RETHINKING UNITY AS A 'WORKING HYPOTHESIS' FOR PHILOSOPHY OF SCIENCE: HOW ARCHAEOLOGISTS EXPLOIT THE DISUNITIES OF SCIENCE

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As compelling as they once were, and as influential as they continue to be in many contexts of practice, global unity of science theses have been decisively challenged in all their standard formulations: methodological, epistemic, and metaphysical. It cannot be assumed as a normative ideal or even as a "working hypothesis" (Oppenheim and Putnam 1958) that the sciences presuppose an orderly world, that they are united by the goal of systematically describing and explaining this order, and that they rely on a distinctively scientific method that, successfully applied, produces domain-specific results that converge on a single coherent and comprehensive system of knowledge.

The question immediately arises, what follows from these arguments against unity, given that they represent not just the culmination of critical debate about a particularly influential view of science, but a challenge to assumptions that have very largely defined what it is to do philosophy of science? In this paper I consider the implications of disunity at two levels. I am concerned, first (and primarily), to delineate the scope of arguments against global unity theses. However much the weight of critical argument tells against old-style global unity theses, it is important not to lose sight of the fact that ideals of epistemic and methodological unity remain a powerful force in many sciences,[1] and that local and

1 In a paper written originally for a symposium at the 1999 Annual Meeting of the Society for American Archaeology, "Method and Theory 2000" (Wylie 2000a), I argue that these ideals of scientific unity underpin the protracted public debate now known as the Science Wars (e.g., as transacted by Gross and Levitt 1994 and Ross 1996):

> What science critics contest and science advocates defend is the very idea that there is such a thing as "science": a unified enterprise defined by a set of shared attributes that uniquely determine what it is for a discipline to be scientific, that set real science apart from other (lesser) epistemic enterprises, and that trump any non-scientific interests or knowledge claims that might challenge the epistemic authority of science. (Wylie 2000a: 229)

In the context of archaeology, I argued, these commitments are pivotal to a recurrent pattern of debate in which the credibility of a research program or theory or form of practice is defended (or rejected) on the basis, not of an assessment of its accomplishments or promise of its affiliation with "science"; debate turns on claims and counter-claims about which practices or theories or approaches are properly scientific, which display the essential (unifying) characteristics of science. My thesis was that, just as global unity arguments fail (for reasons I develop in more detail here), so too does this local strategy of legitimation. It is an attempt to settle in advance questions about research practice that can only resolved by evidence and experience. My recommendation for avoiding the pitfalls of Science Wars strategies of argument was, then,

> ...a variant on the advice offered by environmental activists: think globally, about the resources that a wide range of research fields have to offer archaeology, but make the case for the credibility and authority of

contingent unifying strategies are crucial to most scientific inquiry. I argue, with reference to the practice of historical archaeologists, that the inter-field and inter-theory connections necessary to support evidential claims represent a significant if perplexing unifying force, even if they do not support global unity theses. They establish a robust network of cross-connections that binds the sciences together. At the same time, however, the epistemic leverage they provide depends on significant and pervasive disunity in the sciences. In the final section of this chapter, I briefly consider some meta-implications of this argument for philosophy of science and for science studies more generally.

The Unity of Science as a Working Hypothesis

Methodological unity theses

Although claims of methodological unity were the cornerstone of expansionist programs in philosophy and in science in the nineteenth and early twentieth century, they received only cursory attention from such powerful advocates for unified theories of science as Oppenheim and Putnam who declared that, by 1958, this genre of unity thesis "appear[ed] doubtful" (1958: 5).[2] Indeed, such great nineteenth century systematizers as Mill and Whewell took considerable care to catalogue the diversity of methods developed by (successful) sciences and were divided in their assessment of whether or how these could be characterized in unitary terms.[3] Recent reexaminations show that even such a stalwart of the Vienna Circle as Neurath was more interested in the co-ordination of scientific methods than in methodological unity per se (Cat, Cartwright, and Chang 1996, Cartwright and Cat 1996). In short, both classical and logical positivists were equivocal in their endorsement of methodological unity theses. As Hacking puts the point with reference to unity theses of all kinds, two quite distinct senses of unity are at issue: unity qua "singleness" and unity in a looser, contingent sense that he describes as "harmonious integration" (1996: 41).

Twenty years after the appearance of Oppenheim and Putnam's declaration on behalf of unity, Suppes reinforced their caution about theses of methodological unity. He declared them unsustainable in any interesting form (1984 [1978]); if formulated in general enough terms to cover all scientific practice, they are likely to be trivial and to obscure more than they illuminate of the real complexity of scientific practice. They are, moreover, irrelevant; it might have been important to articulate a clear-cut definition of what counts as scientific method when science itself was in need of a philosophical defense, but by the late 1970s, Suppes argued, this was no longer necessary. It was time to turn our attention to "a patient examination of the many ways in which different sciences differ in language, subject matter, and method, as well as [to] synoptic views of the ways in which they are alike" (1984 [1978]: 125). Of recent disunity theorists, Dupré is most uncompromising in pressing this point; he argues that the quest for "general criteria of scientificity" (1993: 229) is largely irrelevant to current unity debates (Dupré 1993). Where methods are concerned, "science is [at best] a family resemblance concept" (Dupré 1993: 242; see also Dupré 1995), and where judgements of scientific credibility are it issue, the most promising and realistic strategy is to apply the standards of a flexible virtue epistemology on a case-by-case basis.

Debate on these issues is by no means closed. There are certainly advocates of more closely delimited methodological unity theses although they usually focus on particular features of scientific inquiry (e.g., models of explanation, confirmation, testing, and belief revision), and some who argue

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archaeological practice locally, in terms of particular interfield relations and their efficacy in solving specific archaeological problems. (2000a: 229)

2 Oppenheim and Putnam describe claims about the unity of scientific method and about logical unity as theses that "today appear doubtful" (1958: 5). By logical unity they mean theses according to which all terms of science are reducible to "sensationalistic predicates" or "observable qualities of physical things" (1958: 5).

3 Hacking makes this point with reference to Comte and Whewell (1996: 38).

strenuously against disunity critics on grounds that if they are right, meaningful distinctions between science and pseudo-science are irrevocably compromised and corrosive relativism is unavoidable (see Stump 1991 on challenges from Worral and Siegl). More modestly, Ereshefsky argues that, despite his disunifying ambitions, Dupré's catalogue of epistemic virtues[4] captures a "fairly stable core" of non-trivial but global features of scientific methodology (1995: 156).[5] In the end, however, what emerges is a decisive rout of theses of methodological unity theses that are global in scope and that posit the "singleness" of scientific method (to use Hacking's term). If they are characterized with any specificity, methodological strategies and standards do seem to be highly variable across the sciences, and they clearly evolve; they are responsive to the empirical conditions of practice (to subject domain) and to the interests of investigators. This appreciation of the complexity and diversity of scientific practice is reinforced as philosophers of science naturalize their practice and attend to the specifics of practice in an increasingly wide range of fields.

Epistemic and ontological unity theses

The unity theses that Oppenheim and Putnam endorse have to do with the content of science and its subject domain(s) rather than its methodology; I refer to these as epistemic and ontological unity theses although they did not.[6] At their most ambitious, the advocates of these theses postulate a hierarchy of micro-reductions that integrate all the sciences into one coherent system; the language and, more to the point, the laws and theories--in short, the content--of each science should (ultimately) derive from, or supervene upon, those of successively more basic sciences until you reach a "unique lowest level," a foundational science of elementary particles (Oppenheim and Putnam 1958: 9). In this they assume that an orderly and unitary structure of part:whole relations holds between the objects studied by sciences at each level; reduction is accomplished if it can be shown that a science at one level involves the study of objects that can be "decompos[ed] into things belonging to the next lowest level" (1958: 9). Oppenheim and Putnam are clear on the point that in the late 1950s actual science displayed no such ideal unity, although they note a number of "unifying trends" that warrant systematic investigation; ultimately, they insist, the "unity of science" is an hypothesis that "can only be justified on empirical grounds" (1958: 12). It is in this sense that it has served as the central "organizing principle" for a great deal of philosophical work on science in the last forty years.

In practice the interest in global epistemic reduction has fragmented into localized debates about the likelihood that micro-reductions will be realized between pairs of sciences: physicalist or materialist reductions of psychology to neuroscience; biochemical reductions of genetics; the "quantum takeover" in physics that has been contested by Cartwright (1995). And in virtually all such cases the prospects for reduction remain at least contentious and certainly distant. Even paradigm examples of apparently successful unification prove unexpectedly complex, providing, at best, equivocal support for epistemic

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4 The core virtues cited are: "sensitivity to empirical fact"; cohesion with things we know; reliance on plausible background assumptions; and exposure to as wide a variety of criticisms as possible. In addition, Ereshefsky observes that Dupré considers a number of other "free floating aesthetic virtues," such as unity, generality, and simplicity, that may be differentially relevant to some fields, or subfields, of science (1995: 157).

5 Note that such an argument does not establish the kind of unity thesis about scientific methodology that Dupré objects to--a monistic thesis that posits the uniqueness and "singleness" of scientific method--and it does not entail or support "scientific imperialism" (Dupré 1995). On Ereshefsky's account, the central challenge for philosophers of science is to understand, in local and contingent terms, how far Dupré's virtues extend across scientific disciplines and how they are realized in diverse contexts of practice. Ironically, similar arguments for studying, in fine-grained (naturalistic) detail the flexibility, adaptability, and mutability of scientific method have been made both by critics and by advocates of unity theses. Compare Laudan and Laudan, "Dominance and the Disunity of Method" (1989) with Giere, "Toward a Unified Theory of Science" (1984).

6 What Oppenheim and Putnam refer to as "epistemic theses" are claims about methodological and logical unity, the senses of unity they thought untenable (1958: 5).

and ontological unity theses. For example, Morrison argues that the unification effected by Maxwell’s electromagnetic theory (1992), and more recently by electroweak theory, is “structural rather than substantial” (1995: 369); unity is accomplished at a theoretical level by extension of a powerful mathematical formalism to diverse phenomena, but it leaves key elements of the constituent theories either uninterpreted or unreduced and provides little ground for claiming that any deeper (ontological) unity in nature has been discovered (1995: 372).[7] Far from establishing a part:whole relation between the forces or entities posited by the conjoined theories, Morrison argues that in the case of Maxwell’s theory physical interpretation of the unifying mathematical model remained a fundamental difficulty, while electromagnetic and weak forces remain distinct in electroweak theory. Here theoretical unity co-exists with ontological disunity (1995: 371), indeed, in the case of electroweak theory, “unity is achieved at the price of introducing an element of disunity” (1995: 369).

In a recent discussion of the “special” sciences (specifically, economics), Kincaid makes the complementary argument that even if we accept some form of metaphysical unity thesis it does not follow that we (“real human agents”) can or should make epistemic unity our central objective. The entities and events studied by social scientists may all be dependent upon or, indeed, constituted by their physical realization, but it does not follow that “lower level” physical theories should be granted explanatory primacy (1997: 3); higher level theories in the special sciences may well describe the causal dynamics of socio-political, cultural, or economic systems that cannot be strictly derived from physical theories. Insofar as the special sciences prove capable of establishing interesting (counter-factual supporting) generalizations--something that must be considered an open, empirical question--it seems unlikely that these will map onto physical descriptions of the objects and events they systematize. As Fodor put the point in 1974, “what is interesting about monetary exchanges [for example] is surely not their commonalities under physical description” (1974: 103-104).[8]

Dupré extends this line of argument, noting disjunctions between the theories produced not only by distinct branches of science but within them as well (this distinction is made by Davies 1996). Biologists actively debate divergent classificatory schemas all of which may be said to “cut nature at its

7 Wayne (1996) challenges Morrison’s account of the unification accomplished by electroweak theory. He argues that the structural unification she describes depends upon the selection of a particular “argument pattern”--a particular component of the complex array of formal models developed to make sense of different kinds of subatomic systems (1996: 399)--as the element of formal structure that will be held invariant through all transformations required to apply the theory to diverse systems. This choice, he insists, depends on prior ontological commitments that inform “an interpretation of the standard model that includes a small ontology of elementary quantum fields” (1996: 404), and it is these ontological commitments, not the formalism, that makes unification possible. This undermines Morrison’s argument that unification in the electroweak case is strictly structural, and may have important implications for her arguments against realist construals of theoretical unification (1990, as well as 1994), but it does not necessarily establish (nor does it seem intended to establish) that the case fits the theory reduction model associated with Oppenheim and Putnam’s “working hypothesis.” If anything, it suggests that electroweak theory is an example of the kind of “interfield theory” (discussed below) that Darden and Maull described in the late 1970s as a standard form of field-bridging development in science that does not fit and, indeed, was obscured by, a preoccupation with derivational reduction (1977).

8 Fodor elaborates this point later in his discussion:

> This brings us to why there are special sciences at all. Reductivism...flies in the face of the facts about the scientific institution: the existence of a vast and interleaved conglomerate of special science disciplines which often appear to proceed with only the most token acknowledgment of the constraint that their theories must turn out to be physics ‘in the long run’.
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> I am suggesting, roughly, that there are special sciences not because of the nature of our epistemic relation to the world, but because of the way the world is put together: not all natural kinds (not all classes of things and events about which there are important, counterfactual supporting generalizations to be made) are, or correspond to, physical natural kinds. (Fodor 1974, pp. 112-113)

joints" but reflect different selections of joints. These are distinguished, not by concern with different levels of reality that fit neatly together when parts are reassembled into wholes, but by an interest, pragmatic or scholarly, in different aspects of a complex reality and its diverse causes: "evolution, the source of biological diversity, is itself a diverse set of processes...[why expect] that it will give rise to any unique and privileged set of categories suited to the varied sorts of inquiries and interests that we bring to the study of biological organisms" (Dupré 1996b: 443)? It is no accident, Dupré concludes, that epistemic disunity seems to be the rule, rather than the exception. Reduction projects founder on an "apparent diversity and disorder of nature" that poses a fundamental (empirical) challenge to the metaphysical as well as the epistemic components of Oppenheim and Putnam's "working hypothesis."

One sympathetic critic objects that if Dupré is seriously committed to pluralism, he must allow that essentialist categories may yet prove viable in some areas (Ereshefsky 1995), while another who is less sympathetic argues that Dupré puts too much weight on the state of disarray in which he finds contemporary biology: perhaps "our epistemic situation now is most like that within late sixteenth-century astronomy where much integrative conceptual and empirical work lay in the future and the endorsement of a pluralistic realism would have been at best premature" (Wilson 1996: 312). In response, Dupré reasserts a point acknowledged at the outset by Oppenheim and Putnam and made repeatedly by disunifiers. It is an empirical question whether unity theses are viable which certainly remains open; it is, indeed, "hazardous to read a philosophical position off the current state of science" (Dupré 1996b: 441).[9] Certainly no one can claim to offer arguments that decisively settle the case for or against unity theses, given that these are prospective and, to some degree, normative, as well as empirical. Nevertheless, at this juncture the weight of evidence and argument counts strongly against any form of global, "singleness" theses. If anything, methodological and theoretical disunity seems to proliferate rather than diminish as the sciences mature and specialize. And, as often as not, this leads to a recognition of greater complexity rather than of simplicity in the ontology of the subject domains scientists investigate. The more we learn about the specifics of scientific inquiry, the more tenuous seems the rationale for taking any form of global unity thesis as the point of departure for philosophical analysis. The real challenge is to determine to what extent disunity prevails, in what different forms, and for what reasons.

Integration and Unification

By no means does this brief for taking disunity seriously displace all questions about unity in more contingent and localized senses. Unification remains an important ideal for many scientists (cf. Morrison 1995; Wayne 1996) and, as unifiers and disunifiers alike acknowledge, unifying connections within and between the sciences are a crucial feature of much research practice. Fine-grained studies of inter- and intra-field relations bring into focus a complex network of interdependencies--counterparts to the methodological, epistemic, and metaphysical unity postulated by traditional unity theses--that do not fit reductionist models but nonetheless bind the sciences together "by much more subtle routes" (Kincaid 1997: 6).

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9 Dupré is careful to say that he does not claim to have "refute[d] a priori the possibility that future scientific developments might make a monistic, even essentialist view of species increasingly attractive" (1996b: 441). See also Fodor:

> The question whether reductivism is too strong is finally an empirical question. (The world could turn out to be such that every natural kind corresponds to a physical natural kind, just as it could turn out to be such that the property is transported to a distance of less than three miles from the Eiffel Tower determines a natural kind in, say, hydrodynamics. It's just that, as things stand, it seems very unlikely that the world will turn out to be either of these ways.) (Emphasis in the original; 1974: 103).

Perhaps the most tangible evidence of such cross-field connections is in the examples of "interfield theories" analyzed in the late 1970s by Darden and Maull (1977), in the emergence of "cross-disciplinary research clusters" described by Bechtel (1988) and by Abrahmsen (1987), and in the expanded range of inter-field problem-solving strategies that have subsequently been identified by Darden (1991) and by Galison (1996). In the cases considered by these analyses, questions arise concerning aspects of the subject domain studied by one field that can only be addressed adequately by engaging the resources of another. The interaction between fields that this generates often results, not in a reductive assimilation of one field (or theory) to the other, or in a simple borrowing of information, technology, or explanatory models that leaves each essentially unchanged, but in the formation of substantially new theories and research programs concerned with relations between phenomena that cross-cut the traditional domains of neighboring fields (Darden and Maull 1977: 50). The cases that pose the most telling challenge to traditional unity theses are those originally described by Darden and Maull in which vertical links hold between interacting fields of just the sort that should support micro-reduction--their subject domains stand in a part:whole relation to one another--but what emerges is a semi-autonomous theory. Typically these concern aspects of the entities studied by one field that have not typically been its focal concern but are relevant for understanding the wholes studied by another field. In addition, Darden and Maull consider interfield theories that are formed to account for a range of other structural:functional and causal relations between phenomena at the same level of organization that are studied by distinct fields.[10] Bechtel, and Abrahmsen, add to this expanded account of interfield relations a consideration of instances of horizontal integration that result when a number of fields concerned with overlapping problems form loosely co-ordinated "disciplinary research clusters" (Bechtel 1988: 110; Abrahmsen 1978). In some cases practitioners in these clusters study relations that hold between distinct phenomena studied by different fields; in others they concern what are recognizably the same phenomena studied from different field-specific perspectives. But despite assuming some form of local ontological unity, what emerges are conjoint bodies of theory and research practice that are integrated to varying degrees but fall well short of content reduction.

The technology-induced emergence of trading zones recently discussed by Galison (1996) is a rather different but related phenomenon that arises from methodological rather than theoretical integration. Here the technology of computer simulation establishes unifying connections between fields, connections embodied in "strategies of practice" that depend on no presumption of ontological unity (however local) and yield no substantial theoretical integration, much less theory reduction (1996: 157). They do represent, however, "a new cluster of skills...a new mode of producing scientific knowledge that was rich enough to coordinate highly diverse subject matters" (1996: 119). As the pioneers and advocates of these computer applications refined their technical practice, they found themselves marginalized in their home fields and increasingly drawn into a delocalized trading zone (1996: 155); they

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10 The pattern Darden and Maull describe is one in which interfield explanatory problems arise by virtue of causal interaction or part:whole interdependence between the entities that comprise the distinct domains of two fields, or because two fields study the same phenomena from distinct points of view; for example, one may focus on structure and another on function (Darden and Maull 1977: 45), or on process as opposed to product, in the same domain (Abrahmsen 1987: 370). Darden and Maull describe situations in which existing background knowledge in one or both fields establishes, in advance, that they are dealing with phenomena studied by another field. An interfield theory arises when, in order to account for these cross-field connections, it becomes necessary to introduce substantially new ideas not derived from either contributing field or the background knowledge that establishes the link between them. As Bechtel describes the cases Darden and Maull consider, they arise in situations where interfield theories emerge to "fill...in missing information about a phenomena that was already partially understood in other fields" (1986: 45). He illustrates these points with an example in which the development of an interfield theory--one that links research on vitamins to research on metabolism--was triggered by the accidental discovery of domain-transgressive phenomena (1986: 45-46). Darden has since expanded this catalogue of interfield theories in connection with a general account of interfield relations that are instrumental to the formation of theories (1991).

developed a language[11] and a style of inquiry that took on a life of its own; it was this creole that gave rise to a reconceptualization of the subject domains of contiguous fields. Computer simulation technologies may have been introduced as a tool that could help diverse fields solve internally defined problems but, "bit by bit (byte by byte)...the computer came to stand...for nature itself" (1996: 157). What emerges is a body of practice that, like interfield theories, is not strictly the product of any one existing field; to varying degrees and in different ways it transformed and integrated the research of distinct disciplines, but did not generate an autonomous new field nor reduce any one existing field to another.[12]

When Darden and Maull first described interfield theories they were concerned that these had been ignored because the mandate set by Oppenheim and Putnam's working hypothesis focused philosophical attention on just one kind of interfield relationship, that of derivational micro-reductions. They proposed a new working hypothesis, one that conceptualizes unity in science as "a complex network of relationships between fields effected by interfield theories" (1977: 60). Even this remains too restrictive, however.[13] Bechtel's and Abrahmsen's research clusters represent a looser interfield co-ordination of theory, and Galison's trading zones, quite another (primarily methodological) interfield formation. In addition, there are innumerable other more mundane and "work-a-day" connections (Abrahmsen 1987:356; see also Darden 1991) that sustain durable networks of relationships between fields but they do not supplement the donor or recipient fields substantially enough to warrant the formation of an interfield theory or cross-disciplinary research cluster, and they are not contentious enough to generate a semi-autonomous trading zone. These are exchanges that proceed relatively quietly, establishing themselves as a stable and ubiquitous form of discipline bridging (Abrahmsen 1987: 356).[14]

At the relatively broad and transformational end of this spectrum of interactions, one field may appropriate the orienting theory or domain-defining metaphors and sometimes, with it, the problematic, of another field, but remain a theoretically and methodologically (as well as institutionally) autonomous endeavor. Psycholinguistics is an example considered in some detail by Abrahmsen (1987) in which the balance between influence and assimilation is renegotiated on an ongoing basis. The diffusion of structuralist approaches through the social sciences, described in another connection by Pettit (1975), is a case in which a linguistic metaphor and, selectively, some aspects of linguistic theory and linguistic methods of analysis, were extended to a wide range of fields dealing with cultural subjects that could reasonably be conceived as meaning bearing in various senses. Archaeology is a field whose recent history has been shaped by a succession of experiments with different metaphoric and theoretical constructions of its cultural-material subject domain: a reductive eco-materialism that privileges the environmental determinants of cultural behavior; various forms of historical materialism, structuralism, and post-structuralism; and, recently, a family of evolutionist approaches on which cultural phenomena

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11 Galison argues that the formation of a distinct technical language, an interfield pidgin that became a creole (1996: 153), was instrumental in setting simulation researchers apart from colleagues in their home fields and in creating the heterogenous domain Galison refers to as a trading zone.
12 It is a contingent matter whether, in fact, these interfield theories or technical trading zones will crystallize into a distinct new field. This may be the ultimate outcome but, Bechtel argues, the degree of autonomy that is realized by new research initiatives depends on such factors as the integrity of the inter-field phenomena under study and the ability of inter-field researchers to maintain ties with their home disciplines (Bechtel 1986: 37).
13 As indicated earlier, Darden has subsequently broadened her account of the relations that bind (and divide) research fields (Darden 1991).
14 The following catalogue of interfield connections is based on Abrahmsen (1987), Darden (1991), Bechtel (1988, chapters 5 and 6), and the introduction by Bechtel as well as contributions to Integrating Scientific Disciplines (edited by Bechtel, 1986). It is also informed by Kincaid's more general analysis (1997) and by Baigrie and Hattiangadi's discussion of consensus and stability in science (1992), and Hacking's account of self-stabilizing, consilient networks of "ideas, things, and marks" (1992a: 44).

are conceived as part of the extended human phenotype, to be explained in terms of selection pressures. In addition, however, borrowings of more limited scope are essential even in fields with less permeable boundaries whose subject domains and problematics are distinctly their own. These include the transfer of explanatory models, empirical results, and research technologies (skills and instruments) from one field to another where they are used to develop field-specific explanatory theories and to establish the evidential basis necessary for evaluating these theories.

Dupré considers these elements of the "densely connected network" that binds various sciences together, but dismisses them as irrelevant to the debate about unity; they establish nothing more than that "no form of knowledge production can be entirely isolated from all the others" and this, he says, is "too banal an observation to glorify with the title 'unity of science'" (1993: 227). He is certainly right that such interfield connections provide little support for the kind of global, "singleness" unity thesis he contests.[15] But if Dupré's critique of these theses is taken as a point of departure, such "banal" interactions are crucial for understanding the relationships that productively integrate and coordinate the actual practice of science. These low-level, unexceptional connections often involve just the kind of paradoxical juxtaposition, indeed, interdependence, of unity and disunity on which Morrison remarks; they preserve disunities in many areas while at the same time building localized bridges, trading zones, and points of integration between fields. In the case I consider below, historical archaeologists make use of integrative connections between fields to establish an evidential basis for building and testing claims about the past, but the epistemic advantage this affords depends on their ability to systematically exploit the disunities that persist on many levels among scientific fields and theories.

Localized Unity: Historical Archaeology

Conjoint uses of evidence

Historical archaeology has emerged as a distinct field only in the last thirty years.[16] In North American contexts its proponents have struggled vociferously to establish its credibility and define its identity in opposition to two powerful parental disciplines: real archaeology and real history. Prehistoric archaeologists have been inclined to treat historical archaeology as shallow, literally and figuratively, and historians dismiss it as a hopelessly thin source of insight about the past. Historical archaeologists, for their part, insist that much damage has been done by arrogant prehistorians who, enlisted by contract firms, government agencies, and university field schools, assume that historic sites pose no interesting challenge of their own, and by insular historians who insist that there is nothing to be learned from kitchen middens and cellar pits that cannot be better learned from the documentary record. It is striking, however, that as intent as historical archaeologists have been on defining the boundaries of their new field, they consistently emphasize the need for, and value of, substantial interfield connections. A recurrent theme in

15 Dupré cites Darden and Maull in this connection, arguing that their examples of interfield theories do not establish grounds for endorsing any very strong unity thesis. In fact, Darden and Maull's account of interfield theories was specifically intended as a counter-example to Oppenheim and Putnam's working hypothesis; even in cases where neighboring fields deal with just the kinds of boundary-straddling, part:whole relations that are most amenable to interfield reduction, reduction is often not realized.

16 See, for example, Schuyler's discussion in the preface to the first comprehensive reader in historical archaeology, Historical Archaeology: A Guide to Substantive and Theoretical Contributions (1978a: ix, 1978b). He argues that historical archaeology--"an entirely new area of scholarly research and public concern"--took shape after World War II, but notes that the Society for Historical Archaeology was not formed, and did not begin publishing Historical Archaeology, until 1967. Schuyler describes parallel developments in the U.K. and in Australia, although the disciplinary affiliations are different in these contexts from those in North America (e.g., industrial archaeology and the archaeology of Roman Britain, in the case of historical archaeology in the U.K.). As he notes, much of the impetus for the development of this fledgling field in North America came from federal and state or provincial programs of historic site preservation and interpretation.

these debates is an insistence that, where historic period events and conditions of life are concerned, vastly more can be achieved by making conjoint use of the evidential, methodological, and theoretical resources of archaeology and documentary history than can be achieved by either field working in isolation from the other.[17]

This is not just an argument that archaeological inquiry provides supplementary detail about the past, useful for animating museum displays but of only marginal relevance to the bigger picture historians construct on the basis of documentary research. In resisting the imperialism of history, historical archaeologists sometimes insist that they offer substantially different, potentially transformative insights about the recent past. The gritty details of the archaeological record bear witness to "the inarticulate" (Ascher 1974: 11), the "endless silent majority who did not leave us written projections of their minds" (Glassie 1977: 29), whose dispossession extended well beyond the alienation of their labor to the production of what Glassie describes as "superficial and elitist...tale[s] of viciousness"--"myth[s] for the contemporary power structure" (1977:29). Historical archaeology promises not just to fill in missing information about those who are largely invisible in the narratives of text-based history, but to counter the "inevitable elitism" (1977:29) of traditional history. While this sells short the insights afforded by radical history (e.g., "history from below"; Sharpe 1991), and ignores the conservatism of much historical archaeology, it does draw attention to the transformative potential of the field, a potential that has been realized in a number of areas in which historical archaeologists have been active the last thirty years.[18]

Sometimes these claims about the corrective powers of historical archaeology are generalized in epistemologically interesting ways. The discipline-bridging position of the new field is represented as a resource rather than a liability because the credibility of claims about the historical past is improved to the extent that they are supported by both documentary and archaeological evidence. Here historical archaeologists appropriate arguments that are central to a long-running debate in North American archaeology about the status of archaeological evidence: claims about the evidential significance of material identified as a record of the past is always mediated by ladening theory (linking principles, auxiliary assumptions, background knowledge); they are always and inevitably an interpretive construct. At one extreme uncompromising anti-positivists argue, on this basis, that any appeal to archaeological evidence is inescapably and viciously circular, while at the other, unreconstructed positivists take refuge in the conviction that certain ranges of auxiliaries, usually those established by the most successful of the physical sciences, can secure a surrogate foundation of stable (if not given) evidence.[19]

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17 There is a parallel here with cases described by Bechtel who argues, with reference to psychology and neuroscience, physiology and chemistry, that an insistence on strict disciplinary autonomy can be as counterproductive as reductive unification schemes (1988, pp. 79-81), and by Darden who shows how important a role interfield interactions play in the creative development of science (1991).

18See the discussion, in Chapter 14, of recent developments in the archaeology of slavery, European:Native American interactions in the contact period, the changing roles and activities of women in the historic period, and the history of colonial oppression (e.g., contributions to Schmidt and Patterson 1995, Sued-Badillo 1992) and of capitalist systems (Leone and Potter 1999).

19If a self-vindicating foundation cannot be found, one vindicated by physics is close enough (see Chapter 7). This is sometimes the tenor of Binford's defensive responses to his post-processual critics (Binford 1989). See also recent debates about the merits and limitations of evolutionary archaeology (e.g., Schiffer 1996: 650). In both cases the identification of a quasi-foundation is the opening move in an argument to restrict the scope of archaeological inquiry to those aspects of the cultural past that can be investigated using just the kinds of evidence that can be considered secure beyond reasonable doubt.

More typically, however, archaeologists embrace a range of mediating positions.[20] They do what they can to assess, or establish, the relative security of the sources on which they rely. But beyond this, they exploit the fact that strong constructivist arguments presuppose a degree of unity in science that simply does not exist. Circularity is an inescapable problem only if one assumes a seamless integration of all the various fields and theories on which archaeologists rely when constructing models of the past and when interpreting their data as evidence for or against these models.[21] In practice, when archaeologists exploit the dimensions of epistemic independence described in previous chapters (Chapters 12, 13, and 14), they make good use of disunities that (contingently) ensure a disjunction between background assumptions drawn from different sources and the hypotheses they test against evidence interpreted in light of these assumptions. Consider, for example, the possibility that an archaeologist might use radiocarbon dating and various types of materials analysis to test the plausibility of an hypothesis about trade connections, perhaps an hypothesis inspired by structuralist analysis of the grammar of design traditions evident in the burial goods, elite ceramics, and architecture of two distant and otherwise distinct prehistoric communities.[22] The test in question would be a matter of establishing whether the material thought to have been traded into one context could have originated in the other (e.g., whether it is contemporaneous and whether it is made of materials or by means of technologies possessed by the source culture). In such a case, there is sufficient disjunction between the linguistics and socio-cultural anthropology from which assumptions framing the test hypothesis are drawn, and the chemistry and physics necessary to establish the source and dates of the archaeological material, that the resulting evidence could not be expected to converge in support of the structuralist hypothesis about trade relations linking the cultural traditions; its convergence is not plausibly an artifact of the interpretive principles used to bring archaeological data to bear on the test hypothesis.[23] In this spirit, Leone and Potter argue that if we "abandon the conceit that the documentary record was created for us," and the underlying premise that interpretation of the archaeological record is dependent on the documentary record, it becomes possible to exploit these records as "two *independent* sources of evidence" (1988: 14). A process of "analytical byplay" between documentary and archaeological data, of working "back and forth, from one to the other," suggests that each can be used "to extend the meaning of the other" (1988: 14). The crucial methodological corollary is that if two sources are indeed independent, then a failure to converge can be counted on to expose weakness in the constituent chains of reasoning that may not be evident when the security of each is considered on its own; each line of evidence can be used as a check on the other.

20 See Kosso (1992) and VanPool and VanPool (1999), as well as Chapter 12, for parallel accounts of the similarities in research strategy that underlie the sharply drawn opposition between these archaeological positions.
21 It is sometimes suggested that if unificationist ideals were realized, the resulting system of scientific knowledge could not be subjected to systematic empirical evaluation: "the success of a Grand Unified Theory in contemporary physics would make science untestable (or only circularly testable)" (Stump 1991: 468). If, however, the broad outlines of Glymour's bootstrapping model of confirmation captures the practice typical of even a few cases (i.e., one need not embrace Glymour's more expansive claims), this consequence may not follow; see Chapter 13 for an account of this theory in application to archaeology.
22 I construct this hypothetical example using the examples of physical dating and materials analysis that appears most often, in this connection, in Binford's discussions of epistemic independence, conceived here as two components of the evidence relevant to an archaeological problem like that posed by Allchin's dilemma (as discussed in Chapter 4; Binford 1968a: 18).
23 To put this point in more general terms: the credibility of an hypothesis is enhanced much beyond a simple additive function insofar as it is implausible that multiple lines of support could arise accidentally or as a result of compensating errors that compromise each line of evidence.

Causal, Inferential, and Disciplinary Independence

Although I am sympathetic to these claims on behalf of historical archaeology, they conflate several different senses of independence between lines of evidence not all of which are epistemically relevant or coincident with the disciplinary boundaries between history and archaeology. There are at least three kinds of (horizontal) independence at issue here: causal, inferential, and disciplinary independence. I disentangle them initially with reference to the examples of microscope development and use that Hacking considers (1983, chapter 11), and that I discussed in chapter 12 (see also Wylie 1999a); the parallels with archaeological practice are instructive, particularly if it is conceptualized as a matter of “indirect observation” of the cultural past (e.g., Fritz 1972). On Hacking’s account, the makers and users of microscopes exploit a number of different physical (causal) processes of interaction (or, signal production) between the target and the receiving instrument. The great value of the proliferation of microscopes that exploit these different causal processes (e.g., acoustic as opposed to optical microscopes) is that they allow for a triangulation of signals, correcting and enhancing the information any one microscope could provide about the entities they allow us to observe: "we believe what we see [through microscopes] largely because quite different physical systems provide the same picture" (1983: xiii). Triangulation depends on crucial unities between fields and domains, but the judgement that some lines of evidence are (horizontally) independent in an epistemically relevant sense depends on causal and theoretical disunities.

The first and most obvious dimension of independence in Hacking’s examples is that which distinguishes the different physical systems--different causal processes or mechanisms--that produce the traces detectable by different kinds of microscope. It has to be assumed that these causal pathways all emanate from, or interact in the production of, an ontologically unified subject: the entity or events that the microscope is meant to detect. At the same time, however, triangulation depends on the plausibility of the assumption that these different trace-generating systems are causally independent in the sense that they do not interact in such a way as to ensure an artificial congruence in the signals they transmit.

Independence in a second sense holds between the bodies of background knowledge, the auxiliaries, that are deployed in inferentially reconstructing the pathways by which signals are transmitted and received. The transfer to one field of empirical or theoretical results established in another--the basis for constructing any one line of evidence--depends on a limited assumption of ontological unity and of theoretical congruity between the source and target fields. That is, it is assumed that the causal processes exploited by microscopes are relevantly the same in the (export destination) contexts where they support mediated observation as in the source contexts where these causal processes are a primary object of study. Under these conditions the knowledge developed of these causal systems by one science can serve as the basis for auxiliaries in another. But if horizontal independence is to be established, it must also be assumed that the background knowledge concerning causally distinct processes is epistemically independent. Here the crucial forms of inferential independence are those which ensure that a coincidence of images produced by different instruments is not an artifact of the instruments themselves or of the auxiliaries that inform our interpretation of the traces they allow us to detect. Triangulation thus depends on theoretical disunities between the different ranges of auxiliaries on which microscopists rely to make observations of the same entity or process; these include disunities of content, domain defining presuppositions, and traditions of research practice that mitigate against an arbitrary congruence between lines of evidence constructed using different instruments.

One indication of such inferential (epistemic) independence may be the fact that the background theories on which microscope makers rely have been developed by institutionally distinct disciplines. This disciplinary disunity is a third sense of independence that figures in archaeological contexts, with particular prominence in historical archaeology. Although these three senses of independence--causal, theoretical, and disciplinary--are often treated as one, it cannot be assumed that they will coincide. The same signal transmission process might be detected, or interpreted, using very different bodies of background theory but may, nonetheless, carry the same distortion through different channels. Alternatively, bodies of background theory that are drawn from different disciplines and that seem distinct in content may share enough in the way of common assumptions--perhaps a consequence of the kinds of trade between fields described by Darden, Bechtel, Abrahmsen and others--that persistent, compensating errors arise in the detection and interpretation of signals even when they are generated by causally independent processes and interpreted using apparently distinct bodies of theory.

The kind of (horizontal) independence that archaeologists invoke is assumed, in the ideal, to incorporate all three of these dimensions of independence: causal independence is assumed to be aligned with an independence in the content of background theory that is marked, in turn, by the distinctness of its disciplinary sources. The most obvious archaeological examples of this ideal are cases in which different methods of physical dating are applied to material from a single archaeological context. Consider the use of tree ring counts and measures of radio-carbon decay, magnetic orientation, and the internal evolution of stylistic traditions to determine (respectively) absolute cutting, burning, and deposition dates, and tradition-specific production dates. The disciplines that supply the relevant technologies of detection are certainly institutionally autonomous, and the content of their theories is substantially independent; it is unlikely that the assumptions that might produce error in the reconstruction of a date using principles from physics will be the same as those that might bias a date based on background knowledge from botany or socio-cultural studies of stylistic change.[24] Finally, this independence in the content of the auxiliaries and in their disciplinary origins is especially compelling because it is assumed to reflect a genuine causal independence between the chemical, biological, and social processes that generated and transmitted the distinct kinds of material trace exploited by different dating techniques.

The case of historical archaeology makes clear, however, just how complex and uncertain the argument for epistemically significant independence between textual and archaeological sources can be. In some respects and in some cases the archaeological record can reasonably be assumed to be independent of the documentary record in all the senses described here. It may be entirely plausible that the contents of trash pits and various kinds of official documentary history are produced by such different means and for such different purposes that they can be regarded as causally independent, even though they derive from (and therefore serve as evidence of) the same community or set of historical events. Moreover, to make effective use of such different kinds of material as a record of the (same) past it may be necessary to rely on interpretive techniques and bodies of background knowledge that derive from distinct research traditions and depend on fundamentally different skills and presuppositions, specifically, those necessary for the interpretation of documentary records as opposed to the analysis of material culture.[25] In such cases historical and archaeological lines of evidence may be expected to provide a check on one another: the disunity of their sources confers epistemic advantage on their conjoint use.

In many cases, however, these assumptions of independence cannot be made, and none can be assumed to entail the others. The disposal of trash may reflect the same principles of decorum as writing

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24 It is important to note, however, that the calibration of C14 dates depended on tree-ring and design sequence dating. As Renfrew describes the debate that informed the calibration of C14, one of the questions raised concerned the independence of the long bristlecone pine tree ring sequences, given their high altitude, from variations in the rates of breakdown of C14 that they were being used to calibrate (1973b: 89-90). If these worries had been born out, the strategy of relying on one line of evidence to correct another would have been confounded unless (as was the case) it was possible to counter the threat of compensating error by using still other dating techniques (e.g., archaeomagnetism, varve and other methods of geological dating) to cross-check and refine the accuracy of the calibration curves.

25 For an account of the differences between these approaches as they arise within archaeology see Patrick (1985).

for the public record, and both lines of evidence may systematically obscure precisely the underlying contradictions that are reflected in the silences of elitist history that historical archaeologists mean to correct. Indeed, there may be greater causal independence between different types of documentary record, for example, between legal statutes and personal diaries, than between certain kinds of archaeological and documentary record: public architecture and speeches made by the heads of state, for example. In addition, however resolute archaeologists and historians have been in maintaining the boundaries between their disciplines, they are almost certainly subject to many common influences and often rely on similar interpretive resources; they are affected by a range of bridging and integrating forces that persistently undermine the institutional disunities they guard so jealously. For example, there is no reason to believe that the politics structuring debate about how to mark the Quincentennial would have had a fundamentally different impact on historians, as opposed to archaeologists, who study the operations of various colonial powers in the Americas (see, e.g., Trouillot 1995: Chapter 4). Similarly, it is implausible that the systematically distorting romanticism about First Nations cultures critiqued by Trigger (1991) would have shaped the archaeological interpretations he considers but not the accounts developed by historians of the dynamics of contact. Historians and archaeologists often interpret the different records with which they deal in strikingly similar ways, consequently, they may consistently overlook or misinterpret aspects of their subject that are incongruous (unpalatable or unrecognizable) given a common stock of background assumptions. The emergence of closely parallel feminist critiques in both fields makes it clear that the practice of deploying different kinds of evidence, even in conjunction with one another, is not in itself proof against pervasive androcentrism or sexism. Appearances of disciplinary and theoretical disunity may be deceiving.

Questions about the conceptual, causal, and disciplinary independence of distinct lines of evidence must be treated as empirically open and assessed on a case by case basis. Disciplinary boundaries may not cut the world at its joints where different orders of causal production are concerned, and may not insulate neighboring disciplines from the influence of assumptions that are capable of inducing compensatory errors in seemingly independent lines of evidence. It follows that to determine epistemically relevant independence, two lines of inquiry are necessary: one to establish the extent to which the processes responsible for ostensibly different records are, in fact, <u>causally independent</u> of one another; and another to determine the extent to which the background theories concerning these processes--the interpretive principles used to read these records--are <u>conceptually independent</u>. While questions of causal independence can be addressed only by first order empirical research, questions of conceptual independence require a program of second order, meta-scientific investigation that is both philosophical and empirical (specifically, sociological and historical); confounding presuppositions that are deeply embedded in disciplinary traditions may come to light only through systematic study of the various kinds and degrees of interaction that bind apparently distinct fields together. Whenever archaeologists assess the transferability and the (likely) independence of the auxiliaries they borrow they make judgements about the reach across disciplinary boundaries of cross-cutting interests, shared assumptions, and common theoretical models and methodologies. If these judgements are to bear this epistemic weight, they must be grounded in a detailed understanding of the diverse patterns of integration, trade, co-ordination, and differentiation that both unify and fragment the scientific enterprise.

Meta-philosophical Implications

Although philosophers and colleagues in neighboring fields of science studies have largely abandoned global unity theses *about* the sciences, meta-versions of these theses often underpin our own practice. The relations between history, philosophy, and sociology of science continue to be structured by claims that privilege a particular methodology as the only way properly to study science. Whether the approach in question is that of exact philosophy or informal conceptual analysis, technical or social history, sociometrics or ethnography, the assumption lying just below the surface is often that, as a subject for investigation, science falls within the ambit of a specific discipline and the methodology of that discipline is uniquely appropriate to its study. To be sure, this confident imperialism has been sharply contested in recent years. A number of sociologists now urge a strategy of "alternation" between diverse standpoints and methods for studying science, while philosophers have long negotiated an uneasy alliance with historians of science and some are now intent on socializing and humanizing, as well as naturalizing, the philosophical study of science. My claim is, however, that if unity theses are called into question as the working hypothesis that frames science studies, two meta-philosophical consequences follow that require a substantial extension of these initiatives.

First, the working hypothesis that frames our research must be redefined. We must finally set aside the polarized options defined by debate over global unity and disunity theses; neither is tenable and both obscure important features of research practice. Although unity cannot be presupposed, the scientific disciplines are unevenly and contingently interdependent in any number of ways that are crucial to their practice and success as a family of enterprises. If we are to understand the sciences, we must attend to the diverse networks of interaction responsible both for the proliferation and for the integration of distinct bodies of theory and research traditions. This serves not just a philosophical interest but, more specifically, a normative and practical concern to clarify concepts, such as that of evidential independence, which are methodologically central to the practice of science in a great many contexts.

Reorientation along these lines requires, second, a commitment to methodological pluralism at a meta-level that substantially undermines the boundaries that persist between various fields of science studies. As the case of historical archaeology makes clear, epistemically salient notions of evidential independence cannot be explicated in strictly philosophical terms. Some kinds and degrees of inter-field integration are necessary conditions for the effective transfer of expertise and theory between fields. At the same time, the epistemic significance of appeals to diverse (horizontally independent) lines of evidence depends on the persistence of substantial ontological, epistemic, and institutional disunities between the sciences. To determine whether epistemically relevant independence holds in any particular case requires close examination, not just of conceptual connections that may hold between fields, but also of the histories of discipline formation and the social, institutional dynamics that bind these fields (uneasily) together.[26]

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26 In presentation of these ideas to an archaeological audience (Wylie 2000a), I drew the following conclusion:

> In conclusion, I urge a skeptical attitude toward claims about the scientific status of archaeological practices that depend on appeals to unifying features of science. It is important to think systematically, even globally, about the ways in which archaeological inquiry is embedded in an extended network of integrating and fragmenting relationships with other fields of inquiry (by no means all or only scientific fields). But where arguments of justification are concerned, it is crucial to act locally; it is the details of interfield relations that count in assessing epistemic independence, not the affiliation of a particular line of inquiry or method or set of auxiliaries with a corporate entity we valorize as science. The alternative to this admittedly uncertain and defeasible strategy of argument is not the security of self-warranting foundations and logical necessity. It is a dogmatic narrowing of horizons that is profoundly divisive and that undermines the one Enlightenment ideal that survives scrutiny: that of holding practice, as well as belief, open to revision in light of experience. (Wylie 2000a: 234)