (1983a, p 421) results in the creation

of a pseudo-science or a subjective idealism as we have argued above. This is coupled with a naturalism in which the archaeological record is considered to be purely a product of a 'complex mechanical system of causation' (ibid., p. 417), a view which is reproduced in considerations of human agency. Social action is reduced to the logistics of adaptation and maximization of resources - 'labour accommodations to incongruent distributions of critical resources or conditions' (ibid., p. 344). As will be argued in Chapter 3, such a viewpoint owes much lo the value-system of the capitalist west and indirectly serves its reproduction. The adoption of positivism results in a view of the past dangerously close lo Hollis's caricature. It is high time we changed stations.