

for conceptualizing inquiry in these terms—as a process of “inference to the best explanation” (Hanan and Kelley 1989)—is that it makes clear the dynamic, provisional nature of hypothesis evaluation.²⁶ That archaeologists can rarely establish conclusive grounds for accepting one hypothesis over others does not make their judgments wholly subjective, a matter of arbitrary speculation or convention. None of the diverse factors that enter into archaeologists’ assessments is decisive, but all can provide a basis for eliminative induction by which the field of alternative hypotheses can be narrowed and nuanced judgments made about their relative credibility.

Despite taking quite different positions on the question of explanation, however, Hanan and Kelley share with Gibbon a strong commitment to broaden the scope of analytic philosophy of archaeology. All make the case that philosophical analysis cannot proceed alone; it must be aligned with an investigation of the history and sociopolitics of the discipline. The debates that unfolded in archaeology in the 1970s and 1980s demonstrate, Gibbon argues, that the problems, conventions of practice, even the epistemological commitments that define dominant “research programmes,” are profoundly shaped by nonepistemic factors; in this regard, archaeology is “a more uncertain, open, challenging and perhaps anxiety-ridden enterprise than our positivist heritage has indicated” (1989: 180). If practitioners are to make well-informed choices about how to proceed under these conditions, they must not only make broader, more discerning use of the resources offered by philosophy of science but also must understand how archaeology “relate[s] to its historical and social contexts” (Gibbon 1989: 180; see also Trigger 1989b: 27–72; Meltzer 1989 and other contributors to Christenson 1989). In particular, Gibbon argues, to systematically assess the New Archaeology and its alternatives we must understand why Hempelian positivism exerted such a powerful influence on North American archaeologists in the 1960s and 1970s, and this, he insists, can be provided only by an “anthropology of [archaeological] knowledge” (1989: 178).

Kelley and Hanan argue the need for a robust sociology of archaeology on principled as well as pragmatic grounds that reflect their conviction that philosophy of science must be “rooted in the science” and, more specifically, that “the interac-

tion between philosophy and archaeology must be an on-going, two-way exchange” (1988: 22). One of the central conclusions they draw from the range of case studies they develop is that epistemic considerations always operate in conjunction with “various non-scientific or contextual/sociological factors” in determining which ideas will be “accept[ed] into the working body of knowledge” (277). To understand archaeological practice accurately and in detail, it is crucial that these sociopolitical factors be as much the object of investigation as more traditional cognitive, epistemic considerations. Kelley and Hanan hasten to add that in advocating this holistic approach (350)—one that not only roots philosophy in archaeology but aligns it with an empirical sociology of archaeology—their purpose is not to endorse a corrosive relativism. There is a pressing need, they argue, to rethink notions of epistemic justification; philosophical approaches that conceptualize science as rational in a narrow sense are inadequate empirically and normatively (161), but so too are the constructivist conclusions drawn by advocates of the most uncompromising SSK research programs. If science is reduced to the play of (nonrational) sociopolitical and ideological factors, its growth and its successes become inexplicable (160–162). The point is to take stock, realistically, of the status of archaeological knowledge claims, and of the prospects for enhancing their accuracy, scope, and credibility, given the conditions under which they are produced.²⁷ Here, then, is a brief for analytic metaarchaeology that is oriented to the goal not just of understanding but also of improving archaeological practice; it is, in consequence, a resolutely hybrid and naturalized (qua socialized and humanized) enterprise.

ANTI-SCIENTISM

By the early 1980s, when philosophical studies of archaeology were taking shape, the archaeological debate about disciplinary goals was fundamentally reframed from within, by challenges from postprocessual critics. They rejected not just specific aspects of the New Archaeology as a research program but its whole scientific orientation. These challenges had the effect of displacing questions about explanation in favor of those that Kelley and Hanan foreground: questions about whether or in what sense archaeological claims

are ever justified, whether they be descriptive or explanatory.

Where the explanatory goals of archaeology are concerned, postprocessual critics focused their critical attention on the ecosystem conception of the cultural subject with which the covering law model advocated by the New Archaeologists was aligned. They rejected the programmatic claim, which Lewis Binford still defends, that archaeologists should concern themselves exclusively with interactions between cultural systems and their environments, bracketing the ethnographic life-world and “paleopsychology” on the grounds that it is explanatorily irrelevant (as causally inefficacious) and epistemically inaccessible. Ironically, postprocessualists offer empirical arguments for an enriched conception of the cultural subject; they point to a range of archaeological cases in which the variability evident in material culture cannot be explained in strictly functional and ecological terms, and they identify ethnohistoric contexts in which the intentional, ideational dimensions of cultural life play a crucial role in shaping the large-scale, long-term development of cultural systems (e.g., Hodder 1982a, 1983b).

With these arguments, postprocessualists renew the case for humanistic and historical approaches to archaeology that had been displaced by the New Archaeologists (e.g., as articulated by MacWhite 1956; Lowther 1962; and, most famously, by J. Hawkes 1968). If archaeologists are to understand the cultural past as *cultural*, they must grasp what Collingwood described as the “insides of actions” (Collingwood 1946; Hodder 1991)—the intentions and beliefs of agents and the systems of intersubjective meaning that inform their actions—however inscrutable or inferentially distant these may be. And to do so, they must explore strategies of inquiry that make possible the interpretive understanding of cultural material as the meaningful products of “rule-following” action, encoding or bearing meaning, rather than of “law-governed” behavior. Postprocessualists have drawn inspiration, in this connection, from symbolic and structuralist trends in anthropology (Hodder 1982a, 1982b), hermeneutics (Johnsen and Olsen 1992; Hodder 1991), phenomenology (see n. 4 to this chapter; Byers 1992, 1999; Tilley 1990, 1993; P. Watson and Fotiadis 1990), and critical theory (Leone 1982a; Preucel 1991a; see also Hesse 1992). In an elegant philo-

sophical analysis of systematic ambiguity in what archaeologists mean by “the archaeological record,” Patrik (1985) identifies the interpretivist approaches emerging in the mid-1980s as part of a long-standing tradition in which archaeological material has been treated as a textual record of intentional action rather than as a fossil record that requires scientific modes of explanation.

Despite the tendency to regard strategies for “recovering mind” (Leone 1982b) as non- or anti-scientific alternatives to the New Archaeology, the case is often made that they are in fact a necessary complement to, or component of, a scientific methodology. Some of the earliest arguments for structuralist approaches came from historical archaeologists who were as intent on making their field scientific as they were on grasping the intentional dimensions of their subject (see, e.g., the commentary offered by Fitting 1977: 63–67; Deetz 1967). Consider, too, the case Trigger made in the late 1970s for recognizing that the scientific goals of the New Archaeology depend on historical reconstructions of the cultural past, that is, on culture history; he insists that it is a mistake to treat these as independent and opposed alternatives (1978). In a similar spirit Deetz advocates a pluralism that can accommodate a “scientific humanism and humanistic science” (1983), a theme that recurs in Young’s argument for drawing on narrativist philosophy of history for models of interpretation in prehistoric archaeology (1988).

Increasingly, those who advocate explicitly scientific approaches, including some who have been outspoken critics of postprocessualism, declare that the tools of a scientific archaeology can and should be used to investigate the cognitive dimensions of the cultural past (Cowgill 1993; Gardin and Peebles 1992; Bell 1994; Renfrew 1982b, 1993a). Even direct heirs of the New Archaeology call for a renewal of the behavioral archaeology, originally advocated by Reid, Rathje, and Schiffer (1974), that emphasizes questions about such intangibles as religious practice (see, e.g., Walker 1995 and other contributors to Skibo, Walker, and Nielsen 1995). Some unreconstructed eco-materialists and evolutionists do reject outright any such humanizing of the cultural subject and now favor an even more reductive scientism than that originally embraced by New Archaeologists (e.g., Dunnell 1989a; O’Brien and Holland 1992, 1995; Lyman and O’Brien 1998 in response to Schiffer

1996 and Wylie 2000b). For the most part, however, all parties to the debate about the conceptual foundations of archaeology agree that if you are committed to empirical inquiry, you cannot presume to settle, *a priori*, questions about what factors will prove to be causally or explanatorily relevant for understanding the cultural past. Consequently, most now accept that models of explanation must be flexible enough to accommodate reconstructions of beliefs, intentions, cultural conventions, and social institutions—the ideational dimensions of human life—even though these are unlikely to be law-governed (Nickles 1977) or accessible to material-causal analysis (M. Salmon 1982; Levin 1976).

What made postprocessual arguments so contentious a challenge to the New Archaeology was the fear that if the ideational dimensions of the past are in fact radically inaccessible to scientific modes of investigation, then a commitment to understand them will force a return to the speculative induction that a resolutely scientific archaeology was meant to displace. Indeed, the most confrontational postprocessualists did endorse precisely this conclusion. They insisted that there was, in effect, nothing to lose by expanding the scope of inquiry to include even the most elusive aspects of the past; the scientific ambitions of the New Archaeology are unrealizable in any case. If archaeological evidence is inevitably theory-laden, then it must be admitted that archaeologists simply “create facts” (Hodder 1983a: 6; see also 1984a): there are thus no independent empirical grounds for testing reconstructive or explanatory claims about the cultural past (Shanks and Tilley 1987: 111). Pushing this antifoundationalism to its limit, Shanks and Tilley make the case for an uncompromising social constructivism. All claims to objectivity are a pretense; the best archaeologists can do is to make their interests explicit and hold their claims about the past politically accountable. In the end, few postprocessuals have consistently maintained so strong a constructivist line (see chapter 12). When they move beyond the critique of processual archaeology and advance counterclaims of their own, they typically endorse a pluralism; on most formulations this allows for multiple interpretations of the past but also leaves room for the judgment that some claims are more plausible than others—indeed, some are simply untenable.²⁸ It is as important to postprocessual-

ists as to processualists to exploit the capacity of archaeological data to selectively resist “theoretical appropriation” (Shanks and Tilley 1989: 44).

MIDDLE-RANGE THEORY AND ACTUALISTIC RESEARCH

Although these challenges to the scientism—specifically, the objectivism and foundationalism—of the New Archaeology generated a highly polarized debate that has persisted since the early 1980s, postprocessualists were not alone in raising difficult questions about the status and stability of archaeological evidence. As soon as the New Archaeologists undertook to implement the testing methodology they hoped would obviate dependence on inductive inference, they confronted the problem that to assess the implications of archaeological data for a particular test hypothesis, they had to develop “arguments of relevance” (J. Fritz 1972: 140), or “bridging arguments” (B. Smith 1977: 611), that link surviving elements of the archaeological record to the past events and conditions that produced them. In this archaeologists necessarily rely on auxiliary hypotheses—various forms of background and collateral knowledge—to establish the significance of archaeological data as evidence (M. Salmon 1975). By 1977 Lewis Binford had taken the point that archaeological data stands as evidence only under interpretation: “the scientist must use conceptual tools to evaluate alternative conceptual tools that have been advanced regarding the ways the world works” (1977b: 3). And a few years later, writing with Sabloff, he invoked Kuhn in an argument to the effect that theory-ladenness (and paradigm dependence more generally) is an unavoidable fact of scientific life (L. Binford and Sabloff 1982).

None of this undermined the positivist commitments of New Archaeologists like Binford; their confidence in empirical testing was unshaken so long as the argument could be made that there are means of rationally, empirically evaluating the background knowledge, and even the paradigms, on which archaeologists depend (e.g., L. Binford and Sabloff 1982: 139). The response of processualists, prefigured by a long-standing interest in ethnoarchaeology and experimental archaeology (e.g., the “action research” advocated by Kleindienst and Watson in 1956), was to declare that a scientific archaeology must

systematically develop, or selectively borrow, the background knowledge necessary to establish reliable arguments of relevance—"ascriptions of meaning"—to archaeological data. Some advocates of actualistic research²⁹ maintained the deductivism of Hempelian models; their goal was to establish universal laws capable of retrodicting past events or conditions of life from their surviving material record. For example, John Fritz's central concern, in characterizing "systems for indirect observation of the past," was to show how arguments of relevance could be formulated that "meet the requirement of deducibility[,] . . . permit[ting] us to deduce the characteristics of the data from those of the past sociocultural phenomena we hope to observe" (1972: 149). This is a theme that recurs in the literature on actualistic research and, later, in that on "middle-range theory"; it is evident in Gould's uncompromising rejection of analogical inference (1980; see discussion in chapter 9), and in Schiffer's insistence that "arguments of relevance" are "nothing less than laws of cultural process" (1972b: 155). Schiffer drew the conclusion that the first priority for a scientific archaeology must be to establish a body of universal laws governing the natural and cultural "transforms" responsible for the archaeological record (1972a, 1972b, 1975, 1976).³⁰

Both philosophers and archaeologists have argued that the inability of all but a few of the claims archaeologists make about the import of archaeological evidence to meet Fritz's deducibility requirement does not entail the "hyperrelativism" associated with some forms of postprocessualism (Trigger 1989b). Archaeological claims are always defeasible, as postprocessual critics have argued; there are no absolutely stable and transparently meaningful empirical foundations on which they can be grounded. However, the very analyses that expose error demonstrate the potential for systematically adjudicating the (relative) credibility of competing hypotheses, whether they be explanatory, interpretive, or descriptive. The challenge is to articulate models of archaeological inference that capture the range of interlinked considerations bearing on these judgments.

In this spirit most philosophical commentators and a number of archaeologists have argued for a more complex and open-ended account of hypothesis evaluation than deductivist ideals allow. For example, Merrilee Salmon proposes a

modified Bayesian account; she conceptualizes judgments of evidential support as a matter of assessing the difference that new evidence makes to the prior probability of a hypothesis, and the likelihood that this evidence could occur even if the hypothesis were false (1982: 49–56). Building on some early suggestions of Salmon's (1975, 1976) and anticipating her later, more fully developed account of archaeological testing, Bruce Smith (1977) argued the case for a hypothetico-analog model of evidential reasoning that puts particular emphasis on its inductive character and the role of plausibility judgments. Hanen and Kelley push this line of argument further (1989; Kelley and Hanen 1988), stressing the importance of intra-theoretic consistency—the fit of new hypotheses with a conceptual core of established and background knowledge—in judging the relative credibility of competing explanatory claims. This is an approach Gibbon shares (1989), though as a realist he regards "best explanations" as those that afford the most comprehensive and plausible causal explanation of the available data. In a sophisticated argument for "typological instrumentalism," William Adams and Ernest Adams (1991) make a case for recognizing the role played not only by background knowledge but also by pragmatic considerations in constructing typologies and other tools of analysis.³¹ And in a series of analyses of the inferential processes underlying all forms of "archaeological construct," Gardin likewise eschews top-down, philosophically driven models, using what he describes as a "logicist" approach to capture the range of operations by which archaeologists proceed in even the most mundane practices of observation, description, compilation, and explanation. Despite the formalism of these models, Gardin is compelled by the practice he considers to foreground the selective, the interpretive, and even the normative dimensions of archaeological inquiry (Gardin 1980, and in Gardin and Peebles 1992; also Galloway 1989).

Increasingly, the justifications archaeologists offer for the development of middle-range theory suggest a range of alternatives that mediate between the extremes of a strict deductivism, on the one hand, and radical constructivism on the other (e.g., Tschauner 1996). This repositioning has led Kosso to argue that there is actually very little difference between the practice of processualists and that of postprocessualists; they exploit linking

principles in essentially the same ways, whether their goals are to establish causal explanations or interpretive readings of the archaeological record (1991).³² The philosophical theory that best captures these forms of practice, Kosso argues, is a sophisticated antifoundationalism that shares a number of key features with Kelley and Hanen's broadly coherentist account (1988); although there are no self-justifying grounds for belief (empirical or otherwise), the various constituents of networks of belief constrain one another in ways that can stabilize evidential claims (Kosso 1993).

The debate continues, however. In 1994 Bell renewed the arguments of the New Archaeologists against inductivism, translating the central insights of Popper's refutationism into a set of methodological guidelines for archaeological practice. Invoking Popper's famous rejection of Vienna Circle verificationism, he urges archaeologists to treat hypothesis evaluation not as a process of building evidential support for hypotheses but rather as a matter of subjecting bold conjectures to the most rigorous tests they can devise; what distinguishes genuine science from pseudo-science, on Popper's account, is not the degree of empirical support or the empirical content (the cognitive significance) of its constituent claims, but the uncompromising critical attitude that scientists bring to bear in evaluating these claims (Popper 1989: 50–52). To give these general guidelines purchase on archaeological practice, Bell extracts from Popper's critical methodology what he describes as a checklist of questions archaeologists should ask about the hypotheses they mean to evaluate.³³ Although Bell's objective is to bring philosophy into closer contact with the practical concerns of field archaeologists, this engagement between fields remains largely an exercise in exporting philosophical wisdom. He gives no indication that the Popperian models he advocates will be held accountable to archaeological practice. In fact, in cleaving to quite traditional, normative scientific ideals Bell sets aside the whole range of contextualist, antifoundationalist critiques, both philosophical and archaeological, that call into question faith in the capacity of evidence to decisively refute a test hypothesis. These are a long-standing source of intractable difficulties for Popperian theories, and they capture a methodological conundrum with which archaeologists have struggled with growing intensity and

sophistication since the advent of the New Archaeology: how to interpret archaeological data as evidence so that despite its theory-ladenness, it retains a capacity to challenge our expectations about the past and even, on occasion, to subvert the framing assumptions that inform the research enterprise as a whole.

THE SOCIOPOLITICS AND ETHICS OF ARCHAEOLOGY

For all its acrimony, the polarized debate between processualists and postprocessualists has had the salutary effect of giving new prominence to questions about archaeologists' social and historical location, and about their political and ethical accountability. These questions have been taken up in two different connections.

On the one hand, a growing contingent of critical archaeologists have used historical and sociological tools to document the influence on archaeology of its colonial, nationalist, and imperialist entanglements (Trigger 1989b); its relationship to intranational and international elites and its class structure (Patterson 1986a, 1986b, 1995b); its assimilation of racist and sexist presuppositions (Trigger 1980; Gero and Conkey 1991; Moser 1996); and myriad features of its funding base, internal communication patterns, institutionalization, recruiting and training, and reward structures (Gero, Lacy, and Blakey 1983; Kelley and Hanen 1988; contributors to Pinsky and Wylie 1989; Gibbon 1989; Moser 1993, 1996, 1998; Molyneux 1997; Shelley 1996). These studies reinforce contextualist and constructivist arguments for rethinking ideals of objectivity that make a primary virtue of neutrality and value freedom; they have both arisen from and provided the impetus for postprocessual challenges to the scientism of the New Archaeology. At the same time, however, the goal of critical archaeology is often centrally constructive (Kelley and Hanen 1988): it is to ensure that archaeologists are accountable for their presuppositions and to provide a basis for better-informed judgments about the credibility and likely limitations of archaeological knowledge (e.g., Leone, Potter, and Shackel 1987; Preucel 1991a).

On the other hand, a broad cross section of archaeologists have taken up normative, sociopolitical issues in connection with questions about

their professional and public responsibilities. Pressure to consider such issues has been mounting since the early 1970s, when it became clear that the future of archaeology was threatened worldwide by rapidly accelerating destruction of archaeological resources and an unprecedented expansion of the international antiquities market (e.g., Lipe 1974; E. Green 1984). In the same period, archaeologists have faced increasingly vocal and powerful challenges, at home and abroad, from a range of external interest groups who oppose their use of archaeological sites and materials; most prominent among them are indigenous peoples around the world, especially Native Americans, who object that scientific investigation does not serve their interests in preserving what they regard as their cultural heritage. At the same time, as a growing majority of archaeologists find employment in government agencies and industry, internal debate about professional accountability has intensified. Attention focuses on such questions as whether archaeologists are ever justified in making professional use of looted or illegally traded material; whether the goal of preserving archaeological resources should be as central as that of investigating the record for scientific purposes; what responsibilities archaeologists have to the diverse communities affected by their research, especially descendant communities; and how the goals of scientific investigation are to be weighed against heritage interests when these conflict (see, e.g., E. Green 1984; M. Salmon 1997, 1999b; and contributions to M. Salmon 1999a and to Vitelli 1996). Although the discussion of these issues has taken a course of its own, it does impinge in important ways on questions about the goals and epistemic status of archaeology; the need to ground analysis of archaeological practice in an understanding of the contexts of practice now takes on an explicitly normative, as well as sociological and historical, dimension.

What has emerged as analytic metaarchaeology is thus a motley, disunified subfield—or, more accurately, interfield—located at the intersection of archaeology, philosophy, and a growing body of internal historical and sociological research on archaeology. *Analytic* metaarchaeology was initially the product of a philosophical engagement with archaeological practice, but it has generated a body of work that is neither strictly internal to archaeology nor altogether assimilable to

the preoccupations of philosophers. Its status is, in part, a function of the naturalizing turn that is substantially reshaping philosophy of science; many philosophical questions about science are now recognized to require forms of investigation that integrate conceptual analysis with empirical (historical, social scientific, psychological) studies of scientific practice. In the case of analytic philosophy of archaeology, the resolve to take this naturalizing turn has been reinforced by the collateral growth within archaeology of highly sophisticated normative, historical, and sociopolitical studies of archaeological practice. In this regard, analytic metaarchaeology exhibits all the strain and uncertainty attendant to grounding philosophy in science. At the same time it illustrates concretely just what can be gained when philosophers reopen the question of what their tools of analysis have to offer an empirical discipline like archaeology and also what they stand to learn from an understanding of real rather than idealized (“fantasy”) research practice.

WHAT FOLLOWS

Coming into analytic philosophy of science and archaeology in the early 1970s, I took it for granted that both fields were undergoing a sea change. The analyses that follow are all, in one way or another, a legacy of the interfield connections forged both by the intense philosophical interest of archaeologists and by a growing commitment among philosophers of science to ground their analyses of science in the sciences themselves. In this spirit I have proceeded as a hopeful amphibian and naturalizer; I address questions that arise as much from the philosophical complexity of archaeological practice as from reflection on the archaeological fitness of philosophical models of science.

At first I was struck by incongruities that had drawn the attention of philosophical commentators in the early 1970s. The positivism endorsed by the New Archaeologists seemed fundamentally at odds with their expansive anthropological, processual ambitions. They insisted that “space-time systematics” must not define the limits of archaeological inquiry, and yet they advocated positivist models of explanation and confirmation that presuppose a view of science according to which its primary aim is to systematize observables. In-

deed, the New Archaeologists demanded no less than a Kuhnian revolution, but they invoked precisely the empiricist/positivist “building block” model of scientific inquiry that Kuhn repudiated. What I found puzzling at the time was not so much how such philosophical incongruities could arise—what misconceptions about the content or purposes of philosophy they revealed—but why philosophical models of science should have seemed relevant to archaeological practice in the first place. In Collingwoodian terms,³⁴ I wanted to understand what the questions were to which logical positivism seemed a compelling answer, despite philosophical contraindications.

I learned that the New Archaeology was not altogether new; it is structured by debates about the goals and strategies of inquiry that have deep historical roots. At the heart of those debates is a methodological dilemma that has resurfaced, in increasingly polarized terms, every twenty or thirty years since the early twentieth century, when North American archaeology was rapidly becoming professionalized and institutionalized: if archaeologists pursue anthropological goals it seems unavoidable that they will overreach the limits of their evidence, risking the pitfalls of arm-chair speculation; and if they honor a commitment to rigorously scientific modes of practice (construed in empiricist terms) it seems that they must largely restrict inquiry to the recovery and systematic description of the archaeological record. The New Archaeologists sought to circumvent this dilemma by showing how, properly conceived, the tools of science might be harnessed to anthropological goals; if archaeological data were used systematically to test speculative hypotheses, the requirements of empirical rigor might actually support, rather than mitigate against, ambitious explanatory and interpretive goals. Ironically, however, when the New Archaeologists invoked Hempelian positivism as a source of guidelines for reframing research practice, they reinscribed at the conceptual core of their program the very dilemma that they sought to escape. The conclusion I drew in a doctoral dissertation titled “Positivism and the New Archaeology,” written between 1979 and 1981 (Wylie 1982c), was that a conceptual fault line ran through the New Archaeology; the inherent tensions between the substantive objectives of the program and its positivist commitments could not but generate a new

internal crisis. In part II, I outline the history of the philosophical and methodological debate that prefigured the New Archaeology, elaborating the analysis I have given here of the tensions that were inherent in the program at its inception.

As processual archaeology took hold in increasingly diverse research settings, tensions also emerged between what New Archaeologists recommended and what they actually did. When they followed the directives of a strict deductivism, the results were often acknowledged to be trivial. Flannery caricatures the fruits of these labors as “Mickey Mouse laws” and invokes the wisdom of a colleague: “if this is the ‘new archeology,’ show me how to get back to the Renaissance” (1973: 51). But when New Archaeologists kept in view a fundamental commitment to explanatory goals, they made much more complex and interesting use of the standard resources of archaeological inquiry, both conceptual and evidential, than could be captured by Hempelian idealizations. Part III consists of essays in which I undertake to disentangle these promising and innovative aspects of the New Archaeology from its positivist commitments. In chapter 7 I make the case in general terms that in their practice if not in their programmatic statements, the New Archaeologists make good use of a number of research strategies that go some distance toward finessing their recurrent interpretive dilemma.

It is a mistake, however, to expect that these forms of practice will establish archaeological conclusions with deductive certainty. With few exceptions, archaeologists depend at every turn on broadly inductive forms of inference: interesting conclusions inevitably extend well beyond any evidence or reasons that can be provided in their support.³⁵ What is obscured by the New Archaeologists’ uncompromising anti-inductivism, but made clear by their practice, is that this need not be a counsel for despair. There is no question that the kinds of explanatory and interpretive claims New Archaeologists hope to establish are, to varying degrees, uncertain. Nonetheless, it does not follow that all claims about the cultural past are equally and radically insecure. The challenge is to give a clear, closely specified account of how systematic distinctions can be made between relatively speculative and relatively secure claims: how the degree of support offered by ampliative inference can be assessed.

I take up these issues of epistemic credibility in more specific terms as they arise in connection with the New Archaeologists' rejection of any inference concerning the "insides of actions" (in chapter 8), their repudiation of analogical inference (chapter 9), and the arguments of critical archaeologists who question the strong objectivist claims of self-consciously scientific archaeology (chapter 10). My thesis is that a commitment to scientific, empirical rigor should not be construed so narrowly as to exclude these areas of inquiry or forms of inference. In chapter 11, I offer a general outline of the process of inferential tacking by which archaeologists put localized strategies of evidential argument to work; taken together, they exemplify the promise that there are options "beyond objectivism and relativism" (Bernstein 1983).

In part IV I take up these themes again, but in connection with the sharply polarized debate between processual and post- or antiprocessual archaeologists that took shape through the 1980s. In chapter 12 I argue that when archaeological strategies of inference are understood in more realistic terms than deductivist models allow, they can be seen to play as central a role in the critical arguments of postprocessualists as in the practice of self-consciously scientific New Archaeologists. In the three chapters that follow, I refine a model of the empirical and conceptual checks and balances that can ensure virtuous rather than vicious circularity in the theory-ladenness of evidence; my aim is to show how archaeological evidence can be an interpretive construct at every level, as postprocessual critics argue, and still (sometimes) impose significant empirical constraints on what we can plausibly claim about the cultural past. The key, I argue, lies in the role played by background and collateral knowledge in evidential argument: specifically, in considerations of the soundness of these sources, the variety of evidence they support, and various dimensions of

epistemic independence that can be established within and between lines of evidence.

To illustrate how this model works, in chapter 14 (as in chapter 10) I focus on examples that illustrate how deeply archaeological inquiry is shaped by its normative, sociopolitical, and historical contexts. In particular, I consider feminist analyses that throw into sharp relief the gendered dimensions of the archaeological enterprise. I argue that at the same time as these undercut objectivist pretensions to "a view from nowhere," they reinforce the conclusion that situated interests do not necessarily determine the outcomes of inquiry. As a thoroughgoing naturalist might expect, what balance of contributing factors must be considered in explaining the course and consequences of any given program of archaeological research is an open (and empirical) question. Part IV closes with a recent essay on models of explanation in which I examine arguments for and against treating the unifying power of an explanatory account as evidence of its credibility (chapter 16).

In a concluding essay, chapter 17 (which alone constitutes part V), I explore the nexus of ethical and epistemological issues raised both by internal and by external critics who ask "who owns the past?": whose interests are served by archaeology and what accountability do practitioners have to descendant communities, to others who are affected by their work, to a broader public, and to the range of interests evoked by the conservationist slogan "save the past for the future"? It is increasingly in this arena of debate that questions about the goals of archaeology, its identity, and its standards of practice are addressed, recast as normative questions of accountability. Clarke's injunction to abandon innocence is more apposite now than ever before; there is very little an archaeologist can do that is epistemically, ethically, or sociopolitically innocent.