UNIFICATION AND CONVERGENCE IN ARCHAEOLOGICAL EXPLANATION

Alison Wylie

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Introduction

In the mid-1990s something of a watershed was reached in philosophical theorizing about explanation. While questions about explanation have always been central to philosophy of science, with the widely touted demise of positivism they assumed the status of paradigm disrupting anomalies, and since the early 1970s a number of widely divergent approaches to understanding explanation have been continuously in play. After 1988 there appeared a spate of syntheses, overviews, and collections in which some of the central contributors, most visibly Salmon and Kitcher, undertook to bring order to this proliferation of positions. The upshot is a tripartite categorization of philosophical theories about explanation: epistemic, ontic, and erotetic.[i]

Epistemic theories of explanation offer a top-down account of explanation according to which explanations are distinguished by the way they organize what we know about the world, not any specific content or type of claim about the world. These include the original Hempel-Oppenheim (deductive-nomological) covering law models of explanation, the statistical and inductive variants of these models that were formulated through the 1970s, and information-theoretic accounts. They also include the unificationist models originally proposed by Friedman in 1974 and by Kitcher in 1976. On this account explanation is conceptualized as a function of the systematizing power of theory, although not mediated by a particular argument structure: a theory is explanatory when it "effects a significant unification in what we have to accept" (Friedman 1974: 14): "science increases our understanding of the world by reducing the number of independent phenomena we have to accept as ultimate or given" rendering the world more "comprehensible" (Friedman 1974: 14-15; see also Kitcher 1989: 432, and 1981). Where Friedman's account ran into difficulties differentiating the units of basic or "brute" phenomena that are more or less successfully unified, Kitcher has moved to an "argument pattern" account of explanatory unification. He describes explanation as increasing scientific understanding "by showing how to derive descriptions of many phenomena using the same patterns of derivation again and again" (1989: 432). The central intuition here is that successful explanations allow the generation of as many conclusions as possible from as few premises as possible.[ii]

By contrast to epistemic theories, ontic accounts of explanation represent a bottom-up approach; explanations are characterized in terms of their content. It may be required, for example, that they be grounded in an understanding of causal or other relations of dependence that obtain in the external world.[iii] On the causalist account that Salmon has advocated since the late 1970s,[iv] explanations are understood to "reveal the mechanisms, causal or otherwise, that produce the facts we are trying to explain" (Salmon 1989: 121): "to explain is to expose the inner workings, to lay bare the hidden mechanisms, to open the black boxes nature presents to us" (Salmon 1989: 134). Given this, Salmon insists that what counts as an explanation "depends on the kinds of mechanisms--causal or noncausal--that are [actually] operative in our world" (1989: 149-150), and cannot be settled a priori. On some ontic theories explanation may be grounded in an understanding of "worldly relations other than causation" (Ruben 1993: 12), for example, various forms of structural dependence and determination, identity, supervenience, event (and entity) composition which "give significant structure to the world of events" (Kim 1974: 52; 1993), but are not strictly causal.

Pragmatic or erotetic theories of explanations are a third family of theories that characterize

explanations, not by appeal to any specific feature of content or form but, rather, as answers to why-questions; explanations are accounts that satisfy the curiosity or puzzlement of a particular inquirer under given circumstances. As part of his program of formulating a viable (constructive) empiricism, at the turn of the 1980s van Fraassen argued a deflationary view of explanation: he reaffirmed the empiricist thesis that the systematization of observables, not the explanatory modeling of causes, is the primary aim of science and argued that what counts as an explanation is a function of the pragmatic circumstances of question-asking constrained only by the requirement that the content of answers given be scientifically acceptable (van Fraassen 1980; see also Lloyd and Anderson 1993). Others pursue the projects of distinguishing different types of explanation-eliciting questions and elaborating a fine-grained account of the pragmatics of answer-giving (e.g., Bromberger 1966; Garfinkel 1981). In the interest of reconciling subjective and objective accounts of explanation, Railton (1981, 1989) proposes a distinction between an "ideal explanatory text" and "explanatory information." The ideal explanatory text constitutes the framework of complete, ideal understanding within which choices may be made to foreground different selections of explanatory information depending on the circumstances under which an explanatory question is raised (see also Salmon's discussion of Railton, 1989).

In addition to normalizing debate, there seems to be general agreement, at least among the synthesizers, that the leading contenders among the theories of explanation on offer are particular versions of ontic and epistemic theories: viz., Salmon's causalist theory and Kitcher's unificationism. While Kitcher holds that causalist theories depend on metaphysically contentious claims about causal processes which are best understood in terms of the unifying power of our schemas--"objective dependencies among phenomena are all generated from our efforts at organization" (1993: 172)--Salmon suggests that there may be room for rapprochement, building on Railton's proposals. Perhaps unificationist and causalist accounts represent different but compatible strategies for understanding "the same facts," while pragmatic approaches "determine which way of 'reading' is appropriate in any given explanatory context" (Salmon 1989: 185).

I will argue that, while a healthy pluralism is desirable, especially given the diversity of explanatory practices that is typical of the sciences not to mention ordinary life, Salmon's conciliatory move may be premature. My thesis is that the powers of unification central to Kitcher's account are dependent upon the understanding of underlying mechanisms, dispositions, constitutions, and dependencies central to explanation on a causalist account. This case can be made through analysis of Kitcher's account of the conditions under which apparent improvements in unifying power may be judged spurious. But to clarify what is at issue here I will consider, in some detail, an archaeological case in which debate about the merits of an ambitious and highly controversial explanatory account has unfolded along lines defined by precisely the intuitions that divide Salmon and Kitcher. Here the credibility of a powerfully unifying argument pattern--whether or not it should be accepted as a plausible explanation--depends fundamentally on the plausibility of its claims about the conditions actually responsible for the explanandum and not on an elaboration of its unificationist virtues. I first describe this case, and then consider its implications for the newly normalized philosophical debate about explanation.

Renfrew's Grand Synthesis

One of the most ambitious and perplexing explanatory theories currently under discussion in archaeology is an account of contemporary linguistic diversity advanced by Renfrew in the late 1980s: a subsistence driven demic-diffusion model of the long-term, large-scale cultural processes that he believes must explain the existence and distribution of linguistic macrofamilies. As originally developed in Archaeology and Language: The Puzzle of Indo-European Origins (1987), Renfrew's focus was the long-standing problem of explaining the "remarkable relations that link nearly all the European languages, many of the languages spoken in India and Pakistan, and some of those in the lands between" (1989b: 106).[v] His thesis was that these widely distributed linguistic affinities should be explained as a consequence of the Neolithic revolution. As agricultural subsistence technologies diffused across Europe in the early Neolithic (approximately 8000 B.P.), the populations using these technologies carried with them a common stem language, proto-Indo-European, which inexorably displaced the diverse local languages of existing foraging societies. Renfrew describes this process as one of subsistence driven demic-diffusion because, on his account, the mechanism responsible for the linguistic diffusion of proto--

Indo-European were demographic pressure operating on the expanding population of agriculturalists, reinforced by what he describes as the inherent superiority of agricultural technologies. Renfrew has since argued that processes of demic-diffusion may prove capable of explaining much of the confusing pattern of distribution of languages in the contemporary world as a whole (1992a: 12), and may be supported by emerging patterns of genetic affinity among human populations. In both local (Indo-European) and global form, the demic-diffusion model is to be recommended, on Renfrew's account, because it holds out the promise of a "remarkable potential synthesis between archaeology and historical linguistics [and]...an emerging discipline which we might call 'historical genetics'" (1992b: 445-446).

To argue the case for the demic-diffusion model, Renfrew develops a typology of the cultural processes by which a language may come to be spoken in a region and rejects the main alternatives to the demic-diffusion hypothesis of linguistic replacement that he prefers. He argues that the simplest model, that of initial colonization and continuous (local) linguistic development, is patently implausible. The first populations to enter Europe would have introduced a common stem language much too early to account for contemporary linguistic affinities (between 35,000 and 12,000 B.P.). Processes of linguistic divergence (analogous to genetic drift) and of linguistic convergence (comparable to gene flow) would have generated a much more highly fragmented, locally diverse linguistic picture if they had operated continuously since initial colonization. Renfrew thus concludes that some intervening episode of linguistic recolonization must have occurred, introducing a proximate stem language to the region recently enough that contemporary Indo-European languages would still bear the marks of a common origin.

Having thus eliminated the explanatory models that posit initial colonization and continuous development, Renfrew's chief concern is to demonstrate that demic-diffusion is the most plausible of the linguistic replacement models available. In particular he is intent on establishing the inadequacy of a widely accepted alternative according to which proto-Indo-European was introduced to the region in which Indo-European languages are now spoken by a Kurdic invasion: it was carried into the region by mounted warriors emanating from north of the Black Sea (western Russia) "somewhere between the late Neolithic period and the beginning of the Bronze Age," some 5000 to 6000 years ago (Renfrew 1989b: 108). On Renfrew's topology this is a type of elite dominance model. As a family, such models postulate a process of linguistic replacement by which a relatively small, well organized external force displaces an internal elite and imposes its language on the local population. While the Kurdic invasion hypothesis fits the time frame for linguistic replacement required by standard linguistic reconstructions of proto-Indo-European, Renfrew insists that such a model is unsustainable both conceptually and empirically.

Although elite dominance models vary considerably in what they postulate by way of a homeland, the trajectory of the invasion or migration, and the mechanisms responsible for population displacement and consequent linguistic replacement, all are flawed, Renfrew argues, by a shared assumption that something like our contemporary "separation of the non-urban world into distinct ethne" can be projected thousands of years into prehistory. All assume a static three-way identification between linguistic communities, social units (ethnic identities and/or populations), and archaeological cultures. Renfrew objects that "a strongly developed ethnicity is not, in fact, a universal among human societies" (1988: 438), and was not likely to have obtained 5000 to 6000 years ago.[vi] Moreover, the Kurdic invasion hypothesis makes specific assumptions about the technology and social organization of the invading population that are "a travesty of archaeological interpretation" (Renfrew 1988: 438). The military advantage of the Kurgan warriors remains hypothetical. The model offers no plausible account for why "hordes of mounted warriors [would] have moved west at the end of the Neolithic, subjugating the inhabitants of Europe and imposing the proto-Indo-European language on them" (Renfrew 1989b 110). And there is no evidence that the societies of either the invaders or the populations invaded were centrally organized or socially stratified in ways Renfrew considers a necessary condition for the sort of conquest that could have brought about wholesale linguistic replacement (Renfrew 1989b: 110). In short, there is scant evidence that the conditions necessary for an episode of elite dominance could have obtained in the period in question, even if there was large scale movement of population.

Given the inadequacy of these competitors, Renfrew argues the case for considering some form of demic-diffusion (or, demography-subsistence) model according to which a large number of people bearing the required stem language diffuse slowly into a given territory and displace the old population (and its

languages), not by force of arms, but through the introduction of a "new exploitative technology" (Renfrew 1988: 439) which confers on the incoming population a decisive adaptive advantage. In Europe, Renfrew observes, the Neolithic revolution represents just such a process: "if one surveys European prehistory there is an event wide-ranging and radical enough in its effect to be a candidate, and that event does indeed fall squarely into the subsistence category: the coming of farming" (Renfrew 1989b: 110). The effect of this bold conjecture is to push the requisite episode of linguistic recolonization much further back into prehistory than historical linguists had considered plausible.[vii] On Renfrew's account it should be understood as a consequence of the Neolithic transition which occurred 8000 years ago (6000-6500 B.C.), some 3000 years earlier than the appearance of the Kurdic invaders who, on the main rival explanation, spread proto-Indo-European from the northern steppes in the transition from the Neolithic to the Bronze Age.

The specifics of Renfrew's demic-diffusion model are adapted from an influential account of the Neolithic revolution published by Ammerman and Cavalli-Sforza (1973, 1979), and later elaborated by Cavalli-Sforza in much more ambitious and controversial terms (e.g., 1991; see also Cavalli-Sforza et al. 1988, 1990; Ross 1991; and critical discussion by Bateman et al. 1990). Ammerman and Cavalli-Sforza proposed that the wheat and barley, goat and sheep agricultural complex, traced back to central Anatolia (where the prototypes of the domesticates later found throughout Europe existed in the wild), was carried into Europe in the 7th millennium B.C. by relatively small, incremental movements of farmers and their offspring. A crucial feature of this model is the assumption that the population density that farming could support had the potential to increase that associated with a foraging economy by as much as a factor of fifty. Ammerman and Cavalli-Sforza estimate that this population pressure would have forced each generation of farmers to seek new territory at a rate of approximately 1 km a year (18 km in any direction per generation, where generations are estimated at 25 years each). On this "wave-of-advance" model, Renfrew argues, farming would have been carried across Europe in about 1500 years--approximately the time frame suggested by archaeological evidence (Renfrew 1989b: 111).[viii] The inexorable nature of this advance is due to population pressure (the demographic component of the model), and to the adaptive advantage that agricultural subsistence practices and technology would have given the incoming population (the subsistence component). What Renfrew adds is that this slowly, steadily diffusing population of farmers carried with them, not just agricultural technology, but also their language, and that this language displaced other local sister languages to become the common linguistic foundation out of which contemporary Indo-European languages emerged by processes of local divergence from other another. Secondary processes--processes of linguistic replacement or convergence due to later episodes of elite-dominance and ongoing contact through trading links and proximity--would then have redistributed these languages and established an overlay of later commonalities in lexicon and structure.

On the original formulation of the "wave of advance" of advance model, Ammerman and Cavalli-Sforza appealed not just to convergent patterns in the archaeological and linguistic evidence but also to congruencies with the distribution of gene frequencies in European populations, specifically in the distribution of blood types and antigens. They make much of the fact that, for example, the frequency of the Rhesus negative factor is significantly higher among the Basque population, a linguistic isolate, than the surrounding European population, and that other genetic affinities correspond to linguistic affinities; they find in these data crucial support for the hypothesis that the Neolithization of Europe involved population diffusion and replacement. Although this is an aspect of Ammerman and Cavalli-Sforza's model that Renfrew does not invoke (see his criticisms; 1992b: 463-465), he does hold out hope that a more refined analysis of genetic markers may be an important source of collateral evidence for the population movements he postulates in connection with his demic-diffusion model of linguistic replacement.

Despite strong critical reactions to every aspect of this original model, Renfrew now argues that it can usefully be generalized to many other areas of the world. Some linguists propose the existence of a few broad macrofamilies which reduce the bewildering diversity of contemporary languages--some 5,000 to 10,000 distinct languages, depending on how you count (Renfrew 1992b: 449)--to between 17 and 20 linguistic phyla, excluding 6 or 7 isolates and various pidgins and creoles of recent origin (Renfrew 1992a: 13; see also Ruhlen 1987). At the same time, Cavalli-Sforza argues that there is a broad congruence between these linguistic families and genetic affinities now being documented among contemporary

human populations (Cavalli-Sforza 1991; Cavalli-Sforza et al. 1988: 1990).

The case Renfrew makes for extending the demic-diffusion model to other linguistic macrofamilies closely parallels his original arguments for the Indo-European hypothesis. The affinities between the languages which constitute these macrofamilies cannot be explained by relatively simple models that postulate a single episode of initial colonization followed by local processes of linguistic change.[ix] Over 12,000 or more years, linguistic divergence would have generated a plethora of local languages whose connection to an original proto-language would probably no longer be evident. To account for contemporary affinities, this diversity must have been reduced in many regions by episodes of recolonization like that postulated for the region in which Indo-European languages are now spoken. Renfrew argues that "much of the world's [contemporary] linguistic map" must have been shaped by large-scale linguistic replacement realized roughly between 7000 and 3000 B.C. (1992a: 39), a period in which waves of agricultural advance can be documented for many of the regions in question.

Renfrew's global thesis is, then, that the demic-diffusion model can be extended to roughly a third of the macrofamilies thus far identified by linguists (1992a: 24).[x] The broad outlines of contemporary linguistic families were established by the end of the Neolithic, and subsequent episodes of elite-dominance (including colonial expansions of the last five centuries) have served primarily to complicate, rather than fundamentally alter, this picture. While Renfrew remains cautious about appeals to parallels between genetic and linguistic affinities here, as in the case of Indo-European, he is hopeful that new techniques for molecular analysis will refine the existing phonetic dendrograms and put reconstructions of common genetic stock on a more secure footing (Renfrew 1992b: 467). The really significant genetic contributions to Renfrew's synthesis will come when these techniques are successfully applied to the surviving skeletal remains of ancestral populations.

Reservations and Questions

Convergence arguments and unification.

There is an unmistakable sense of excitement associated with this grand synthesis. Here we stand, on Renfrew's telling, heirs to decades, indeed, a century or more of intensive programs of research in at least three independent fields that all bear on a common set of explanatory problems: archaeological work on the origins and spread of modern humans and on the rise of farming in various parts of the world; linguistic investigation of the distribution of contemporary languages and their affinities; and biogenetic studies of human populations. These lines of inquiry have now progressed to a point where each is in a position to map the large-scale distribution of linguistic, cultural, and genetic phenomena and to propose general explanatory models of how diversity in their realm might have arisen. What most intrigues Renfrew about the demic-diffusion model is the new synthesis it promises of these diverse lines of inquiry, not only in the Indo-European case but, potentially, across a number of different cultural and linguistic regions. Indeed, the theme that figures most prominently in his advocacy of the demic-diffusion model is the remarkable nature of the "convergence" (1992a: 12; 1989b: 114)--the "congruence," the "mutual compatibility," the "curious parallel[s]" (1992b: 449)--that this model brings into view and makes intelligible.

A related theme, especially prominent in discussions published in the early 1990s, is Renfrew's conviction that any model building exercise should be guided by a "principle of parsimony" (1992a: 16-17). Although the demic-diffusion synthesis is still very much a conjecture, he insists that it has "the merit...of offering a relatively simple account in historical terms for the distribution of languages of the world" (Renfrew 1992a: 23). Indeed, Renfrew argues that "it is the function of models to simplify and make intelligible, so that despite the scepticism of some, it is no reproach to my explanations that they are simple, and offer simpler outcomes than are seen in reality among the data" (Renfrew 1992a: 55). In these statements, Renfrew articulates a conception of the nature and aims of scientific explanation which is strikingly similar to the intuitions that Kitcher, among others, describes as central to unificationist theories of explanation.[xi] He treats explanation as serving primarily to systematize as many and as diverse a range of phenomena, using as few premises and as limited a store of "argument pattern"s or "ways of thinking," as possible (Kitcher 1989).

Consistent with the central tenets of unificationism, Renfrew repeatedly defends the value of idealizations. His arguments here closely approximate Kitcher's observation that a key step in developing an explanatory theory is always to formulate an idealized description of the explanandum, shifting the focus of explanatory inquiry from the question of why a particular object behaves as it does, to that of why "ideal objects of this general type exhibit these properties" (Kitcher 1989: 453). In adopting a strategy of unification by means of idealization, Renfrew's approach to explaining the distribution of Indo-European languages and other macrolanguage families is very much top-down, an example (if it succeeds) of theoretical explanation that proceeds by appeal to general principles, showing how particular explananda "fit into the universal scheme of things" (Salmon 1989: 183) or, at least, fit into larger and encompassing structures. It is specifically not the point of such explanations to provide a detailed account of the mechanisms or processes by which a given outcome is produced--the "underlying micro-structure of what they endeavor to explain" (Salmon 1989: 184)--as would be typical of the bottom-up, causalist approach that Salmon advocates. In the evaluation of prospective explanations, on this account, it is crucial that idealizations be formulated and selected with an eye to their scope of application (albeit subject to a proviso which I discuss below). And here, again, Renfrew's intuitions about the significance of the synthesis afforded by the demic-diffusion model seems to be exactly those central to Kitcher's unificationism, especially where its extension to language families other than Indo-European is concerned.

When pressed on the question of why simplifying idealization is desirable, however, Renfrew notes not only that it enlarges the scope of a model, allowing for a broader synthesis of disparate phenomena within its domain and across formerly distinct domains, but also that such unifying power is surely desirable because it enhances the credibility of the model. It is an indication that the model successfully captures what he describes as "an intelligible mechanism by which a basic process can be understood" (Renfrew 1989b: 463). This formulation is consistent with the unificationist intuition that basic-ness just is a matter of providing broad unification (Kitcher 1989: 487, 496-497), but Renfrew later adds a much stronger claim: that if the demic-diffusion model proves applicable to a number of non-Indo-European language families--to "much of the world's language map" (1992a: 39)--this will improve its credibility in the original domain of its application, as an explanation for the spread of Indo-European languages. Such extensions are understood not just to expand the unifying breadth of the model, but to reinforce its claims about the causal efficacy of the mechanism invoked; if it seems likely that linguistic replacement was accomplished by means of demic-diffusion in other contexts, then it is all the more plausible that it could have been responsible for the spread of a stem language in the Indo-European case. Here Renfrew shifts from a claim about the explanatory power of the model, conceived first and foremost as a function of its capacity to bring diverse phenomena under a common argument pattern, to a claim about the evidential support that accrues to the model, now construed in causalist terms, when a number of independent lines of evidence converge on its central claims. This latter line of response sits uneasily with the unificationist themes that dominate Renfrew's defences of his model; he suggests that powers of unification are a virtue of explanation in part because they provide reason to believe the model’s ontological and causal claims. Although this introduces a striking tension into Renfrew’s own arguments, in invoking these considerations he makes use of a pattern of justificatory argument that is ubiquitous in archaeology, viz., that it would be highly implausible, given the independence of these various lines of evidence, if the mechanisms postulated (abductively) to explain them did not actually exist and operate as proposed.

Despite recurrent epistemic pessimism about the prospects for making effective use of fragmentary, ephemeral archaeological data as evidence, I argue elsewhere that the strategies archaeologists have developed for exploiting a range of background knowledge can be very effective in establishing a network of evidential constraint (Chapters 12-15). The interpretive ladening of data with theory--its (inductive) constitution as evidence--is a complicated business, but the complications cut in both directions where worries about circularity are concerned. Given pervasive disunity among the sciences on which archaeologists must rely to establish empirical claims about the temporal depth and contemporaneity of neolithic sites, dietary profiles, prehistoric demography (especially changes in fertility), subsistence practices, social organization, and patterns of cultural contact and diffusion--the claims central to the debate about the demic-diffusion model and its explanatory power as an account of the distribution of proto-Indo-European--there is no guarantee that all the relevant lines of evidence will converge on one

explanatory model rather than another (or, indeed, on any of the models under consideration). When they do, archaeologists (sometimes) have grounds for confidence that they know, within a specifiable range of error, how old the record is, what plant resources a prehistoric community exploited, how resources were distributed, and perhaps how the community was organized productively and reproductively; they can establish that particular events and conditions, and not others, actually (or likely) did obtain in a particular past context as described. As I have suggested, the principle at work here is that of a modest "piecemeal" or "local" realism (Miller 1987; Wimsatt 1987: 23-24, respectively; Chapter 5): to varying degrees it would be a miracle if each of these lines of evidence incorporated compensating errors, producing a spurious convergence, given their independence from one another.[xii] In these cases, the power of an explanatory hypothesis to induce convergence among disparate (inductively constituted) lines of evidence establishes its credibility as an account the causal conditions (broadly construed) responsible for the surviving record.

Despite my sympathy for convergence arguments in this evidential sense, Renfrew's use of them gives me pause. Their appearance in some of his defensive arguments for the demic-diffusion model seems, at best, incongruous. They mark significant slippage in what he means by convergence which allows him to shift from a primary emphasis on the explanatory power of the demic-diffusion model--from a preoccupation with what this model can do for the archaeological and linguistic (and prospectively, genetic) phenomena in the wide range of locales where various macrofamily languages are now spoken--to a concern with what these phenomena, considered as evidence, can do for the model construed as an account of mechanisms and processes that actually produced these phenomena and their intriguing patterns of distribution and affinity. Renfrew's appeal to large scale (quite literally global) consilience forces the question of when the convergence of evidence is compelling and when not.

It is by no means clear that Renfrew's synthesis of historical linguistics, archaeology, and historical genetics, and its extension to a range of linguistic macrofamilies, fills the evidential role he claims for it. That is, it is by no means clear that this unifying power as an explanation establishes grounds for believing that the processes of linguistic diffusion and replacement posited by the model actually occurred in anything like the sense I have been claiming for the homelier reconstructions that find support in the unexpected convergence of diverse lines of archaeological evidence. In fact, a central point of contention in the debate about Renfrew's syntheses is precisely that of what relationship holds between his highly abstract and simplified ("parsimonious") explanatory model and the reconstructions of local sequences of cultural transition it subsumes. In what follows I summarize the key lines of criticism brought against Renfrew's equivocal use of convergence arguments and, in the process draw out their implications for the philosophical debate about the nature and ground of explanatory power.

Four objections.

First, much depends on how the linguistic explanandum is characterized. Renfrew's assessment that some form of linguistic replacement model is required turns on his claim that the current distribution of languages is too simple to be explained in terms of initial colonization and the subsequent (local) differentiation of daughter languages. In fact, the global synthesis assumes the credibility of the macrofamily constructs and these are themselves quite contentious in some respects. If they were rejected or substantially reformulated, Renfrew might well find himself in the awkward position of providing an elaborately unifying explanation for a non-pattern. As one pair of critics put it, referring to Renfrew's safest case; "a linguist would have expected the author to stress the fact that Indo-European is a construct, not a demonstrable reality" (Zvelebil and Zvelebil 1988: 575). In effect, a construct of this sort is already a substantially simplifying unification that prefigures the quest for a unifying explanation.[xiii] And in this case evidential nepotism threatens (to use Kosso's term; 1989); vertical independence is compromised to the extent that the linguistic evidence of macrofamily affinities presupposes Renfrew's favored explanatory hypothesis.

Second, Renfrew's argument that demic-diffusion is the most plausible linguistic replacement hypothesis depends on the claim that all serious competitors have been considered and are inadequate. Not surprisingly, a number of critics object that even if the existence of Indo-European or other macrofamilies is accepted, it does not follow that an event, a single unitary process of similar scale, must be invoked to explain this outcome (see Zvelebil and Zvelebil 1988). One insists that Renfrew underestimates just how continuously dynamic language can be in small scale, non-literate societies;

significant linguistic change can occur "without radical change in the material particulars of life and with an amount of change in the human gene pool so small as to be for all practical purposes undetectable" (Ehret 1988: 571). One implication of this potential for rapid local change is that language replacement at the time of the Neolithic transition may be too early to account for contemporary affinities among Indo-European languages; "it is by no means certain that after 8,000 years the languages introduced by the first farmers in Europe could even be recognized as having a common origin" (Sherratt 1988: 459). At the very least, several intermediate steps must be postulated for intervening time periods (mainly the Bronze Age) in which it is plausible that processes of linguistic convergence, the formation of common trading languages, and lesser episodes of invasion and subjugation (associated with the secondary products revolution) would have occurred. And this reopens the case for considering hypotheses that postulate messier, more localized processes of continuous development.

In this case, contra Renfrew, models of convergence through interaction, and/or the formation of creoles and lingua franca may well have the resources to explain contemporary language distributions without invoking large-scale diffusion of a proto-language ancestral to those that now appear similar (Sherratt and Sherratt 1988). Perhaps the more local (but widespread) movements of people and cultural traits documented for the Bronze Age did constitute migrations and diffusions of cultural influence capable of accounting for contemporary linguistic affinities even if they do not constitute an episode of elite dominance (Anthony 1996; Anthony and Wailes 1988).[xiv] The general point is that, "by starting from the premise of unity, we simply stack the deck" (Barker 1989: 448).[xv] It is by no means clear that Renfrew's demic-diffusion model is the only one capable of explaining the existence of contemporary linguistic macrofamilies if you are prepared to question this premise and consider less tidy, tightly unifying, models of prehistory. His claim that it is the only adequate option on offer reflects an implicit metaphysical commitment to the view that causes must match effects in scale, and that it must be possible to discern a causal hierarchy in which the messy, multi-component factors distinctive of local contexts must ultimately depend upon (or be reducible to) a small set of simple, “basic” causal processes. To make this assumption determines in advance what range of explanatory hypotheses can be considered, establishing a reference class defined by the key characteristics of the hypothesis Renfrew himself favors. Again, epistemic independence is compromised when the evidence an hypothesis is designed to unify is then cited as its main source of empirical support.

Third, a number of archaeological critics have objected that, even if you grant Renfrew his arguments for preferring hypotheses that postulate a single, fundamental replacement process, it is by no means clear that the demic-diffusion model has the resources to explain the existence of Indo-European or, indeed, other macrofamily languages. Renfrew helps himself to a number of assumptions about the causal efficacy of the (subsistence) mechanisms and (demographic) catalyst central to this model that his critics challenge. For example, why should we assume that early Neolithic farmers have such a decisive adaptive advantage over foragers that they will inevitably displace them? In many locales, both in Europe and elsewhere, there is evidence that farming did not automatically or completely displace foraging and gathering-hunting modes of subsistence; sometimes foragers and farmers co-existed for a very long time, and often those who made use of cultigens relied on a mixed subsistence strategy. Moreover, when farming did ultimately prevail, it was often through a much slower and more uneven process than Renfrew's model envisions.[xvi] In particular, given local continuities in cultural traditions through the Neolithic transition, it seems that farming technology often diffused on its own; the methods and tools of farming were taken up, piecemeal and syncretically, by indigenous foragers who did not necessarily find themselves displaced as a population, and did not necessarily adopt other cultural practices associated with farming.[xvii] This leaves open the question whether, and to what extent, the language of the original farmers diffused with their farming technology.[xviii]

A related criticism focuses on the demic component of the model, drawing attention to the fact that the proposed catalyst for diffusion--population pressure--is by no means an automatic corollary to the advent of farming. The fifty-fold increase cited by Renfrew (and by Ammerman and Cavalli-Sforza) is a potential, but “it cannot be assumed that such potential had a profound impact in the Neolithic...Neolithic farmers faced many social, technological and environmental handicaps in Europe which might have reduced their reproduction capacity” (Zvelebil and Zvelebil 1988: 579). Indeed, in many areas early farming populations seem to have been worse off than their Mesolithic counterparts where health status

was concerned, and their population densities were not different enough from those of foragers for demographic pressure to have functioned as the sort of catalyst required by Renfrew's demic-diffusion model. Reflecting on these and related problems, a number of archaeological critics conclude that Renfrew's updated and expanded formulation of the wave-of-advance model remains, in its specifics, "an improbable hypothesis for most parts of the continent" (Zvelebil and Zvelebil 1988: 579); similar objections are emerging where the global synthesis is concerned. In short, collateral evidence is lacking for key elements of Renfrew's hypothesis, construed in causalist terms.

These considerations lead, in turn, to a fourth (and final) critical point that raises directly the philosophical issues that concern me here. A number of Renfrew's critics object that, as a matter of principle, his model is inadequate because it is not properly grounded in, or congruent with, lower level, local reconstructions of the transitional processes responsible for the Neolithic revolution. Generalizing on this concern, they question the wisdom of his commitment to idealization and synthesis; perhaps the unifying power Renfrew so values is not, in fact, a virtue that should be given priority over all else. One critic pointedly describes the dangers of "excessively a priori" models as they arise in Renfrew's case:

> Whilst the "wave-of-advance" model has a beguiling simplicity, it probably misrepresents the reality of the process so profoundly that it may not be useful to keep it, albeit hedged around with the increasing number of ifs and buts about regional 'acculturation' and 'Neolithisation,' as our central notion for what was going on. (Barker 1988: 449)

Another relatively sympathetic commentator observes that:

> ... any enquiry which claims to be scientific or even merely systematic has to be shaped by models of some kind, whether these are explicit or not...[but serious problems can arise when models are] generated and shaped by mathematical criteria of elegance rather than by abstraction from the data. (Coleman 1988: 451).[xix]

In short, Renfrew's critics raise serious questions about both the inherent plausibility and the archaeological applicability of his demic-diffusion model which suggest that the "grand synthesis" may be spurious. They object that many of the instances the model is meant to cover do not conform to its expectations; that the mechanisms he posits to account for linguistic replacement are causally inefficacious even if they were instantiated in the contexts where they are supposed to have operated; and that the messier processes described by alternative models are not as obviously incapable of producing the outcomes to be explained as Renfrew had supposed, although they are more complicated and less powerfully unifying. Taken together these critics counter Renfrew's appeal to the unifying power of his model with demands that it should (also) meet the conditions of adequacy central to an ontic (causalist) conception explanation. They require Renfrew to provide an evidentially well-supported account of the mechanisms by which the Neolithic revolution brought about linguistic replacement in the specific locales covered by his demic-diffusion synthesis, and they tend are suspicious of appeals to the virtues of simplicity and unifying power as grounds, in themselves, for accepting Renfrew's synthesis unless causalist conditions of adequacy are met.

Causalist and unificationist criteria of adequacy.

Renfrew's response to these objections takes two forms. In some contexts he seems prepared to take seriously the causalist intuitions that underlie his critics' objections, but argues that the demic-diffusion model is an idealization which is intended to capture, at a high level of abstraction, "primary processes" that operate at a very large scale. It is no reproach to such a model that it fails to capture the details of the Neolithic transition in specific locales:

> The ultimate explanation for the present distribution of Indo-European languages will be a more complicated one than I have presented...but second-order (mainly later) processes can only be correctly interpreted if they are seen within a frame of reference which is approximately valid for the primary processes. (Renfrew 1988: 466)[xx]

Perhaps the demic-diffusion model is meant to describe the structure and mode of operation of underlying primary processes on the understanding that complementary models will provide a detailed account of the mediating secondary processes by which they were realized in particular locales. Or perhaps the explanatory power of claims about such primary processes lies in their ability to delineate broad categories of mechanism or process that did operate, but may have taken quite different forms in

specific instances. On either approach the demic-diffusion model provides an idealization of causal factors, as a causalist would say it must to have explanatory power, but draws attention to emergent properties of these factors or to processes that operate at a different scale than those of interest to his more particularist critics. On this reading Renfrew's model may best be construed as provisional, an example of the various types of "false models" that, on Wimsatt's account, "act as a starting point in a series of models of increasing complexity and realism," or "suggest...new alternative lines for the explanation of the phenomena," or provide a "template that captures larger or otherwise more obvious effects" allowing more accurate modeling of smaller scale, local phenomena (1987: 30-31).[xxi]

In other contexts, however, Renfrew sidesteps the objections raised by causalist critics, insisting on an ontologically thin reading of the claims he makes about "basic processes," and claiming non-causalist virtues for his proposed explanation. He reasserts the principle that models necessarily simplify and idealize in the interests of establishing a powerful, wide ranging “generalizable” synthesis (Renfrew 1988: 463). It should not be held against them that they do not accurately describe all (or any) particular instances in their domain, that they "offer simpler outcomes than are seen in reality among the data" (Renfrew 1992a: 55). Presumably, then, Renfrew’s model should be held accountable, not to individual instances, but to aggregate outcomes characterized in appropriately general terms; it is not necessary that any or all local Neolithic transitions follow a particular pattern, only that they should result in an overall spread of farming that correlates, in the area affected, with the distribution of contemporary language families like Indo-European. At one point Renfrew goes so far as to insist that the wave-of-advance postulated by Ammerman and Cavalli-Sforza model does not, in fact, make empirical assertions about actual Neolithic processes of transition; the 1 km a year rate of advance is a "factual assertion about the mathematics of the model: it is not an assertion of fixed rates of change" (Renfrew 1988: 463). In this case, rather than being provisionally false as in the case of Wimsatt's "false models" (Wimsatt 1987), perhaps, like Cartwright's laws (in How the Laws of Physics Lie, 1984), Renfrew's demic-diffusion model is intended to lie: his synthesis does not assume or establish grounds for ontological commitment to claims about underlying (“basic”) causal processes, just grounds for accepting the model as a formal heuristic--a unifying argument pattern--that serves to systematize, with sufficient accuracy for specific purposes, the aggregate inputs and outputs of large-scale, long-term cultural processes. The significant question is, then, whether the features Renfrew has subtracted or added or smoothed in his idealization make too large a difference in outcome for the idealization to be acceptable.[xxii] To assess Renfrew’s model in these terms, it would be necessary to specify more clearly what ends unification is meant to serve in the cases covered by Renfrew's demic-diffusion model.

The Prospects for Rapprochement

If consistently maintained, Renfrew’s second strategy of response may seem to diffuse the objections of his critics. His objectives are just different from their's. What he offers is a powerful unification of diverse phenomena under a single, elegant (simple) explanatory model, an argument pattern that can be repeated again and again in explaining the linguistic features of a wide range of cultural contexts (within and beyond the Indo-European case). This unifying power has considerable appeal, although it comes at the cost of adequacy to local details and cannot be expected to account for why or how the phenomena subsumed by the model should manifest the patterns that allow their unification.[xxiii] In this case it would seem that Renfrew and his critics are simply arguing at cross purposes. Perhaps Salmon's parable of rapprochement is relevant here. He describes a wager laid by a physicist colleague: that the balloon held by a young boy on an airplane would move toward the front of the cabin at takeoff, rather than toward the rear. The physicist won the bet but, Salmon notes, two explanations could equally be given to account for the phenomena: one would cite the behavior of expanding and jostling molecules, taking the form of a causal/mechanical explanation, and the other would appeal to the general Einsteinian principle that establishes an equivalence between the effects of acceleration and the effects of a gravitational field, exemplifying a unificationist approach (Salmon 1989: 183). Salmon argues that "both of these explanations are legitimate and...each is illuminating in its own way" (Salmon 1989: 184). He therefore urges a "rapprochement between the two approaches to scientific explanation that have been in conflict for at least three decades," mediated by an assessment of the pragmatic considerations that determine the circumstances under which each of these modes of explanation is appropriate (Salmon 1989: 185).

I believe, however, that there is more at stake than simply a judicious decision to focus on different aspects of the subject domain and the (ideal) explanatory text that it supports. The causalist objections raised by Renfrew's critics should be telling for Renfrew even if he were to adopt a consistently unificationist stance. Let me first indicate why this is the case with reference to Renfrew's synthesis and Kitcher's account of unificationism, and then conclude with a more general philosophical observation about about models of explanation and their relationship to arguments of confirmation that depend on the convergence of evidence.

Although a staunch advocate for the "church of unification," Kitcher is careful to counter the possibility that, if unchecked, the principle of explanatory unification "could run riot over the deliverances of experience" (1989: 489), opening the way to explanatory accounts whose superior unifying power is realized by arbitrarily fusing or embedding patterns, or by embracing implausible beliefs whose only recommendation is that they effect unification. He insists, in this connection, on the "proviso" that explanatory unification must be "conditional on principles that govern the modification of language and that rule on the acceptability of the proposed beliefs" (Kitcher 1989: 49). Any modifications to the existing knowledge base or language that a new theory proposes--the introduction or subtraction of beliefs to the knowledge base (K) and of predicates to the language (L)--must be justified on grounds that are, in effect, independent of any appeal to the unifying power of the theory and its modifications. If the dispute about the merits of Renfrew's synthesis is set in the larger context of theoretical and methodological debate within archaeology, it becomes clear that his critics are drawing attention to a number of ways in which Renfrew has not met Kitcher's “proviso”.

Renfrew's critics are frequently concerned not just that his passion for synthesis and simplicity obscures a number of complexities that are important if you have a taste for causal models or otherwise prefer to focus on the specifics of a given prehistoric period and locale; their complaint is not against idealization as such. Rather, they object that Renfrew is highly selective in granting priority to a small range of factors--specifically subsistence-technological and demographic factors--which, they insist, cannot account for the phenomena in question taken on their own. One such critic argues that there is a pressing need to "put aside the question of 'origins' that has dominated the subject [of Indo-European] for a hundred years" (Barker 1988: 449) and, in this spirit, urges the importance of coming to terms with the vagaries of modeling the social processes that mediated the response of human communities to the ecological factors, the biological desiderata of reproduction, and the technological and subsistence innovations associated with farming that Renfrew privileges as key catalysts and basic causal processes.[xxiv] The counterexamples introduced by such critics--for example, local transitions where farming was adopted only very slowly, was not associated with any major increase in population density, and did not involve wholesale replacement of local populations or cultures--serve to foreground the role and effects of precisely the sorts of social, symbolic, and cultural factors that Renfrew systematically discounts.

This objection has particular significance when you consider it in light of the intense debate among North American archaeologists about explanatory goals and criteria of explanatory adequacy since the late 1960s (see Chapter 4). Renfrew maintains a broad allegiance to the central tenets of processual archaeology, specifically, its commitment to an eco-materialist conception of the cultural subject and conviction that, if the technological and adaptive dimensions of these systems are granted causal primacy, it will be possible to set archaeological interpretation on a firm scientific footing; all aspects of cultural systems will be explicable in terms of those (material, eco-environmental) aspects of cultural past that can be most reliably reconstructed. Despite trenchant criticisms of these methodological and theoretical commitments, Binford continues to insist on a quite uncompromising and reductive form of this thesis: "institutions and cultural forms [which presumably include Renfrew's "basic processes"] must be thought of as having a life independent of their participants; they are the conditioners of the participants' behavior" (emphasis added, Binford 1983: 221). Given this understanding of the causal structure of cultural systems, Binford urges that archaeologists focus on "the macroforces that condition and modify lifeways in contexts unappreciated by the participants within complex thermodynamic systems" (1986: 474). The internal dynamics of cultural systems--social relations and structures, ideational factors, in short, the ethnographic lifeworld of human agents--are thus ruled out of account as irrelevant to

archaeological explanation, on eco-materialist principles, they are assumed to have no causal efficacy at the level of large-scale system dynamics; to use Renfrew's terms, they can be treated as (epiphenomenal) "secondary" factors and processes. Although Renfrew distances himself from Binford's more extreme statements--he is, after all, concerned to make sense of linguistic affinities and is a prominent advocate of "cognitive archaeology" (1993a)--he does presuppose something like Binford's distinction between internal or ethnographic, context and agent-specific factors (components of secondary processes), and emergent system-level dynamics (primary processes). And, among the range of systemic processes he might consider, he accords technological, demographic, and subsistence-related factors causal efficacy and priority in his explanation of Indo-European and other linguistic macrofamilies.

Those engaged in debate with Renfrew are by no means among the most radical critics of processual archaeology, but their substantive objections to his model clearly presuppose the fact that the more reductive and functionalist elements of its eco-materialist conception of culture have been seriously challenged; they are at least controversial, if not largely unsustainable. Matters are far from settled; many aspects of the processualist paradigm fruitfully persist alongside a diversity of anti- or post-processual approaches. Nevertheless, it seems fair to say that Renfrew's critics engage the resources of a knowledge base (K') that has been significantly modified by arguments establishing that, however difficult the task may be of reconstructing the internal social dynamics and ethnographic dimensions of past cultural systems, archaeologists cannot assume their explanatory and causal irrelevance, either at a local or at a systemic level, however advantageous this might be methodologically. Critics of processual archaeology routinely point out that there is much greater variability in the archaeological record than can be accounted for in adaptive-functionalist or eco-reductive terms (see, e.g., Hodder 1982b, 1986; Chapters 4 and 7), and they appeal to collateral ethno-historic evidence to establish, in general terms, the limitations of explanatory idealizations that privilege these factors.

These broad theoretical concerns are central to the debate about the adequacy of Renfrew's demic-diffusion model. It is specifically various sorts of social factors and internal dynamics that his critics insist are relevant for understanding how and why farming advanced in the (particular) way it did in various contexts and for determining whether, in fact, its advance could have been responsible for the processes of linguistic replacement that Renfrew considers necessary for explaining contemporary linguistic macrofamilies. The force of their objections is that Renfrew has not rebutted the collateral arguments that call into question his processual assumptions about the culture-transforming powers of technological advantage and demographic pressure. That is, he has not provided grounds for resisting the shift from K to K'--from a restricted eco-materialism to a conceptual framework that includes consideration of social, historical factors--apart from repeated assertions that, if the K-beliefs constitutive of the demic-diffusion hypothesis are retained, they promise powerful cross-context and cross-field unification. The situation is analogous, in the inverse, to that faced by the continental drift hypothesis in the early days of its elaboration, as described by Kitcher; it was beside the point to "expand the inventory of the advantages of unification" until objections to the very possibility of continents drifting had been addressed (Kitcher 1989: 492).

Notice, however, what Kitcher's “proviso” requires of Renfrew by way of rebuttal. To meet the objections to his failure to modify key (processual) beliefs about the cultural subject, Renfrew must provide independent (non-unificationist) grounds for believing both that the complex of subsistence, technological, and demographic factors he postulates did actually obtain in the contexts in question, and that they had the causal capacity (broadly construed) to bring about large-scale linguistic replacement as the primary processes responsible for establishing proto-Indo-European in the regions where its daughter languages are now spoken. That is, he must establish that the socio-cultural factors complicating this picture in most locales are causally dependent (or irrelevant), where this crucial transition is concerned. And he must show that wholesale linguistic replacement as early as 8000 B.P., in the case of proto-Indo-European, can account for the contemporary linguistic macrofamilies he means to explain without recourse to explanatory models that grant a central role to secondary (local) processes of continuous linguistic development. Kitcher's “proviso”, like Renfrew's critics, thus requires systematic evaluation of the claims Renfrew makes about the causal powers and capacities of the various factors cited by the demic-diffusion model. Indeed, at every level the debate over Renfrew's demic-diffusion synthesis turns

on judgments about the credibility of precisely the sorts of claims central to an ontic, if not narrowly causal, model of explanation. Far from being purely heuristic, assumptions about the causal efficacy

of demographic and technological/subsistence factors inform Renfrew's judgments about how to idealize in the first instance; they underwrite his assessment that social, internal factors had negligible effects at a systemic level. Most critiques of his model make these assumptions explicit and call them into question. And, in the end, whether not Renfrew's (non-)modification of processual beliefs is acceptable will depend on whether such causalist claims can be sustained empirically, even on a consistently unificationist view of the aims of explanation.

I suspect that the pivotal role played by such causal claims (and the need to establish their credibility) is not unique to Renfrew's model or to archaeology. I propose, more generally, that ontic considerations of a broadly causal sort routinely re-enter the picture with Kitcher's "proviso." Where appeals to unification are conditional on independent principles governing belief modification (Kitcher 1989: 489), as often as not the principles in question will specify conditions under which it is reasonable to believe that specific causal mechanisms, or other (structural) relations of dependence and determination, actually exist and have specific powers or liabilities. By extension, Renfrew's critics challenge not just his commitment to processual ideals or his (inconsistently maintained) unificationist view of explanation, but use he makes of convergent lines of evidence to support the claims about causal mechanisms and processes central to his demic-diffusion account of the spread of proto-Indo-European. Renfrew's appeal to the unifying power of his model as a source of evidence as well as explanatory power is problematic inasmuch as, at a number of junctures, the independence of the evidence he invokes from his test hypothesis is compromised. The capacity of the model to integrate, under one argument pattern, a range of archaeological and historical-linguistic phenomena is the primary reason for positing demic-diffusion as the mechanism responsible for the linguistic outcomes that require explanation, but Renfrew provides little evidence that this mechanism was (or could have been) responsible for the spread of proto-Indo-European independent of that which suggested the model in the first place; by contrast, his critics provide considerable evidence that it could not, or did not, in a number of specific locales. Perhaps the appeal to convergent evidence carries with it a requirement beyond epistemic independence of the various kinds discussed in previous chapters. The fact that a model can be fit to multiple lines of evidence (unifying and, in this sense, explaining them) is not, in itself, grounds for concluding that its ontological and causal claims should be accepted; if the dangers of reification are to be avoided, there must be evidence for the existence and operation the entities or mechanisms posits that is independent of the outcomes that the model was designed to explain.

Conclusions

I conclude with a jointly philosophical and archaeological observation. I suggest that it is no rebuke to ontic theorists that, on their view of explanation, it is "a purely contingent truth" that the independent causal structure of the world includes a limited number of basic mechanisms, rendering "unification...at best a contingent commitment of the tracing of causal structure" (Kitcher 1989: 497). Although it is too early to tell how the debate between archaeological processualists and anti- or post-processualists will turn out, I believe that what we are witnessing here is, at bottom, a dispute about whether the cultural subject domain studied by archaeologists is structured by a sufficiently small number of basic mechanisms to support a rigorous unificationism of the sort endorsed by Renfrew. The legacy of several decades of work under the aegis of the (positivist) New Archaeology is the growing realization that, as a matter of contingent (if explanatorily unfortunate) fact, the cultural worlds in question are sufficiently complex that they require an expanded store of argument patterns, many of which are not widely applicable. It is an open and empirical question what kinds of mechanisms or processes shape the cultural formations archaeologists hope to reconstruct and explain, but all indications are that simplifying, reductive models are unequal to the task of understanding these historically and dimensionally complex systems.[xxv]

The strategies archaeologists are using to sort out the scope and plausibility of claims about basic mechanisms (which is not at all specific to archaeology) turn on the judicious use of (evidential) convergence arguments to assess the plausibility of claims about the causal processes, structures, and relations of dependence responsible for prehistoric cultural forms and their archaeological record. The philosophical lesson here

is that the viability of a unificationist program (and associated methodological principles) is contingent on facts about the world, specifically, facts about the nature of the generative mechanisms and structures of dependency that actually inhere (or not) in the subject domains under investigation. And determining these facts of the matter requires a strategic variety of evidence, selected with an eye to countering not only the threat of circularity, but also a tendency to reify those hypothetical constructs that seem equal to the task of integrating the bewildering complexity of evidence that is the archaeological record.

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i Kitcher and Salmon (1989), Pitt (1988), and Ruben (1993) assembled anthologies on explanation and provide overviews of the recent history of the post-positivist debate about explanation. Kitcher (1995) describes causal and unificationist options as the two main (philosophical) approaches to understanding explanation that have emerged in response to the problems of asymmetry and irrelevance identified in protracted debates over the problems inherent in Hempel's D-N and I-S models. Salmon identifies three broad categories--modal, epistemic, and ontic--which subsume the causal and unificationist theories (variants of the ontic and epistemic conceptions of explanation, respectively) that will concern me here (Salmon 1984, 1989), as well as the pragmatic theories I mention in passing.

ii Kitcher adds, in a note, that it may be "entirely possible that a different system of representation might articulate the idea of explanatory unification by employing the 'same way of thinking again and again' in quite a different--and possibly more revealing--way than the notions from logic I draw on here" (1989: 501 n. 18).

iii Robustly realist variants of this approach were articulated in the 1970s by Harré (1970) and Bhaskar (1978), among others (see Keat and Urry 1975), who insisted that the central aim of science is not systematization which affords explanation as a derivative virtue (as empiricists maintain), but rather explanatory modeling of underlying causal mechanisms.

iv Salmon developed this causalist account in response to difficulties that overwhelmed the statistical relevance model he had earlier proposed as an alternative to refined versions of the nomic covering law model. On the SR model, explanations are not arguments, but simply accounts that identify factors, variables, that make a difference to the likelihood that the events/properties requiring explanation will occur. His 1978 presidential address to the American Philosophical Association, "Why Ask Why'?," marked this transition. Here he argued that concepts of statistical significance are not sufficiently rich to capture what we mean by explanatory relevance, an adequate understanding of explanation requires that we "put the 'cause' back in because" (1978).

v Renfrew observes that, "for more than two centuries," since 1786 (1988: 437), historical linguists have recognized these affinities as puzzling, and sometimes gives the problem a historical formulation:

> If we look at the distribution of Indo-European languages in Europe when we first see them in the centuries shortly before or after the beginning of the Christian era (or, in the case of Greece, a thousand years earlier), virtually the whole of Europe seems to have been Indo-European-speaking [by 2000 to 3000 years ago]....This is a vast area for such a degree of uniformity. (1987: 145)

vi Note the parallels with Binford's argument against Bordes' interpretation of Mousterian assemblages (Binford 1972b; Chapter 7 above).

vii Indeed, Anthony argues that, "to agree with Renfrew, archaeologists must dismiss most of what linguists have learned about the PIE [proto-Indo-European] lexicon in the past 200 years" (1996: 36; see also Anthony and Wailes 1988).

viii This estimate is disputed by various of Renfrew's critics; for example, Zvelebil and Zvelebil argue that the Neolithization of Europe is more likely to have taken 3500 years, given available archaeological evidence (1988: 578; see also Zvelebil and Zvelebil 1990).

ix Where original colonization is concerned, Renfrew appeals to the "out of Africa" monogenesis hypothesis according to which contemporary human populations are all descended from a species of modern humans that "emerged in Africa about 100,000 years ago," displacing earlier hominid forms as they diffused out of Africa; he sets the extinction of other proto-hominid forms at about 35,000 years ago (1992a: 12). These modern humans are presumed to have had a capacity for speech and symbol manipulation that earlier hominids did not, so this species diffusion is characterized as the primary episode of initial (linguistic) colonization. A series of other (later) initial colonizations took the form of post-Pleistocene circum-polar dispersals; these account for the distribution of four macrofamilies in the arctic and sub-arctic and into Austronesia.

x Renfrew (1992a) argues that his demic-diffusion model can be applied directly to at least three major language groups (Indo-European, Afro-Asiatic, and Elamo-Dravidian), and with some modification to several others (Niger-Kordofanian [Bantu] Austronesian, Sino-Tibetan).

xi Although Renfrew is sympathetic to the programmatic claims of New Archaeologists who invoked Hempelian covering law models of explanation, he is generally impatient with philosophical debates within archaeology (see his critique of "isms"; Renfrew 1982b), and there is no indication that he is familiar with, or has been influenced by, these post-positivist theories of explanation.

xii The arguments Salmon describes as causal/analogical turn on this sort of convergence argument (1989: 152). He refers to Hacking in this connection who discusses in some detail just the sort of miracle argument I describe here and throughout Section IV (discussed by Salmon 1989: 153; Hacking 1981: 317).

xiii See, for example, Anthony's summary of recent work in historical linguistics which suggests that a complex evolutionary tree for Indo-European languages and a sequence of splits from proto-Indo-European must be postulated to account for different kinds and degrees of affinity between the resulting daughter languages (1996: 38; see also Anthony and Wailes 1988).

xiv Anthony postulates two episodes of migration and cultural diffusion (not invasion) by horse-mounted pastoralists, the Yamna culture, between 3100 and 2200 B.P., emanating from western steppes and the Volga-Ural region (1996: 38-39).

xv I am struck by the parallel between this line of criticism and more general arguments against the standard wisdom that "the response of a large interactive system [must be] proportional to the disturbance [the events or states of the system providing explanation]" (Bak and Chen 1991: 46). These advocates of "self-organized criticality" suggest that a range of complex natural and social systems may be better understood by starting with the assumption that, if they are "weakly chaotic," they have a capacity to "perpetually organize themselves to a critical state" in which quite minor events can set off chains of interactions that have dramatic (indeed, catastrophic) effects.

xvi See, for example, the argument Smith has made where the development of agriculture in the Americas is concerned. In many (perhaps most) contexts a developmentally complex transition period involving mixed-strategy subsistence lasted many thousands of years (e.g., in Mexico, 6000 years); it is a mistake to treat this "'in-between' territory" as a "processually brief transitional interlude separating the steady-state solutions of hunting-gathering and agriculture" (1998: 1651).

xvii In some areas of Europe there was apparently a quite rapid transition to organized mixed farming, while in others, local sequences indicate that "agro-pastoral farming was added to the existing patterns of resource use by the indigenous populations" and was not associated with population movement or displacement (Zvelebil and Zvelebil 1988: 578 see also Barker 1988: 448). In still other areas, farming groups seem to have lived side by side with indigenous hunter-gatherers for long periods of time without having much impact on their subsistence practices; indeed, in many cases they have been mutually dependent. This pattern of non-conversion/non-displacement, or of long delayed intensification and diffusion of farming, was by no means unusual. In the Americas maize cultivation was viable some thousand years before it was intensified to become a transforming staple of life and diffused (unevenly) northward. In southern Africa Bantu-speaking agriculturalists evidently lived in close, symbiotic proximity with Khoisan gatherer-hunters for several thousand years without the latter being displaced (linguistically or in subsistence practice). In many areas, the transition to farming was accomplished only with the expansion of imperialist and more recent capitalist powers where the factors responsible for the diffusion of farming technologies (and, in some cases, associated languages and other cultural traits) are by no means reducible to agriculturally induced demographic pressure.

xviii Ironically, the critics who raise these questions turn back on Renfrew's own model a version of his central objection to elite dominance hypothesis: they ask whether farming technologies, language, and populations are so tightly interdependent that they must be assumed to diffuse or to change together.

xix Coleman continues: "most serious of all is the temptation, whenever a new model is developed, to apply it to the exclusion of all others [out of a zeal to compensate for, or overcome, the perceived inadequacies of existing models to account for a particular group of observations]" (Coleman 1988: 451). See also Sherratt's objection that Renfrew's approach can "justly be described as Procrustean in that it consists of lopping off those reconstructions which do not conform to a small number of preconceived models...the answers which are finally proposed are essentially large-scale versions of the migrations

sought by an earlier generation of scholarship" (1989: 459).

xx Alternatively, Renfrew observes that the role of an abstract demic-diffusion model like Ammerman and Cavalli-Sforza's (and Renfrew's) is to offer "an intelligible mechanism by which a basic process can be understood" (1988: 463). He makes this statement in the context of observing that the wave-of-advance model "was formulated by Ammerman and Cavalli-Sforza for a well-defined general case (involving an anisotropic landscape and a homogenous population of farmers) such as could never exist in the real world" (1988: 463).

xxi I am here referring to three of the twelve types of false models that Wimsatt describes as functioning to generate "truer theories" (Wimsatt 1987: 30-32).

xxii See, for example, Cartwright's summary of discussions of idealization (1989: 354), and Kitcher's assessment of their implications (1989: 453).

xxiii Renfrew is inevitably negotiating a trade-off between theoretical virtues that is familiar throughout the social and life sciences, for example, as discussed by Levins: "there is no single, best all-purpose model...it is not possible to maximize simultaneously generality, realism, and precision" (Levins 1968: 7). I am grateful to James Griesemer for directing me to Wimsatt's and Levin's discussions.

xxiv To anticipate the argument that follows, the factors of technology, subsistence, and demographic pressure are the deus ex machina, as Barker refers to them (1988: 449), that are typical of the genre of explanation in archaeology associated with the processual or New Archaeology.

xxv For a parallel argument where research in the life sciences is concerned, see Longino (1994: 476-79, and 1990 ; discussion in Wylie 1995b).