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What the New Archaeology Has Accomplished

by Richard A. Watson

The New Archaeology has focussed attention on the fact that archaeology is grounded in common sense and the principles of such basic sciences as geology and biology. Heuristic use of the hypothetico-deductive covering-law model has made all archaeologists recognize the need for explicit statements of how interpretations are derived from data. Archaeologists are not philosophers; they need not be concerned with metaphysical questions about reality. In particular, skepticism strengthens confirmation techniques— it does not jeopardize our knowledge of the past.


In 1625, Marin Mersenne [1588–1648], a champion of the New Science based on material atomism, mechanism, and mathematical methods of hypothesis, deduction, and test by empirical experimentation, published a book of over a thousand folio pages entitled The Truth of the Sciences against the Sceptics, in which he listed everything he knew. Given that he was a translator of Galileo, an editor of Descartes, a publisher of Hobbes, a colleague of Gassendi, a collaborator of Roberval, a mentor of Pascal, and a correspondent of all the major philosophers and scientists of his day, as well as being a superb mathematician and natural philosopher in his own right, this was plenty. In fact, however, like all the 17th-century founders of modern science, Mersenne was himself a mitigated sceptic in Descartes’s sense; that is, for him it was clear that, although certainty may be the goal, the limitations of the human intellect, the complexity of the universe, the multiplicity of viable but opposing explanations, and the differences in human backgrounds, needs, and interests mean that certain knowledge in natural science can never be attained. He and the other New Scientists nevertheless believed that by proposing mechanical and mathematical models and testing them with experiments it is possible to move closer and closer to true explanations. Thus, from its beginnings, modern science has employed a sceptical methodology and has been judged to be successful on the basis of its practical results.

The sceptics Mersenne was attacking were not working scientists, who knew their limitations and, in particular, were sensitive to the biases—arising from human nature, individual differences, ambiguous words, and tradition and authority—that Francis Bacon (1561–1626) outlined in his Novum Organum (1620). Rather, they were dogmatic sceptics who claimed that such biases cannot be corrected for, that it is impossible to know whether representations and interpretations of the world are true of it or even whether there is a world outside sensory impressions and imaginations at all. In particular, such dogmatic sceptics denied that the usefulness of science and the obvious advance of scientific knowledge through confirmation of hypotheses by empirical experiments count as evidence for the probable or even the possible truth of such mundane knowledge. In short, they held out for certain knowledge or nothing, and since even scientists admit that certainty is unattainable they triumphantly proclaimed that nothing at all is known or can be known. Thus, not only did they reject science but also they flew in the face of common sense.

Such nescience is the road to nihilism, the denial that anything at all exists—no facts, no values, no world. For Nietzsche (1844–1900), this means that God—that is, any ground for order or rules—is dead, and so anything goes, opening the way to irresponsible libertinage. For Kierkegaard (1813–55), in contrast, the sceptical destruction of human pretensions to scientific knowledge prepares one for fidesism, for taking an irrational leap of faith to belief in a God hidden by logic and reason. Mersenne feared both these offspring of dogmatic
scepticism. He agreed with Thomas Hobbes (1588–1679) that humans must oppose nihilism by establishing a social contract for law and order. And he agreed with René Descartes (1596–1650) that reason is superior to faith in approaching God—an approach they believed was best made through trying to understand scientifically the world God has made.

No one today could occupy Mersenne’s place, for no one now could ever hope to command the bulk of current scientific knowledge as he did. I tell this cautionary tale to suggest a parallel between early 17th- and 20th-century critics and defenders of science. The dogmatic sceptics—those who argue that scientific knowledge is not really knowledge, that we can have no scientific knowledge of human affairs, that there are irrefutable forms of knowledge that are equal to or better than scientific knowledge, that we can know nothing at all, or even that there is no world, no past, no future to know—are still with us. A few of them want us to take the mystical route to God; some suggest that we write novels, and others urge us to enter politics, to flaunt the biases they say we cannot overcome, to construct the sociopolitical reality that best serves our (or their) own interests. We are to give up the search for objective knowledge of the world in favor of subjective “reality.”

On the side of dogmatic scepticism, nothing much has changed since the 17th century. If the sceptics Pyrrho (365–275 B.C.) and Sextus Empiricus (ca. A.D. 200) were cited then and Derrida or Feyerabend now, the arguments against reason and common sense and the proposed alternatives of nihilistic libertinage and fideistic mysticism are the same. On the side of scientific knowledge, however, there has been the immense accumulation already alluded to. Scientific knowledge today is no different in kind from the scientific knowledge of the 17th century—it is still only probable, not certain—but as it has grown it has become more useful and effective. And as its quantity and quality have increased so has the probability that it approaches—even if it will never reach—knowledge of the way the world is. No scientist need claim this much, however; it is enough to point to the practical results of taking science seriously as a way of explaining and predicting the characteristics and behavior of things [including humans] in the world of our experience. And these practical results have led to the acceptance of science as a matter of common sense.

Now, my defense of science is like Mersenne’s except that I do not pretend to know all fields and I discuss generalities more than specifics. Also, because my main concern is with archaeology, I focus on the human sciences. I would begin like this:

Human beings are animals that through natural selection have developed considerably greater capacities of consciousness, language use, intentional behavior, and rational long-range and wide-scope planning than members of any other species. These biological determinants are the general grounds on which understanding, explanation, and prediction of human social [group] psychological [individual] behavior must be based (Watson and Watson 1969). This does not mean that the human or social sciences are reducible to biology, but it does mean that anyone who ignores their biological foundations is not serious about understanding human beings.

Human beings are part of the physical world, from which they draw sustenance by understanding and manipulating it. Human understanding is based on the application of linguistic categories and generalizations that are useful for survival only if they conform to the world with some high degree of accuracy. Different linguistic sets that overlap very little, for example, physics and jurisprudence, can apply to the same things in the world and are useful for different purposes. But the language of physics does not contradict or rule out the language of jurisprudence just because the one describes a given event as a dynamic distribution of masses and materials and the other describes it as murder. For example, in interpreting the presence of a projectile point in a prehistoric human vertebra as evidence of murder, an archaeologist uses basic principles both of physics and of jurisprudence.

Science in the most general sense is an attempt to learn as much as possible about the world in as many ways as possible with the sole restriction that what is claimed as knowledge be both testable and attainable by everyone. This rules out the claims of mystics, intuitionists, and fideists to transcendental knowledge based on special experiences, capacities, or faith that are demonstrably not in the domain of public experience. Knowledge that is in the public domain is objective, private or esoteric or privileged knowledge is subjective.

Public, objective knowledge of the world including human beings is not certain, but neither is it merely one interpretation out of many, each of which is no better than any other. Our current scientific picture of the world is the result of progressive improvements in methods, techniques, and instruments employed to understand and manipulate the world. Scientific conclusions are based on tests of commonsense categories and generalizations made by human beings during at least the last 50,000 years. Scientific knowledge is set in contrast to sceptical, idealist, and religious claims that even if there is a world we can never know it or that the world we do know is nothing but language or ideas or that there is a transcendent world of which this one is at best a shadow and at worst a travesty, a snare, and a delusion—claims that repudiate common sense.

As for archaeological knowledge, I argue (1972:58; see also 1976), that it is based on a multi-leveled interdisciplinary system of descriptions, laws, and explanations. Archaeologists try to provide systemic descriptions and to confirm hypotheses about past social structures on the assumption that they are represented by selected parts of extant material remains. Inferences are about and based on processes and relations among social structure, material culture, and its unaltered, altered, and selected remains. Archaeological inference depends on principles of cultural behavior, the accumulation and alteration of material, and archaeologists’ methods.
These physico-chemical, geological, biological, psychological, sociological, anthropological, and methodological principles derive from the present behavior of men and material.

I find that

archaeology is ... an eclectic, interdisciplinary science. ... Archaeologists' main goals are to describe, explain, and test generalizations about the cultural behavior that resulted in the remains of past human societies. Traditionally, archaeological data have been described and explained with reference to two bodies of lawlike generalizations. First, there are those psychological and sociological laws or generalizations concerning human behavior derived from history or observation of contemporary societies; these principles form the explicit or implicit base of description and explanation for any historian. Second, there is the general body of physico-chemical and biological laws.

And I conclude that in the interpretation of archaeological data,

... to establish one's explanation, one must predict things about the archaeological record based on already accepted facts and confirmed laws—from both archaeology and other sciences—and then find that these predictions are themselves fulfilled on examination of the record.

Archaeologists not only describe past peoples, cultures, and events in narrative fashion but also aim at confirming laws in the social sciences through the use of a general hypothetico-deductive covering-law method. They also use this method internally to test their hypotheses about the artifacts themselves. Thus, archaeology is a science both on the level of immediate excavation interpretation and on the level of overall cultural interpretation.

In sum, archaeologists base their work on the assumption—without challenge—of the basic principles, data, and current knowledge of all the other natural and social sciences, from physics to history. This does not mean that work in archaeology cannot lead to alterations of specific knowledge claims in, for example, geology, climatology, and botany (excavations can provide new data) or sociology, anthropology, and history (remains of material culture can exhibit new configurations). Some of the best archaeological work is done by interdisciplinary teams such as those assembled in the Near East by Napoleon Bonaparte [Commission des Monuments d'Egypte 1809–28], Rafael Pumpelly [1908], James Henry Breasted [1931], V. Gordon Childe [1937, 1942], and Robert J. Braidwood [1937].

If I were really to emulate Mersenne, I would write a large volume listing the things we know today about prehistoric archaeology—early villages, the origins of plant and animal domestication, metallurgy, demography, commerce, chronology—that were not known in 1945. But we all know these things.

What the New Archaeology of the 1960s [Watson 1972] has accomplished in general terms is first and foremost to focus attention on archaeology's dependence on the more basic sciences. In particular, geology and paleontology have provided a model for archaeological fieldwork and interpretation, and their geologically inspired practice is the source of the feeling of many archaeologists that they really had been doing all along what the New Archaeologists were proposing as new field methodology.

A second major accomplishment of the New Archaeology is the direction of attention to the evidential relations between artifactual data and archaeological interpretations [Binford 1962, Watson, LeBlanc, and Redman 1971]. Its hypothetico-deductive covering-law method of explanation and prediction is rightly called Hempelian because Hempel and Oppenheim [1948] presented it in a form that caught contemporary archaeologists' attention. But it had already been presented to archaeologists in that year by Walter W. Taylor, and in 1897 T. C. Chamberlin had provided geologists with a general description of it in an article proposing that several hypotheses for explaining a phenomenon be considered and tested at the same time. The hypothetico-deductive method was in some respects only a refinement of the method most geologists were already using, but the publication of Chamberlin's account of it did mark their explicit recognition of the evidential bases of their knowledge claims. This method of multiple working hypotheses became so much a part of the common-sense foundations of geological research that during the 1960s when Binford and others were introducing the hypothetico-deductive method into archaeology, many geologists were surprised that it had to be explained and astonished that some archaeologists opposed it.

The method is in fact even older than this. Pierre Gassendi [1592–1655], a friend and colleague of Mersenne who with him introduced Epicurean material atomism and mechanism into modern science, developed it in his Exercitationes paradoxicae adversus Aristoteleos [1624] and Disquisitio Metaphysica [1644]. As Detel [1978] has shown, Gassendi refines not only the method of posing and testing generalizations for use in explaining phenomena but also the idea, elaborated in our times by Popper [1959], of seeking falsifying instances to refute hypotheses. To merit consideration, an explanatory hypothesis must be empirically falsifiable, and while a hypothesis can be conclusively rejected on the basis of negative results, positive results can never make a hypothesis more than probable. Gassendi himself attempts to show that Epicurus [341–270 B.C.] also proposes the method of hypothesis and deduction, but there is no need for us to go that far back. Just before Mersenne and Gassendi Galileo [1564–1642] and just after them Newton [1642–1727] tested hypotheses, and when Newton said, "I feign no hypotheses," he meant not that he did not hypothesize but that he considered only explanations that have a basis in empirical observations and systemic mathematical models.

Now, none of these scientists had a formalized account of method. An enormous amount of the polemic
over the New Archaeology and particularly over the hypothetico-deductive covering-law method has been the result of supposing that it must be applied formally. Both advocates and opponents have overlooked the fact that it is proposed as a heuristic device, “serving to discover or stimulate investigation: to lead a person to investigate further by himself” [Webster’s New College Dictionary, 1961, s.v. “heuristic”]. The point is not to impose a strict formal method upon archaeology but to employ a hypothetico-deductive method heuristically to improve the grounds on which we make inferences from and base interpretations on archaeological data. Taylor [1967:113] expressed as well as anyone what the New Archaeology—at that point merely a gleam in his eye—was destined to accomplish. Henceforth, to legitimate archaeological work.

it behooves the archaeologist . . . to derive his observational data as objectively as possible, to differentiate between observed fact and derived inference, to make explicitly labeled interpretations of as detailed and full a nature as possible, and then to look, either in the ground or among the data at hand, for evidence by which his hypotheses may be tested.

That this is heuristic advice is a point missed by, for example, Morgan [1973] when he attacks New Archaeologists as though they were logicians making a mistake in formal logic. Even Hempel [1965] proposes probabilistic covering laws. Any generalization will do, as long as it is empirically confirmable (or falsifiable) and helps us explain and understand the data and phenomena we are investigating.

In fact, the argument that the hypothetico-deductive covering-law method is defunct because it is logically incoherent is nonsense in two ways. As Grimes [1990:517] remarks,

the basic idea underlying the H-D method . . . that a hypothesis is confirmed if part of its content, part of what it asserts about the world, is shown to be true . . . seems unassailable. That is, it is difficult to see how anyone could plausibly deny that a hypothesis is confirmed (to at least some degree) if some part of what it asserts about the world is shown to be true. This simple idea is about as obvious as anything gets in philosophy. And though the attempts noted above to formalize this idea quickly lead to absurdity, this may be an indication that there is something wrong with the tools used in formalizing it, namely, the devices of standard first-order logic, and not with the idea itself. Standard logic, after all, is a formal system designed to preserve truth rather than content, and whereas this system is quite successful in the case of truth, it is not so successful when it comes to preserving content.

In other words, the method is not necessarily wrong just because one attempted formalization of it does not work. Grimes [1990:518] goes on to say that it should not be concluded that this system is altogether inadequate for the task. Instead, the point is merely that there may be no simple and elegant way of properly expressing the intuitive idea underlying the H-D method using only the devices of standard logic. So . . . I will stick to tradition and attempt to articulate the element of truth embodied in the H-D method using the ordinary tools of standard logic and not make any apologies should the analysis become rather messy.

Now, whether or not Grimes succeeds [I think he does], the crucial point here is that the hypothetico-deductive method’s heuristic value and usefulness should be determined by its results, not by whether it can be represented neatly in standard first-order logic. It is a mistake to take logicians’ objections to proposed logical formulations of a basic and highly fruitful method in science as arguments against the viability of the method.

In any event, the focus on proposing and testing hypotheses has produced a professional milieu in which no archaeologist can write a book entitled How They Lived without showing the grounds on which the story is based. These grounds consist mostly of archaeological remains interpreted in terms of principles [lawlike generalizations] drawn from geology, biology, anthropology, sociology, psychology, and common sense. A primary source of hypotheses is ethnographic analogy [Klenodienst and Watson 1956]. I take this situation to be a major contribution of the New Archaeology. The bases for stories about exotic prehistoric religious practices, the construction of the pyramids, gods from outer space, the climate of the Near East 10,000 years ago, hydraulic civilizations, and so on, must now be explicitly presented in detail, open for testing and criticism. And the result has been stronger confirmation of some interpretations and the fading away or falsification of some very grand theories.

Since the New Archaeology revolution of the 1960s, theoretical archaeologists have become more and more interested in problems of epistemology and the philosophy of science. But, as Flannery [1982] demonstrates, the archaeologist is not a philosopher. The problems of the working archaeologist, and even the problems of theoretical archaeology, are not, as such, philosophical problems. This does not mean that archaeologists cannot also be philosophers of science and vice versa, but it does mean that there is a difference in orientation between the two endeavors—a categorial differentiation between the practical problems of science and the foundational problems of philosophy. And although the results of science may have some influence on philosophy, the results of philosophy strictly speaking need not affect science at all.

To make this point, I want to reiterate, this time concretely, that archaeology is based on all the other sciences and on common sense. When an archaeologist goes into the field to excavate, say, a mound on the hilly banks of the Fertile Crescent in Iraq or Iran, that archae-
ologist proceeds with a mind full of data, hypotheses, ideas, techniques, and expectations. There were people living back then (around 10,000 years ago according to 14C analyses), and this mound—from a preliminary sectioning—seems possibly to contain remains of a period transitional from a foraging/hunting-gathering to an agricultural phase. What will the data show about plant and animal domestication? Will the architecture indicate something about social structure? Will artifact decorations suggest something about beliefs? What will bones and teeth show about demographics and diseases? Archaeologists must utilize basic sciences to answer these questions, but, more to the point, they depend on common sense. That this mound contains remains left by prehistoric people has been known by successive generations who have inhabited this region all the way back to the prehistoric villagers themselves. People have seen the abandoned village turn into a mound. And to get a notion of what such villages were like when first abandoned and how they disintegrate, archaeologists can study mud-brick villages recently abandoned by subsistence farmers like those prehistoric people, and portions of such buildings can be replicated to experiment upon. Archaeological science [and all science] begins and continues in the investigation, testing, and explanation of such commonsense matters. We already know a lot, and we begin with what we know. We may, of course, be wrong, and this is what the mitigated scepticism of our method is all about—to keep us, for example, from telling stories about mother-goddess religions and fertility rites when the drawings and figurines in question might have been made for the same reasons and purposes that boys mold figures of women from clay and scratch pictures of female sex organs on rock faces while herding sheep and goats in the Near East today.

This is how archaeologists work—on a basis of a great deal of given, commonsense, scientific knowledge about people of the past and present, about the natural environment past and present, about how mud bricks weather, how flint fractures, how pollen collects in ponds, about the physics of 14C dating, etc. What archaeologists do not do is challenge the philosophical foundations of such knowledge. They certainly can and do challenge many individual facts and stories based on these facts, and they challenge some and defend other hypotheses and interpretations based on them. But they do not seek certain knowledge about the essence and existence of things [Descartes] or argue that the real world consists not of material objects but only of minds and immediate sensory impressions [Berkeley] or challenge the reliability of inductive inference [Hume] or claim that the real world is beyond our perceptual and conceptual capacities [Kant] or that humanity is the world spirit coming to consciousness [Hegel] or that the world is merely will and ideas [Schopenhauer] or that scientific knowledge consists only of universal generalizations attainable by only one formal method [Mill] or that meaning and verification must be derived directly from empiricist observation and all nonempirical concepts are nonsense (Schlick) or that there is no way the world is (Goodman). In short, they are not philosophers concerned with foundational problems of epistemology and ontology.

Thus, archaeologists as archaeologists do not have to have any thoughts or opinions about scientific realism (the view that theoretical entities in science really exist as they are described) or scientific empiricism (a competing view that the only entities science treats are the objects of immediate empirical experience). As for scientific realism, there are no formal theoretical entities in archaeology, for there is no axiomatized theory of archaeology (and surely never will be one) as there is, for example, in quantum physics. As for scientific empiricism, anyone who is primarily interested in metaphysical grounds for establishing the existence of material objects or the past or for going from sensory data to knowledge about material objects or the past must surely feel impatient (and certainly is out of place) on an archaeological dig (when the same work can be done from an armchair in front of the fireplace at home). In effect, archaeologists are practicing scientists, and science can and does proceed whether or not it is grounded philosophically in a metaphysical system. The ontological status of scientific entities is not a question that concerns the practice or success of science, and the epistemological grounds of science (for example, inductive inference) need not be formally established for science to advance by means of them.

Problems of scientific realism and empiricism are, of course, very important—in philosophy. They are not a part of archaeology, for they concern the foundations on which archaeology must be based. These foundations are the assumptions with which scientists agree to begin—for example, that there is a world, that we can understand it, that there are people, and so on. I emphatically do not wish to deny the philosophical possibility that all life is but a dream or that a demon may be deceiving me into thinking there is a real world out there when there is nothing but the demon and me [Descartes]—but whether there is a real world, whether scientific realism or scientific empiricism is correct (or whether they conflict or can be reconciled), is irrelevant to archaeology as practiced and as a science. Archaeologists [scientists] do not have to answer these questions, nor do they have to pay any attention to philosophers’ answers to them. This is the chasm between science and philosophy.

There is, however, a relation (not a bridge) between philosophy and science—the philosophy of science. On this view of philosophy strictly speaking as metaphysics concerned with ultimate questions of being, the philosophy of science is rather a cognitive, analytical part of science—the examination and analysis of the logic and language of science. It is not philosophy in the sense of metaphysics. It is only conceptual analysis, a concern with the logical relations among concepts and between evidence and conclusions—with such questions, matters of ordinary epistemology, as “Is this concept internally consistent?” and “Do these conclusions follow
from these data?” and “Are these inferences supported by these premises (generalizations)?” and not with such metaphysical questions as “Do we have certain truth?” and “What is ontologically real?” or “Did the past exist?” Philosophy of science is scientific meta-archaeology, but it is not the sceptical meta-archaeology of Shanks and Tilley (1987a, b), who challenge philosophically (metaphysically) the commonsense assumptions on which archaeology is based.

The philosophy of science so conceived is not metaphysics but merely a handmaid of science. This means that when philosophers of science point out, for example, that quantum theory is contradictory, physicists are free to ignore them—and they do! Similarly, when radical sceptics point out that we can doubt that there was a past and that we can learn about it, archaeologists can (and should) simply continue posing and testing hypotheses empirically in search of probable truths and let the metaphysicians worry about Certainty, the nature of Truth, and the existence or nonexistence of Ultimate Being. To take such sceptical arguments seriously is to credit the assumption that science is meant to solve ultimate metaphysical problems, an assumption no scientist need agree to.

To the extent that philosophers of science clarify what scientists do, their work is interesting and even heuristically—but only heuristically, not as an iron-clad set of procedural rules—useful to scientists. When they start asking what is real—theoretical entities? material objects? the past?—then their concerns transcend those of scientists. Scientists assume that there is a world that we can learn something about by posing hypotheses based on empirical observations and testing them with empirical observations and experiments. Philosophers question the grounds for such assumptions. As Leibniz (1688:39–40) said, it is worthwhile for a few philosophers to pursue such basic metaphysical questions, but scientists should get on with their practical work, which is discovering the general laws in accordance with which natural phenomena occur.

Am I saying that there is no philosophical criticism or pronouncement that can undermine science? Yes, exactly. Philosophy just does not matter to science. Scientists need not have any ontological position about what is real or any way to determine the absolute truth. They observe that ordinary entities exist and behave in various uniform ways but are willing to change their minds on the basis of empirical evidence. Thus they have given up belief in witches and phlogiston, doubt the existence of but are willing to conduct experiments concerning telepathy and telekinesis, and believe that there are atoms and people.

The view that science is metaphysics and that scientific entities are ontologically real is a philosophical position called scientism that no scientist need hold [although, of course, some of them do]. An archaeologist as an archaeologist need take no philosophical stand about ontological reality. The working assumption [hypothesis] that the world and the things in it exist is an essential part of the science and practice of archaeology. Observational results may prove this assumption to be wrong about some things [nobody or nothing does what witches and phlogiston are said to do]. But neither archaeology nor science as a whole need be abandoned or rejected because of sceptical arguments that they are metaphysically groundless or misguided or a delusion. Philosophical doubt that there is a world with things in it to be known—criticism of science as such, transcendental meta-archaeology or meta-science—is metaphysics and does not engage with practical pursuits or with practicing science.

If you doubt that people in the past lived in a village of which a given mound is the remains, then what on earth are you doing excavating it? Shanks and Tilley (1987a:31–32) give one answer: You are going through an elaborate charade, using the honorific and evocative title of “The Archaeologist” to write propaganda that will advance your interests and ambitions, to get yourself promoted to full professor with a big salary raise and lots of professional clout. Actually, of course, you can get these things even if you don’t know that what you are doing is a farce—even if in all false consciousness you sincerely believe that you are finding out how they lived, those prehistoric peoples whose existence is an essential, basic, commonsense, practical assumption of your work.

There may be some cynics who use archaeology solely for propaganda and personal gain. But rather than posing the non sequitur argument that such propagandistic use of archaeology shows that archaeology really is a deceit, and then arguing [in a circle] that because archaeology is a deceit we should use it propagandistically—as do Shanks and Tilley (1987a, b)—I argue that propagandizing is a misuse of archaeology that should be exposed and opposed.

Because the extreme scepticism of so-called post-processualist archaeology bears directly not so much on archaeology as on science itself, we can follow the revolutionary lead of the 17th-century New Scientists: unable to beat the sceptics, we can join them. The New Scientists incorporated mitigated [nonphilosophical, nondogmatic] scepticism within science as the method of hypotheses. We don’t need certainty; probabilities are quite satisfactory; no scientific “law” or “fact” or “truth” is absolute or complete or certain; any generalization reached in scientific inquiry is open to revision, rejection, and replacement.

Philosophical scepticism cannot be refuted. We don’t really know whether there is a past or what the facts or laws of nature are. But does this abstract logical result mean that we should give up science? Not at all. Extreme scepticism never has and never will undermine practice. Pyrrho, the greatest sceptic of them all, lived to be 90, and he didn’t reach ripe old age by ignoring commonsense realities. Hume (1739:183) explained his own position as follows:

Shou’d it here be ask’d me . . . whether I be really one of those sceptics, who hold that all is uncertain, and that our judgment is not in any thing possesst of any
measures of truth and falseness, I should reply, that this question is entirely superfluous, and that neither I, nor any other person was ever sincerely and constantly of that opinion. Nature, by an absolute and uncontrollable necessity has determined us to judge as well as to breathe and feel; nor can we any more forbear viewing certain objects in a stronger and fuller light, upon account of their customary connexion with a present impression, than we can hinder ourselves from thinking as long as we are awake, or seeing the surrounding bodies, when we turn our eyes toward them in broad sunshine. Whoever has taken the pains to refute the cavils of this total scepticism, has really disputed without an antagonist, and endeavours by arguments to establish a faculty, which nature has antecedently implanted in the mind, and render’d unavoidable.

Skepticism may convince the reason and is a viable philosophical position, but it cannot override common sense and has always been impossible to maintain in practice. Practically speaking, it doesn’t matter that our knowledge is not certain; we have always acted and always will act with less than certain knowledge, because we have to—because that’s the way the world is. Science is a practical affair the results of which confirm the commonsense assumptions on which it is based.

How do we justify science itself? We don’t. But how do we know its principles are correct? We look at the results. How do we know they apply? We test them and find out. But is science true? All we know is that when its principles are applied, they work. Why does this “working” work as a criterion for legitimating science? Because we accept it. But that’s because of our anthropocentric, subjective interests and needs. So who else cares? But doesn’t science need metaphysical justification? No, because scientists make no metaphysical claims about certain truth and ontological reality. Does this mean that archaeology and science are above criticism? Of course not: areas of research [e.g., vivisection of human subjects], purposes [e.g., a better nerve gas], and uses [e.g., psychology to convince teenagers to take up smoking] of science [e.g., as technocracy] and archaeology [e.g., as propaganda for white supremacy] can be challenged on all sorts of grounds—factual, practical, prudential, moral, commonsensical, political, economic—but not on the metaphysical grounds that we do not or cannot know anything for certain.

In “The ‘New Archaeology’ of the 1960s,” I described how archaeologists had become scientifically self-conscious. I remarked that “no archaelogist henceforth can comfortably present material for which he does not explicitly state his assumptions and show how his conclusions follow from his data,” and I went on to comment that Lewis Binford “is the man who put it all together and made archaelogical theory exciting in the 1960s” (1972:212). I will count this paper a success if someone remarks later on that I was the one who made archaelogical theory boring in the 1990s by showing why it is irrelevant to practicing archaelogy.

Comments

WILLIAM Y. ADAMS
Department of Anthropology, University of Kentucky, Lexington, Ky. 40506, U.S.A. 14 XII 90

I have no quarrel with anything Watson has written, except that it isn’t what his title promises. Except in a couple of brief early paragraphs, his paper is a discussion of what New Archaeology is and isn’t [or was and wasn’t?], not of what it has accomplished. These, I submit, are two quite different issues.

Somehow lost sight of in nearly all discussions of New Archaeology is the fact that archaeology—whether New or Old—is first and last a field science. Ultimately it must stand or fall on what it brings out of the ground, not on its promises or its intellectual posture. And when all is said and done, there has been much more said than done. Perhaps the absence of any discussion of substantial results in Watson’s paper is commentary enough on what the New Archaeology has really accomplished.

BARBARA E. BARICH
Dipartimento di Scienze Storiche, Archeologiche e Antropologiche dell’Antichità, Università di Roma “La Sapienza,” Via Palestro 63, 00185 Rome, Italy. 16 XII 90

In the wake of “Archaeology into the 1990s” (Shanks and Tilley 1989), Watson’s work presents in a direct and detailed way the problem of the status and role of archaeology in modern society. In the 1960s, when the problem was posed for the first time, the alternatives available were descriptive archaeology or scientific archaeology. Now we can venture even farther: can archaeology be envisaged not only as a science but also as philosophy and, consequently, metaphysics?

In the face of today’s questions, one definite fact with undeniable and irreversible consequences is the role played by the New Archaeology in forming a scientific definition of the discipline. We should ask ourselves, however, if the problem posed by Watson fully grasps and completely analyzes all the questions that are being addressed to archaeology at present. In fact there are numerous cues in the context of the current critiques (the emphasis on the subjective and cognitive aspects of research, abandonment of the strict application of the hypothetico-deductive method, questions about the social role of archaeology, and the very definition of archaeological science [Shanks and Tilley 1989:45–46]) that in my opinion cannot be dismissed as extraneous. Watson’s paper would seem to reduce the complexity of the questions we face to some assertions that call for further analysis.

According to Watson, it is not scientists’ task to take ontological positions regarding what is “real” or how truth is established. Archaeologists are practicing scientists, and science must, nevertheless, advance, whether
or not it is philosophically based. Watson indicates that the assumption in principle that the world and the things within it exist ordinarily is fundamental for science and therefore for archaeology; philosophical doubts are considered metaphysics, not directly connected with the aims and the practice of science. Reading his paper, one is led to think of the assertions that led to the development of a specifically archaeological theory centered upon the dynamics of the formation and transformation of the archaeological record [Binford 1978], an approach that is restrained and empirical with respect to problems of human behaviour. In my opinion, human becoming must instead be evaluated in a single dimension, in the all-encompassing sense of anthropology [Barich 1978–82], and therefore the archaeologist-archaeologist cannot avoid questions about the role, tasks, aims, and conditioning of the scientific undertaking in its entirety.

JAMES A. BELL
Philosophy Department, CPR-259, University of South Florida, Tampa, Fla. 33620, U.S.A. 81 91

Mersenne’s epistemological ideas are certainly relevant to the contemporary controversy over method in archaeology; he did view science as a route to knowledge and material prosperity. As Watson points out, he was also convinced that empirical science provides the standard of rationality with which to battle relativism. Relativistic tendencies in the work of some widely known archaeologists are indeed disturbing.

Hodder [1986:30], for example, states that “it will be necessary . . . in the quest for an adequate archaeology of mind, to ditch decisively the natural science, covering law approach.” Typical of those who attack rational standards, he believes that sociological factors and especially “power” are the keys to understanding why theories are adopted. Not surprisingly, the Frankfurt school, with its critical theory, is one source of inspiration to Hodder, and another is Foucault’s interpretation of power in human institutions. Hodder seems to feel justified in playing the same intellectual power game he thinks he has exposed. He recommends imposing his own political and social perspectives—feminist, indigenous, and working-class—on the generation of theory and seems indisturbed that such dogmatism could just as well be used to impose a plethora of political and social perspectives contrary to his own—such as that women are inherently inferior, that dominant civilizations are intrinsically superior, and that people flounder in the working class because they lack intelligence or imagination and/or are lazy [Bell 1987]. It should be added that many archaeologists are using feminist and other perspectives to enhance understanding of prehistory, but with empirical methods [see Gero and Conkey 1991 and esp. Wylie 1991].

Watson points to relativistic themes in the work of Shanks and Tilley, and I would like to add more. Their “politics of truth” amounts to the assumption that truth is a product of politics: “We must be concerned to investigate what kinds of power and determinate social conditions make the truth of a text or a museum’s representation of the past appear plausible” [Shanks and Tilley 1987b:198], critical archaeologists must “take up an oppositional role to contemporary society” and embrace “a notion of archaeological discourse as being part of a war of position” [p. 204]. Like Hodder, they do not want to acknowledge any significant criteria for adopting theories other than political and power relations, and therefore they do not hesitate to impose their own political and social views on archaeological theory [Peebles 1989].

As Watson makes clear, the New Archaeology has provided an epistemological perspective from which to fend off this relativism. Curiously, though, inductive interpretations of the New Archaeology have buttressed a relativistic backlash.

Binford proposed explanations of processes as a scientific alternative to narrative accounts of prehistory or accounts based upon ethnographic analogies. The trouble with this has been that his espousal of a rather narrow positivistic model for legitimating theories as scientific and his insistence on enforcing it has all the elements of the discredited “law-and-order” approach that characterizes scientism. Narrow positivism—also called scientific empiricism—is unworkable [Wylie 1982, Salmon 1982, and Bell 1987], and in fact Binford’s productive work does not reflect it [Salmon 1982]. Nevertheless, by conflating fruitful scientific approaches with positivism, relativists can attack positivism and dismiss fruitful scientific approaches along with it [Bell 1987].

Positivism is an extreme type of induction, other versions of which include covering-law and statistical-relevance models. A primary goal of all inductive approaches is to identify which theories are acceptable—for example, which theories are probable or which of a number of theories is more likely. One problem is that it is difficult if not impossible to make such determinations in practice. Another is that theories can be stepping stones to better theories; a “less probable” theory, for example, may turn out to contribute more than one that seems more probable. Finally, identification of acceptable theories is done in retrospect, in a “reconstruction” of a problem situation and the theories that bear upon it, but the most exciting aspects of theory development are normally over before such a reconstruction can be done [Bell 1984, 1991]. For these reasons I question whether induction, including the covering-law and statistical-relevance versions mentioned by Watson, is the most helpful methodological framework for archaeologists. Some of the most widely read advocates of inductive approaches for archaeology [Watson, LeBlanc, and Redman 1971, 1984; Salmon 1983, Kelley and Hanen 1988] have broadened the meaning of “induction” with such recommendations as generalizing beyond a data-base and incorporating risky tests. Incorporating such methods into the inductive framework makes the latter more workable, but it would seem better yet to replace the inductive vision with one more relevant to archaeological practice.
The role of the hypothetico-deductive model is indeed often misrepresented. Deducing the consequences of a theory functions heuristically in a number of ways: it reveals what a theory explains, helps clarify what it predicts or retrodicts, and helps identify its test points. As Watson explains, the heuristic functions of the hypothetico-deductive model have made it essential for rational procedures from time immemorial. The importance of deducing the implications of hypotheses is also borne out by the fact that it is an essential ingredient of every well-known view of science—inductive, paradigmatic, refutational, and even anarchic. Watson’s emphasis on the heuristic role of the hypothetico-deductive model should help dispel confusion about its function or importance.

I agree with Watson that ultimate ontological questions are philosophical and are not directly important to the practice of science, but I take exception to one component of his reasoning. He states that the philosophy of science is “the examination and analysis of the logic and language of science.” Indeed, the view of philosophy as conceptual analysis or language analysis—the “analytic tradition”—is prevalent in academic circles in the English-speaking world. For a number of historical reasons it is often (but not always) associated with inductive epistemology. I recommend a broader view of the role of philosophy. In the refutationist tradition, for example, philosophical problems are rooted in other fields, and proposed answers may thus have an impact on practice in those fields. Most of Watson’s ideas in this paper are relevant to theory development in archaeology, and therefore he seems to be operating with a definition of philosophy considerably broader than the analytic one he presents.

F. G. FEDELE
Sezione di Antropologia, Università di Napoli, via Mezzocannone 8, 80134 Napoli, Italy. 8 1 9 1

Watson’s paper is a model of directness and clarity in an archaeological milieu often dominated by “postprocessual” cacophony. In this it simply reaffirms ideas and qualities already known from Watson’s earlier writings (1972, 1976). But more perhaps than clarifying, this elegant lesson in philosophy appears to be liberating. While offering a retrospective assessment of the American New Archaeology movement, it in fact mainly addresses the current and future shape of archaeological thinking. I will arrange my remarks in terms of these two concomitant but distinct goals.

A sustained tendency towards explicit methodological questioning has characterized archaeology during the middle part of this century. The most enduring result, in addition to technical advances, has been an irreversible self-awareness. A new perception has emerged of both the intellectual and professional aspects of archaeological work. Like geography at about the same time or geology a century earlier, archaeology can be said to have come of age as a procedure-conscious “scientific” affair, losing its innocence (Clarke 1973) as well as some of its flagrant national idiosyncrasies.

Excitement over archaeological theory culminated in the 1960s, spearheaded by vigorous proponents in the United States and Britain, but distance in time is already sufficient to show how deeply rooted the “revolving adolescence” of the 1960s (MacNeish 1978:52) was in earlier works and trends (Trigger 1989:295, 303, 426). Watson himself is attentive to the antecedents of ideas that were considered new by the most radical or inexperienced New Archaeologists of the 1960s (Watson 1972). From this perspective his opinion about what the New Archaeology has accomplished seems temperate and consistent. If by “accomplishment” he means “lasting contribution to the field,” his choice represents a sensible attempt at reducing the legacy of the New Archaeology to its essentials—a welcome simplification in the face of the movement’s equally verbose supporters and detractors. He credits the movement with [1] implying essential methodological links between archaeology and “more basic sciences” under the guiding hand of common sense (a keyword in this paper) and [2] explicitly calling attention to the relations between archaeological interpretations and material evidence.

I am not sure that the first claim identifies a contribution of the New Archaeology, although I fervently hope that Watson’s judgment is correct. This was a process with a longer history, and its maturation during the New Archaeology decade was largely coincidental. Very few New Archaeologists have paid much attention or followed in Watson’s (1976; Watson and Watson 1969) philosophical footsteps on this point. What we still need is a vision of a true coevolution of archaeology and the natural and social sciences in terms of fully explored conceptual and practical implications, and this is a largely unfinished endeavor. Renewed debate on such a basic subject as stratigraphic theory (Gasche and Tunca 1983, Fedele 1984; cf. also Schoch 1989), for instance, points to vacillation regarding the crucial links between archaeology and geology.

The second claim should not be contentious. A consciousness of the problem of “the evidential bases of knowledge claims,” as Watson puts it, and the resulting critique of the archaeological data-base are belated acquisitions by comparison with similar sciences [e.g., geology (Watson 1969)] but clearly mark an outstanding recent advance. Only the most obdurate archaeologist in the traditional mold can now disregard that archaeological “facts” do not speak for themselves (see Binford 1983).

Perhaps modesty prevents Watson from ranking among the true historical novelists of the New Archaeology the involvement of professional philosophers. This I view as a striking departure from the virtual non-existence or naïveté [e.g., Childe 1956] of previous intercourse between archaeology and philosophy. Professional thinkers are better than amateurs, and attracting some, for the first time, contributed greatly to the casting of archaeology in a suitable epistemological context, while imparting to the New Archaeology its peculiar
flavor (cf. Hole and Heizer 1973:33). Not being bound to be sympathetic with trendy archaeologies, philosophers will remain important in monitoring the health of archaeological theory (e.g., Kelley and Hanen 1988).

To this effect Watson reiterates his allegiance to hypothetico-deductive methods, of which he offers an elaborate historico-philosophical validation, but he sets out to clarify their application and value in a pragmatic and reassuring way. Archaeologists are told not to be afraid of the Hempelian covering-law method, in particular, and to treat it only as a heuristic device. The method is not supposed to be applied formally in archaeology, says Watson, in contrast with radicals and new "Pyrrhonian" sceptics, for "the archaeologist is not a philosopher," and any means of generalization will do.

For archaeology to be scientific, he advocates no more than a mitigated, nondogmatic, common-sense scepticism in inventing and testing hypotheses, bringing the discipline to accept "less than certain knowledge." And he counsels archaeologists to get on with business and to turn to philosophers (of science) only for some informal shoulder-patting. That's liberating.

This essay on how to restrict the invasion of "theory" in archaeology is deserving of careful consideration in the search for ways to deal scientifically with data and interpretations of a historical kind. It is also a sign of the magnitude of pendulum swings in current archaeology.

ZBIGNIEW KOBYLIŃSKI
Institute for History of Material Culture, Polish Academy of Sciences, Świerczewskiego 105, Warsaw, Poland. 17 XII 90

Watson eruditely explores several problems crucial for archaeological theory. I will comment, from the point of view of Polish archaeology and philosophy, on just two: whether the covering-law model of explanation is really a major accomplishment of the New Archaeology and whether archaeology is actually based on common sense.

Contrary to Watson's opinion, the Hempelian deduc-tomonomological model of explanation is different from Popper's model of the hypothetico-deductive confirmation of hypotheses. These procedures correspond to different epistemological aims, and while I agree with Watson that the hypothetico-deductive method can and should be treated as a heuristic device, this is not the case with the covering-law model. I would argue that the most important part of the programme of the New Archaeology for a European student was the anthropological perspective proposed by Binford, while focussing on the logical correctness of explanation procedures was its most serious error. This fallacy resulted in strong criticism (e.g., the "totally abortive attempt to build archaeological theory" [Tabaczyński 1985]) arising precisely from the identification of this orientation with the covering-law model of explanation.

Although the Hempelian model does not adequately describe the explanation of human behaviour (because it is necessary to consider the individual's motivation and assume the rationality of his actions on the basis of his knowledge [this kind of explanation is called humanistic interpretation; see Knita 1971]), I agree that the explanation of global historical facts conforms to this model, although the laws to which it refers are selective-adaptational nomological formulas rather than unambiguous covering laws. The crucial problem, underestimated by the New Archaeology, is, however, that discussion of the logical correctness of explanation procedures should be preceded by discussion of the subject matter of archaeology, particularly of the characteristics of social processes [Pałubińska and Tabaczyński 1986]. The New Archaeology, which in this respect can reasonably be called positivistic (Urbańczyk 1981), has also overlooked the "theory of archaeological sources," in particular, the ontological status of evidence, the complexity of the objective and subjective processes creating the sources, and the role of the archaeologist.

The fundamental methodological problem is not the "context of explanation" (as the New Archaeology has assumed) but the "context of discovery"—how the archaeologist formulates hypotheses, what his inspiration is in modelling the phenomenon under study. Here I cannot agree with Watson that the archaeologist need not ask metaphysical questions. We must not use our common sense—the image of the world shaped by contemporary European culture—to reconstruct the conditions of past phenomena, because different axiologic and ontologic prospects prevailed. The construction of theory and models should be preceded by a phase of ethnarchaeological, sociological, or even experimental study of the phenomena in question. I consider this, as does Watson, the most important lesson of the New Archaeology.

The assumption of free will as the main driving force of human action sometimes (e.g., Dommasnes 1987) results in the rejection of the possibility of regularities in social phenomena. Therefore, what is specific to archaeology (and other humanistic disciplines) is not "explaining" but "understanding," achieved through hermeneutical "insight." The notion of human agency (Topolski 1973) is a fundamental paradigm for Polish theories in the social and historical sciences; man is an active subject of history, and objective reality does not determine his activities but indirectly selects them. The notion of cultural indeterminism results, however, from focussing on the surface layer of events.

According to historical epistemology, the change of archaeology's theoretical consciousness from "mythology" to "science" is a consequence of the refutation of its assumption that the essence of the phenomenon under study is susceptible to observation (Nowak 1975). Post-positivist science breaks with the common-sense image of the world and tries to penetrate the deep structure conditioning human behaviour. Every particular event is at once unique and typical—a nonrepeatable manifestation of certain detectable regularities.

The antiscientific programme in archaeology there-
fore arises to a large extent from an inaccurate diagnosis of the New Archaeology as simply the search for covering laws operating on the superficial level. One should appreciate the achievements of this “postprocessual” or “radical” current in archaeology: the drawing of attention to the role of the individual in socio-cultural reality, the reflection on the role of symbolic communication in human cultures, the demonstration of the effect of the archaeologist’s world view on his image of the past, and the consideration of his moral and political responsibility. However, and here I wholeheartedly agree with Watson, archaeology is threatened by the current of idio-

R O B E R T  D. L E O N A R D

Department of Anthropology, University of New Mexico, Albuquerque, N.M. 87131, U.S.A. 15191

Watson has done an excellent job of placing the arguments of modern sceptics in archaeology within their historical context. He is also to be commended for his discussion of the proper place for the philosophy of science, concluding that such activities belong on the sidelines of archaeological activity. Yet, since his forum is CURRENT ANTHROPOLOGY and not an obscure [to archaeologists] philosophical journal, he clearly has decided to offer a set of recommendations that archaeologists might heed as they pursue their endeavors, thereby relinquishing his comfortable seat in the grandstand in order to participate. I am grateful that he has, for, agree or disagree, this is an interesting and provocative article.

His first recommendation is that we ignore the intrac-
table debates of philosophers regarding truth and the nature of reality and simply go about the business of doing science. He is evenhanded here, clearly recognizing that scientists are social creatures who pursue their research in a contemporary milieu that influences not only the problems they address but the outcomes of their research. Such a recognition does not mean that science should not be practiced because scientists bring biases to their work or that all science is “good” and should be practiced simply because it is science. Few who have ever been ill would deny the value of the science of medicine, but even the most resolute capitalist would likely think twice about Levins and Lewontin’s [1985:209–24] argument that contemporary farmers could feed the hungry of the world if the goal of agricultural science were to do so rather than to reap profits for agribusiness. With all this in mind, his is a most practical recommendation.

The second recommendation is more problematic. Watson suggests that not only do we not have to worry about “truth” and “reality” but also we do not have to worry about the development of theory specific to archaeology. We can instead rely on the hypothetico-
deductive method applied in conjunction with theory developed in other “more basic” sciences and our own common sense. I do not feel that this is a productive stance. First, while theory developed in the “more ba-

sic” sciences has certainly helped us in the ways Watson describes [e.g., chronology building], no amount of borrowing from these sciences is going to be sufficient, even supplemented by common sense, to allow us to address the questions we ask of the archaeological record and the behavior that produced it. The effective study of the human past demands the creation of analytic units and the specification of relationships among units—both the domain of theory. Physicists, chemists, and the rest have their own research goals and no expert knowledge of the problems of interest in our field, and therefore they are extremely unlikely to build theory appropriate to archaeological problems. Theory building in archaeology cannot be left to workers in “more basic” sciences. Moreover, while it would be inane to deny the use of common sense in scientific applications, the differences between common sense and scientific theory are clear [Dunnell 1982]. Indeed, it is because scientific theory allows us to escape the limitations of common sense that what were once fantastic claims [e.g., that the earth orbits the sun and not vice versa and the theory of relativity] are now accepted knowledge.

Watson’s recommendation encourages complacency with respect to scientific theory building in archaeology. It is unfortunate, but most archaeologists seem to agree with him; few work assiduously at theory building. The product is theoretical disarray, with many commonsen-
sical approaches seeking priority on the pages of our journals. A significant and troubling consequence of the primacy of common sense is the absence of secure and consistent standards for the evaluation of ideas or proposed explanations in archaeology. Indeed, common sense cannot offer such standards.

Taking Watson’s perspective on the role of common sense to the extreme, one might even consider his arguments postprocessualist. Common sense is societally or culturally [if not individually] determined, and therefore dependence on it could produce the society- or culture-specific [if not individual-specific] archaeologies, many of them non-Western, that are the goal of many postprocessualists. But would all of these commonsensical perspectives qualify as science? And how would Watson suggest that we evaluate this multitude of perspectives in the absence of an arbiter for common sense? The scientific method has an evaluative procedure in place [see, e.g., Lewontin 1974]. Since common sense cannot be evaluated in similar terms, Watson’s recommendation not only ensures the continuation of the current state of theoretical disarray but also provides a post-hoc rationalization for it.

Watson appears to conclude that the accomplishments of the New Archaeology have been significant, although he discusses very few beyond the important introduction of the hypothetico-deductive method. Inter-

estingly, there is little congruence between the title of his article and its content. If an interested reader is indeed concerned with the topic of the title, I recommend comparison of the several Fieldiana: Anthropology volumes published in the late ’50s and early ’60s on work conducted in the American Southwest by research-
ers at Chicago’s Field Museum of Natural History with work reported after the arrival of Lewis Binford at the University of Chicago. The contrast is a good measure of how the New Archaeology has changed the face of our discipline.

Ironically, it is the failures of the New Archaeology that determine the structure of both Watson’s arguments here and the work of many postprocessualists. Evaluated in terms of its original goals, the New Archaeology has fallen short on major fronts. After nearly 30 years, few if any “laws” of human behavior have been identified. Platitudes about “culture process” abound, but to many the phrase is little more than an emblem. In this theoretical vacuum, Watson has no recourse but to recommend theory drawn from other disciplines coupled with common sense as the path to science. At the same time, many postprocessual approaches would substitute humanism for science as a goal for archaeology—a not unlikely response to the failure of the discipline to achieve science on its own terms. And, if we believe, as Watson does, that theory developed in archaeology to suit archaeological needs is irrelevant, it likely never will do this. Fortunately, as Watson points out, we don’t have to believe everything philosophers of science tell us.

MATS P. MALMER
Department of Archaeology, Stockholm University, S-10691 Stockholm, Sweden. 15 1 91

The title of Watson’s paper leads one to expect a survey of the concrete knowledge about prehistoric times achieved since the 1960s by the advocates of the New Archaeology. Of course we get nothing of the kind. The majority of the world’s archaeologists have not taken part in the debate started by the New Archaeologists, but they have produced the majority of the new concrete results. Moreover, the bulk of our concrete archaeological knowledge was assembled before the birth of the New Archaeology, and a very small part of this knowledge has so far been criticized or revised by the New Archaeology’s champions.

This is not of course to say that the New Archaeology has accomplished little. On the contrary, it has probably achieved more than any other 20th-century school of archaeology. Therefore Watson’s investigating of its accomplishments is important, and he does it with pleasing clarity.

According to Watson it is first and foremost thanks to the New Archaeology that attention has been focused on archaeology’s dependence on “more basic sciences.” But Trigger [1989:80, 244] stresses the ecological approach of mid-19th-century Swiss and Scandinavian archaeology and its eventual influence on Western archaeology. C. J. Thomsen, the real founder of scientific archaeology, called for osteological examination of human and animal bones and for chemical analysis of the contents of prehistoric pots. He urged that the mutual relations of artefacts in a grave be carefully noted, since they are “often more important than the objects themselves” [1836:88]. And Thomsen’s reasoning bears the stamp of the logic of the natural sciences throughout. This is little known in the Western archaeological world; all that Hole and Heizer [1965:55] have to say about Thomsen is that he “devised a method for sorting” artefacts (“This was the birth of the Three-Age System that has plagued more constructive archaeological thought to the present day”). The truth is that Thomsen did not create the Three-Age System [Malmer 1989] but more than anyone else was the creator of a new archaeology.

Western archaeologists often display an astonishing ignorance of the archaeological literature outside the English-speaking world, and the reason is obviously insufficient knowledge of languages. In his Archaeological Perspectives Binford [1972:100] cites 108 works, 1 in French and 107 in English. This is not at all unique, it is normal in the British and American archaeological literature. Watson’s paper is a welcome exception, for he cites no fewer than 6 works in languages other than English. Every Scandinavian archaeologist spends at least one or two years of his life studying foreign languages, and as a result the citations in the average modern Scandinavian archaeological paper are about 45% English, 5% French, 15% German, and 35% Scandinavian. If English-speaking archaeologists would learn to read foreign languages we would indeed have an interesting New Archaeology.

Watson rightly praises Binford’s New Archaeology for its explicit use of the hypothetico-deductive covering-law method of explanation and prediction. This is indeed one of the greatest advances in modern archaeology. But, as Watson stresses, this indispensable method of reasoning was recommended earlier by Taylor [1948], and it was implicit in Thomsen’s work. The world’s first ethnoarchaeologist, Thomsen used the stone implements of modern primitive peoples to explain the use of prehistoric artefacts [1832:67]. Thucydides [ca. 460–399 B.C.] too was an excellent archaeologist. In his Peloponnesian War [1.10] he predicted what the remains of Athens and Sparta would look like if they were destroyed by an enemy. Then he formulated a law on the relations between artefactual data and archaeological interpretations and concluded that Homer was trustworthy in speaking of “mighty Mycenae” [Iliad 2.568–79] in spite of the fact that in Thucydides’s time it was a small town. Science is a matter of common sense, as Watson aptly puts it.

Watson’s main purpose is obviously not to demonstrate what the New Archaeology has accomplished but to defend archaeology against radical sceptics such as Shanks and Tilley [1987a, b], and this is very important. Watson wisely notes that archaeology is not philosophy. Philosophical scepticism cannot be refuted, but there is no more reason for scepticism about the results of archaeology than about those of geology, physics, or medicine. Such scepticism about archaeology may be unwise, but one consequence of it is much worse, namely, the attitude that anything goes, that one is free
to use archaeology for any purpose, including personal gain or political propaganda. To do so would be a misuse of archaeology of a kind actually practiced in some European archaeologies in the years 1933–53. Such tendencies should, as Watson remarks, be exposed and opposed.

Watson’s paper is a breath of fresh air, and I readily forgive his last, joking remark. Archaeological theory is analysis of the central conceptions of our research, and it is not boring at all.

ROBERT W. PREUCEL
Department of Anthropology, Harvard University, Cambridge, Mass. 02138, U.S.A. 14191

Watson raises a number of interesting issues in his didactic article, which might better have been entitled “Remedial Analytic Philosophy for Archaeologists.” The principal stimulus for his contribution seems to be the recent critique of processual archaeology by a group of archaeologists favoring humanistic approaches that have collectively come to be known as postprocessual archaeology. As can be seen from his recent American Antiquity article [Watson 1991], he is particularly incensed by some of the skeptical arguments of Shanks and Tilley [1987a, 1988b]. While I find myself in substantial agreement with Watson on the importance of a scientific archaeology, I differ strongly with him on what constitutes science. Specifically, I favor a broader definition that has room for both processual and postprocessual programs.

Watson appears to recognize no significant methodological distinction between the fields of geology, paleontology, anthropology, and sociology. In doing this, he is adopting the legacy of the logical positivists. For example, Neurath [1973:363] argued that “sociology is an empirical discipline like astronomy, people can be compared to clusters of stars which are in closer contact with each other than with other star clusters”, this is the famous unified-science thesis. However, a number of influential philosophers from the late Ludwig Wittgenstein to Hans-Georg Gadamer [and especially the Continental tradition of philosophy] have taken an alternative position that there are significant differences between the natural sciences and the social or human sciences, differences that require separate methodologies. Gadamer [1989:18] writes, “What makes the human sciences into sciences can be more easily understood from the tradition of Bildung (culture) than from the modern idea of scientific method.”

Where one stands on this debate (and there are in fact other positions than the two I have outlined) really turns on the priority one gives to different research interests. As I have argued elsewhere [Preucel n.d.], at least two different goals—explanation and understanding—drive archaeology, and these may require different methodologies. If we seek to elucidate the functional relationships within natural systems (explanation), then we have no choice but to treat past human actors as objects in our analyses. For example, we may use isotopic analysis to answer a question regarding the importance of corn agriculture to a specific population at a given point in time [e.g., van der Merwe and Vogel 1978]. If, however, our goal is comprehending the meaning underlying a specific social act [understanding], then we must attempt interpretation by treating past human actors as knowing subjects. This might involve investigating the meaning of differential control of production and reproduction for men and women during the European Neolithic [e.g., Hodder 1984]. Both of these approaches have long pedigrees within the social sciences and should be encouraged within archaeology.

What, then, are we to make of the critiques by Shanks and Tilley that Watson sees as threatening the very integrity of science? Here I would point to the distinction between scientism and science. The former involves the reification of scientific principles and the rejection of forms of knowledge that fail to conform to the standards of the dominant research paradigm, while the latter refers to the systematic study of human behavior within established disciplinary structures. As I read them, what Shanks and Tilley are calling into question is not science but rather certain attributes that often attend its practice. In particular, and following the lead of the Frankfurt school, they attack such accepted assumptions as objectivity and rationality that have served to insulate science from critique. It simply does not follow to argue that they wish to give up such obvious advances as radiocarbon dating or bone-chemistry analysis. Yes, science works, but Shanks and Tilley are challenging us (albeit in a rather polemical style) to make it work better. I think that the question must be asked why it is that we can send a man to the moon but have made almost no progress in solving questions of social justice.

Most surprising to me is Watson’s claim that philosophy has no direct relevance to science and archaeology. As an archaeologist, I find myself in the awkward position of defending philosophy to a philosopher. For me, the relevance of philosophy lies in its principled questioning of the nature of reality (ontology), how we know that reality (epistemology), and how we use our knowledge about it (ethics). I think that it would be a mistake for any field to privilege philosophy over practice (even Quine argues that there is no First Philosophy), but it seems self-evident that it is appropriate and indeed necessary for us as scientists to have some idea where our ideas come from. This is especially the case when we must evaluate different research programs with radically different philosophical bases that are competing for attention, as is currently the case in archaeology. Watson seems to suggest that we can afford to ignore them and go about our business. But I submit that the very language that we use (such as selectionist theory or social action theory) and the questions we ask (such as why a site is being excavated in the first place or why a particular explanation or interpretation seems appropriate) have philosophical content.

This position would seem to be supported in part by the growing number of philosophers who have now turned their attentions to archaeology. Jim Bell, Marsha
Hanen, Merrilee Salmon, Wesley Salmon, and Alison Wylie come immediately to mind. The issues addressed by these scholars range widely and include such topics as deduction and induction [M. Salmon 1976], systems theory [M. Salmon 1978], causality and explanation [Bell 1982, W. Salmon 1982], analogy [Wylie 1985a], structuralism [Wylie 1985b], and critical theory [Wylie 1985b]. Merrilee Salmon has written a book entitled Archaeology and Philosophy [1984] and Jane Kelley [an archaeologist] and Marsha Hanen have even collaborated on a book entitled Archaeology and the Methodology of Science [1989]. This research constitutes an important body of work that is only now being assimilated by those archaeologists who are writing on philosophical issues.

In his conclusion, Watson states that he will regard his paper as a success if it can be said that he has demonstrated that theory is irrelevant to practicing archaeology. I take this comment to mean that he hopes that archaeology will not become bogged down in the philosophical mires that have proven so sticky in other fields and thus become immobilized. However, I see no easy way around this. If archaeology is to be taken seriously as a social science (and I think it should be), it must necessarily confront these issues on its own terms. This will require not that every archaeologist be a philosopher but rather that every archaeologist think about the practice of archaeology. To do otherwise would reduce archaeology to a technique and the archaeologist to a technician. I doubt very much that this process will impede the development of archaeology as a discipline. In fact, I fully expect it to reinvigorate our field and help ensure its future in a rapidly changing world.

ZDENĚK VAŠIČEK
26 Louvois, F-78140 Velizy, France. 18 XII 90

If we were to replace “archeology” in Watson’s article with “historiography,” “learning about the past,” or even “anthropology” or “sociology,” his argumentation would remain valid. [Or would he allow for exceptions?] Through this substitution I refer Watson to all the critiques of similar approaches in the social sciences and the humanities. For my part, I wish to point out the following problems:

According to Watson, in archeology we propose and test hypotheses created on the basis of common sense and scientific knowledge acquired from other sciences—at present specifically the social sciences. Hic Rhodus, hic salta—where do they get their hypotheses? Common sense must be their starting point, and Watson mentions this without going into any detail. I believe that common sense is characterized, apart from generally shared truisms, especially by the persuasive method. Rather than prove, it convinces; “after all, what people do is the object of consultations and reflection. . . . We consult each other on matters which, it seems, can be one way or another” [Aristotle Rhetoric 1357]. Facts from the human past were likewise created more on the basis of persuasion than of explanation and prediction. Obviously, it is insufficient to subject them to lawlike statements and probabilistic covering laws. We can go as far as Danto [1965], who substantiated the explanatory power of narration, but in this case Watson’s argumentation clearly loses ground.

Watson mentions only hypotheses that can be tested, without taking into consideration their various levels and backgrounds. These are paradigms [Kuhn 1962] that select, among the numerous truisms, analogies, “cautious” generalizations, normic assertions, and lawlike statements [Scriven 1960], what is suitable as a hypothesis, depending on the individual case or on convention. Furthermore, every hypothesis is part of a cluster of statements that are interlinked and that make one another more precise. Archeology constantly drags its hypotheses along with it—what is its set of terms [culture, type, mode, etc.] but one great hypothesis? It therefore cannot be reduced to the confirmation of partial hypotheses and to the logical analysis of its terms. Knowledge is restructured over time by individual orientations, which may emanate directly from philosophy. For example, the archeologist Ernst Wahle [1941] was inspired by the philosopher Heidegger. According to him, cultural changes are abrupt and originate in small groups, and their occurrence cannot be perceived by means of archeology. Development is therefore represented by disconnected periods, and this is why archeology must be oriented towards the study of settlement. [In order to mitigate any conceivable reproof for irresponsible libertinage, I would point out that Wahle was an opponent of Nazism.] In addition, I mention his case to demonstrate the necessity of incorporating into our understanding what we do not know, i.e., the gaps in the material available.

Knowledge may also be structured by the manner in which it is presented, and here we touch upon the issue of the image of the past. In order to have an understanding of the world, one must have an image of it, and this image probably does not provide an explanation or prediction. The same is true of images of the past [which are not limited to linear narration]. In antiquity man valued the creation of such images and not merely the manipulation of the world, and it is because of this that piecemeal engineering in exploring the past, however beneficial and respectable, leaves much to be desired.

Reply

RICHARD A. WATSON
St. Louis, Mo., U.S.A. 9 II 91

First, I appreciate and (without responding to each) concur with the commentators’ constructive remarks.

Second, by common sense I mean common. It is not societally or culturally determined as Leonard suggests. It consists of such facts as that the world is full of things, among which are animals, plants, and rocks; that fire
burns, that cuts hurt, that water quenches thirst, and that we have to eat to live; that if you fall 15 feet you’re likely to break a leg and if you fall 60 feet you’re dead. Basic common sense probably hasn’t changed much in the last 40,000 to 50,000 years. Dunnell [1982:12] makes reference to this universal common sense when he says, “Our simple existence is ample proof of its power.” But then he loses his grip on it, continuing, “Anthropologists are well aware that common sense is a cultural phenomenon, that there are as many common senses as there are people living and solving the problems of living.” None of these “common senses” are common sense. They are customs, mores, rituals, rules, methods, beliefs—of which there are thousands of varieties elaborated around such commonplaces as that we all drink water. There is no such thing as European or Asian common sense, but there are European and Asian cultures.

But didn’t people once think that the earth was flat? Of course, and if you build a house or drive across town, you’d do well to act on that belief. For planning airline routes from St. Louis to major European cities, it would be better to think of the world as round. But these are matters of good sense, not common sense. There is plenty of evidence for a universal common sense [see the excellent empirically supported elaboration of this thesis by Atran 1990]. The distribution of good sense is another matter.

Third, theory: Of course archaeologists should be concerned with theory in archaeology, for example, the theory that climate affects culture. Such a theory both guides research and is confirmed or disconfirmed by research results. As is often pointed out, this interdependence results not in circular reasoning but in a progressing spiral that charts the growth of knowledge. Most of the philosophers writing on archaeology cited by Bell and Preucel are concerned with theory—and matters of logical analysis—within archaeology. I argue that archaeologists need not—and should not—concern themselves with metaphysical theories—for example, that archaeological results demonstrate the providence of God in human affairs or that the world is really will and idea or entirely material. Ordinary empirical data—of the sort archaeologists deal with—cannot provide evidence for answers to such questions as whether God exists or whether idealism or materialism is true. These metaphysical matters are not unworthy of attention; there is just no point in scientists’ worrying about them. Scientists can also be metaphysicians, of course, although, as Preucel intimates, scientists untrained in the pitfalls of philosophy often profess a particularly naive and pernicious form of scientism. As Adams says, however, archaeology is a field science. I agree that the substantive accomplishments of the New Archaeology are so extensive and well known that there is no point in detailing them, so I stress the main methodological accomplishment of the New Archaeology, which is to focus attention on the evidential bases of knowledge claims.

Finally, while there is a terminology specific to archaeology with which to elaborate theories in archaeology, there is no separate science of archaeology in the sense that physics is separate, say, from psychology (which is, of course, a moot point; see, e.g., Manicas 1987). Although there can be various theories about the utility and ideological or moral status of archaeology, there can be no separate theory of archaeology. Archaeology is basically a branch of history for which primary data are gathered by digging in the ground. Techniques from geology, biology, botany, and other natural sciences and insights from psychology, sociology, and ethnographic analogy are used to interpret the remains of human activities. Archaeologists also provide data useful for confirming or disconfirming lawlike generalizations, although, given archaeology’s dependence on the other human sciences, it is not surprising, as Leonard points out, that the New Archaeologists have established very few if any new laws of human behavior. Archaeologists do, however, discover many previously unknown relationships among and within the physical environments and human cultures of the past.

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Wanted


Contributions to the eighth volume of History of Anthropology, to be devoted to the fin de siècle [with perhaps some spillover into the first decade of this century]. The editor will welcome work on any of the fields of anthropology in any national tradition and from a variety of approaches—conceptual, institutional, biographical, comparative, contextual, etc. The volume is scheduled to appear in December 1992; the anticipated deadline for completed manuscripts is October 1, 1991. Prospective contributors are, however, encouraged to communicate with the editor as soon as possible to indicate the lines along which their contributions might develop. Write: George W. Stocking, Jr., Department of Anthropology, University of Chicago, 1126 E. 59th St., Chicago, Ill. 60637, U.S.A.