*Philosophical Psychology*
*Vol. 22, No. 5, October 2009, 611–635*

Routledge
Taylor & Francis Group

# Time is of the essence: Explanatory pluralism and accommodating theories about long-term processes

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*Unified, all-purpose, philosophical models of reduction in science lack resources for capturing varieties of cross-scientific relations that have proven critical to understanding some scientific achievements. Not only do those models obscure the distinction between successional and cross-scientific relations, their preoccupations with the structures of both theories and things provide no means for accommodating the contributions to various sciences of theories and research about long-term diachronic processes involving large-scale, distributed systems. Darwin's theory of evolution by natural selection is the parade case. Explanatory pluralism accommodates a wider range of connections between theories and inquiries in science than all-purpose models of reduction do. Consequently, it provides analytical tools for understanding the roles of the theoretical proposals about the evolution of the human mind/brain that have proliferated over the last two decades. Those proposals have testable implications pertaining to both structure and processing in the modern human mind/brain. An example of such research illustrates how those proposals and investigative tools and experiments cut across both explanatory levels and modes of analysis within the cognitive sciences and how those studies can yield evidence that bears on the assessment of competing theories and models.*

*Keywords: Amygdala; Cheater Detection; Cross-Scientific Relations; Diachronic Theory; Explanatory Pluralism; Mechanism of Natural Selection; Mental Module; New Wave; Reduction; Selection Task*

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ISSN 0951-5089 (print)/ISSN 1465-394X (online)/09/050611-25 © 2009 Taylor & Francis
DOI: 10.1080/09515080903238906

## 1. Introduction

New Wave models of scientific reduction incorporate the best insights of both the logical empiricists and their critics. They propose that cases of intertheoretic mapping can fall along a continuum of possibilities from nearly perfect isomorphs at one end (which the logical empiricists tended to stress) to utterly irreconcilable conceptions at the other (which Paul Feyerabend and Thomas Kuhn highlighted). The New Wavers have also stressed that what is surely a majority of cases fall somewhere between their continuum's two extremes (Endicott, 2007).

These achievements notwithstanding, the New Wavers' accounts of intertheoretic relations in science (Bickle, 1998, 2003; Churchland & Churchland, 1990; Hooker, 1981) remain blunt instruments for analyzing cross-scientific relations. Like the logical empiricists before them, they offer an *all-purpose*, "one size fits all" model of intertheoretic relations and presume that accounts of the *structural* relations of scientific theories' explanatory principles (e.g., laws) and of the things that those theories describe exhaust what is of ontological and epistemological interest in such comparisons. I have argued elsewhere that their proposals downplay epistemologically significant features of the relevant sorts of scientific research and fail to discriminate between different classes of intertheoretic relations.[1] Specifically, New Wave models obscure the distinction between theory succession over time within some science and the relations of theories from different sciences at some particular point in time. In short, they ignore the differences between *successional* relations and *cross-scientific* relations. Clearly, the New Wave continuum *can* be deployed in *both* settings and cases arise in both in which the intertheoretic translations are abysmal. *But* it does not follow that the two settings involve the same dynamics.

Rapid and substantial change in the succession of one theory for another within some science results in Kuhnian (1970) scientific revolutions. Beyond their overlapping explananda, the connections between the theories in these cases are so fragmentary that the triumphant successor eliminates its predecessor, and so, as the New Wavers emphasize, the history of science is largely a history of discarding once-honored theories, concerning everything from the crystalline spheres above to the bodily humours within, in favor of new, superior theories. Because they do not distinguish among the various contexts in which comparisons of scientific theories and enterprises occur, the New Wavers presume that, *regardless of the context*, grievous breakdowns of intertheoretic mapping will *always* end in the eradication of one of the theories.

Neither the historical evidence nor widespread conceptions of science suggest that this strong conclusion is true. *All* of the illustrations of theory eliminations in the history of science (including the theories of impetus, phlogiston, caloric fluid, the luminiferous ether, phrenological faculties, vital spirits, and so on) that the New Wavers cite have resulted from theory succession within a particular science. Conversely, *none* of these have resulted from the comparison of theories in cross-scientific settings, i.e., from the comparison of theories reigning simultaneously in sciences operating at different analytical levels.[2]

Ernest Nagel (1961) understood that any reduction of theories as a result of cross-scientific comparisons would be tantamount to the reduction of the entire sciences in which they prevail (McCauley, 2008). Thus, so too, presumably, on the New Wave view would the elimination of scientific theories on the basis of cross-scientific comparisons lead to the wholesale elimination of the sciences from which those theories issue. At least some of the time, in the face of *that* apparent consequence of their views, Bickle (1998, pp. 205–206, 2003, p. 110), P.M. Churchland (1981), and Feyerabend (1963, 1967), have not blinked.[3]

Explanatory pluralism (Looren de Jong & Schouten, 2007; McCauley, 1986, 1996; McCauley & Bechtel, 2001) maintains that the New Wavers' prescriptions for such cross-scientific cases would leave the sciences *needlessly* impoverished, since the sciences' honorific epistemic status turns in part on their inexhaustible demand for *new* empirical tests. Much of the evidence that a theory must account for, whether sooner or later, stems from research carried out at other (including higher) levels of analysis in science. This is one of the principal means by which scientists demonstrate a theory's consilience (Wilson, 1998)—a recent illustration is the extensive use that contemporary researchers in neuroscience make of findings from psychological experimentation. The putative cross-scientific eliminations that the New Wavers (and eliminative materialists) envision would simply decrease the theoretical, evidential, and experimental resources available for science to call upon. Once scientific disciplines achieve some stability, as marked not only by their theoretical and empirical accomplishments but also by the emergence of distinctive disciplinary identities and the professional societies, specialized journals, and university departments which follow, they—in contrast to the particular theories that might rule at any moment within those disciplines—do not, subsequently, go extinct. Instead, they add to the explanations and accounts of the world that science furnishes and supply bodies of empirical findings that all sciences are, subsequently, free to exploit.

Convinced that these limitations of all-purpose models of reduction in science prevent them from accurately representing the underlying methodological, epistemological, and ontological complexities that cross-scientific comparisons present, philosophers of science have forged new analyses that aim to capture the dynamics of cross-scientific inquiries more faithfully (Richardson, 2007a, pp. 138 & 142). These new pluralistic analyses accentuate both the multi-level contributions to scientific explanations of complex phenomena (e.g., Machamer, Darden, & Craver, 2000, p. 23) and the multiplicity of complementary explanatory perspectives the sciences offer. Whether writ large as explanatory pluralism or writ small in the mechanists' analyses,[4] this pluralism underscores how the sciences integrate information about patterns that systems exhibit not just with that available at lower levels about those systems' parts but also with that at higher levels. Inquiry at those higher levels takes up factors influencing those parts' organization and workings and examines both the settings in which a system may be situated and the various external factors that constrain its shape and inputs.[5] In cross-scientific contexts scientists may pursue two integrative strategies, viz., *reductive* strategies exploring systems' components or *contextualist* strategies that explore how a system's setting influences its makeup and behavior.

It follows, of course, that neither explanatory pluralists nor mechanists disapprove of reductionist strategies or of reductionism, properly qualified. Arguably, the pursuit of reductive explanations has been the single most effective research strategy in the history of modern science. Broadly, it counsels looking for a mechanism at a lower level of analysis as one of the best ways to explain a pattern at a higher level (Rosenberg, 2006, p. 54). If psychologists find dissociations between people's abilities to locate an object and their abilities to identify that object, it is reasonable to look for separate processing streams for such information in the brain (Ungerleider & Mishkin, 1982). Or if, across cultures, rituals overwhelmingly cluster around certain attractor positions in the space of possibilities, it is reasonable to look for underlying psychological mechanisms to explain the appeal of the corresponding forms (McCauley & Lawson, 2002).

Contrary to the special pleading of doctrinaire anti-reductionists for the autonomy of one social or psychological science (or phenomenon) or another, exploring reductive possibilities downstairs, no less than exploring integrative contextualist possibilities upstairs, opens new avenues for sharing both explanatory insights and methodological, theoretical, and evidential resources. Anti-reductionists' special pleading is simply a way of trying to forestall the checks and balances that reductive integration imposes. Such special pleading also forestalls opportunities for new investigations at both levels and for collaborative research between them. Concerns for access to the full range of available evidence and problem solving strategies will—at all levels of scientific inquiry—safeguard (rather than diminish) spaces for reductive explorations.

New Wave reductionism, my first (long-standing) objection above concerning its failure to attend to the distinction between successional as opposed to cross-scientific contexts, and virtually all previous philosophical discussions of reduction assume the soundness of a hierarchical model of analytical levels in science. In section 2 I will offer two criteria for distinguishing analytical levels in science that are not subject to the criticisms lodged against the standard conception. That conception, which so many philosophers take for granted, characterizes levels in terms of the *scale* of the things that the resident sciences discuss. The two criteria suggest a framework of analytical levels that makes sense of distinguishing cross-scientific contexts and, thus, that suggests that my first, long-standing objection to New Wave reductionism can, in fact, be sustained.

In sketching a more detailed picture of just what explanatory pluralism involves, section 3 considers some additional deficiencies of New Wave reductionism (see too McCauley, 2007b). The New Wavers' preoccupations with structural relations of theories' explanatory principles and of the things those principles discuss in addition to their indifference to the distinction between successional and cross-scientific settings tend to divert their attention from entire areas of scientific theorizing and research when they offer their "ruthlessly" reductionist verdicts about cross-scientific cases (Bickle, 2003). All previous philosophical discussions of scientific reduction, including New Wave accounts, have basically ignored the crucial role that investigations of long-term diachronic processes involving large-scale, distributed systems can play in science. Charles Darwin's theory of evolution by natural selection is the parade case, and its impact on the development of the biological sciences over the past century is patent. Philosophical models of cross-scientific interactions whose analytical tools cannot readily accommodate these theories about long-term diachronic processes overlook some of the most important interlevel influences in the sciences at the turn of the third millennium.

Section 4 reviews a recent illustration in psychology of the impact of such theorizing and research about long-term diachronic matters. It examines how exploring the implications of theories in evolutionary psychology has occasioned new experimental findings that have led to a reassessment of familiar, established findings within cognitive psychology and provoked new cross-scientific collaborations and programs of research that have led to new proposals, downstairs, in clinical neurology. Neglect of these additional kinds of cross-scientific connections does not redound to any philosophical model's credit and especially not to New Wave models that are not equipped with the analytical tools even to distinguish cross-scientific contexts in the first place.

## 2. Analytical Levels in Science

Widespread assumptions about levels of analysis in science and their hierarchical arrangement stand behind the talk above about such things as cross-scientific relations and scientific forays "upstairs" and "downstairs." Those same assumptions also stand behind the project of reductionism more generally. Nagel's (1961, p. 339) model addressed both the reductive "development of a science" (what I have been describing as successional relations) and the reduction of sciences (what I have framed in terms of cross-scientific relations). Nagel accords far greater attention to the latter sort of case, which he construes as the more problematic, since they are "heterogeneous" reductions. Their heterogeneity primarily concerns the special problems they raise for translating the language of the reduced theory into the language of the reducing theory. Theories arising from different sciences take different perspectives. They employ different concepts; they study different patterns; they countenance different entities and predicates, and their analyses usually proceed at different scales.

The standard account of these different perspectives presumed then and continues to presume today claims about a hierarchy of analytical levels in science. The standard account of that hierarchy, likewise, appealed then and continues to appeal today to mereological considerations, i.e., to the relations of parts and wholes. Analyzing the behavior of complex wholes in terms of the behaviors of their parts is the most basic tenet of reductionism in cross-scientific settings. Although reductionists focus on translating the higher level reduced theory's claims into the language of the lower level reducing theory, a central part of that task is mapping the things the reduced theory talks about on to the things that the reducing theory talks about, and *that* turns on establishing bridge principles that link those things' predicates.[6] Bridge principles might, for example, link talk about patterns of activation in brain regions in cognitive neuroscience to claims about cascades of firing neurons in those areas at the level of cell assemblies. In cross-scientific settings the most straightforward briefs for reductions surface when the things that the lower level reducing theory discusses are the parts of the things that the higher level reduced theory discusses and when the properties of the latter are readily translatable into talk about the properties of the former.

Big things are made of smaller things and those smaller things are made of smaller things yet. This organization in nature based on mereological relations and, specifically, on their typical implications for things' relative sizes motivates the standard account of the hierarchical structure among the sciences. A consequence of using considerations of scale for differentiating levels in nature and levels of analysis in science is that higher level sciences treat big things and the lower level sciences treat progressively smaller things. In the resulting picture the physical sciences are the most fundamental sciences, operating at the lowest levels of analysis, because they deal with the smallest things that are the parts of all other things. The biological sciences treat larger systems that involve more complex physical arrangements. The psychological and social sciences tackle larger systems still. At least some of the time, psychology examines organisms situated in physical and social environments, and the socio-cultural sciences address large collections of psychological systems that are, at least, loosely connected causally in social and cultural networks.

Reductionists and philosophers of mind interested in the conundrums of mental causation have routinely subscribed to this approach to delineating analytical levels (see Glennan, in press; Kim, 1998). Even when looking at the broad families of sciences at this comparatively coarse grain, though, such a conception of analytical levels is unsatisfactory. An account of organizational levels in nature and, thereby on this routine story, of analytical levels in science, that looks to considerations of scale will prove inadequate, because not all big things that have lots of parts (e.g., asteroids and sand dunes) are highly integrated systems that demand higher level analyses. A strictly mereological conception of analytical levels that stresses considerations of scale does not square very well with the fact that the physical sciences contend not only with sub-atomic particles but with avalanches, weather systems, and stars or with the fact that the biological sciences not only include molecular genetics but investigations about the evolution of populations. The standard conception of analytical levels handles the relations between the physical, biological, psychological, and even socio-cultural sciences of medium sized terrestrial objects well enough, but they mostly leave sciences like meteorology, geology, astrophysics, ecology, and evolutionary biology largely unaddressed.

A pivotal question for the differentiation of analytical levels, then, is whether or not wholes are notably organized or are simply aggregates of their parts (Wimsatt, 1986, 1997, 2007, chapter 9). One way of casting disagreements about reductionism's promise is by asking whether any features of wholes resist straightforward explanation in terms of their parts, i.e., whether from an explanatory standpoint wholes are greater than the sums of their parts. If an entity or system contains parts and if explanations of some of its behaviors require *organizing* principles pertaining to those parts that, for example, inevitably require their adjustment or transformation in the course of their integration within the system, then findings about that entity or system may sometimes be regarded as issuing from a higher level of organization and, thus, likely pointing to a distinguishable analytical level. Consider Andy Clark's discussion (2008, p. 38) of adjustments to the responses of bimodal neurons in the parietal area when experimental subjects have undertaken but five minutes of goal directed activity with a rake in one of their hands. Such organizational and contextual considerations influence which levels will yield opportune explanatory perspectives.

Such organizational and contextual considerations inspire mechanists' localist accounts of analytical levels.[7] They argue that attention to the organization and operations of situated mechanisms and to the local view of analytical levels that results tends to dissolve temptations to pronounce about causal closure at lower levels and about the putative comprehensiveness of lower level explanations (Bechtel, 2006, 2007, p. 182; Craver, 2007; Craver & Bechtel, 2007). They are agnostic about the generalizability of the pictures of analytical levels that stem from accounts of particular mechanisms and have abandoned sweeping ambitions to characterize the sciences' overall architecture. Focusing on the details of research about particular mechanisms motivates case-specific accounts of analytical levels.[8] With these mechanists' reservations in mind about building up a global account of levels of analysis from models of particular mechanisms, the question of salvaging *any* plausible general notion of analytical levels crops up.

A general account of levels in science, which preserves the broad picture that the mereological conception is out to depict but that avoids its liabilities, undergirds intellectual commentary on some of the most central metaphysical and methodological controversies which modern science occasions, not the least of which are those that swirl around discussions of reduction. Those discussions include both scholarly debates and more popular disputations about the perennial philosophical and social issues that prospects for scientific reductions incite. The mechanists are unquestionably right that in each case the details matter, but that need not rule out the search for ways to talk more carefully about those larger issues (see Rosenberg, 2006, p. 40).

Two criteria help to illuminate a general notion of analytical levels in science. Their virtues are their comparative independence from one another and their convergence on the standard distinctions and arrangements among the major families of sciences, at least.

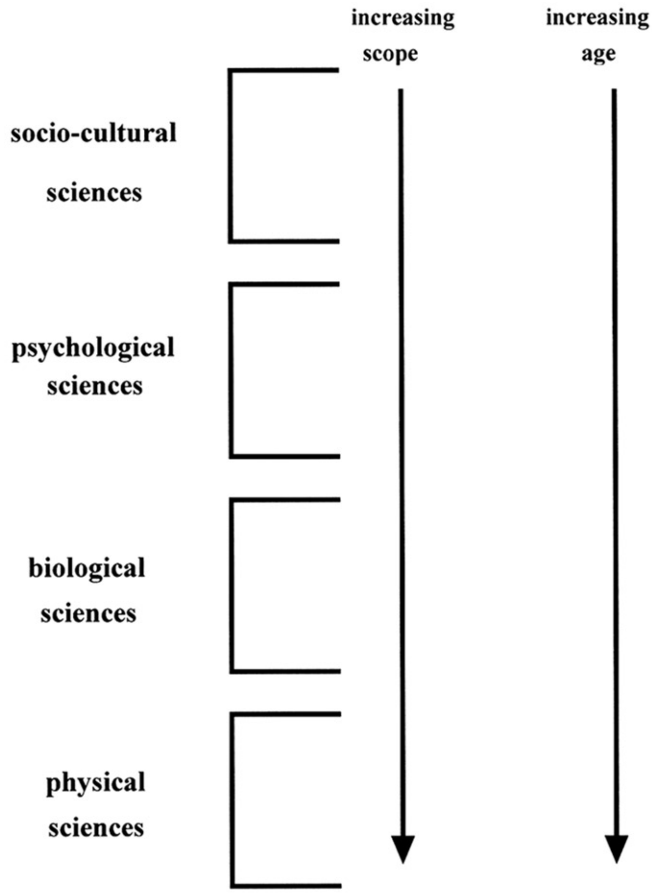


Figure 1 Families of sciences: two criteria for distinguishing levels of analysis in science.

Instead of focusing on the scale of the objects a science studies, the first criterion looks to a science's comparative explanatory *scope*. The range of phenomena to which lower level sciences attend is always greater than the ranges that fall within the purview of higher level sciences. All of the entities, systems, and events studied at higher levels are describable at lower levels, but the opposite is not true. Subatomic particles, discussed in physics, are the building blocks of all other physical systems (from atoms, tectonic plates, and galaxies to kidneys, organisms, and social groups). The range of things a higher level family of sciences concentrates on constitutes a subset of those dealt with by families of sciences at lower levels (Abrahamsen, 1987, 1991). Thus, we accord minds to a subset of biological systems that possess the requisite wetware. Apes and dolphins seem to be in but oysters and earthworms are still out. This criterion delineates a salient respect in which lower level sciences are properly characterized as more fundamental. The more fundamental sciences possess resources for describing a wider range of phenomena.

The order of analytical levels also corresponds to the chronological *order in natural history* that various systems evolved. The lower a science's analytical level, the longer the things, to which it primarily attends, have existed. For example, the subatomic particles and atoms that are the principal objects of study in the basic physical sciences appeared quite soon after the Big Bang whereas the systems that the biological sciences scrutinize first began to appear (on Earth, at least) but a few billion years ago. Developed nervous systems, brains, and the minds that eventually seemed to have accompanied them, by contrast, look to be roughly two billion years newer. And, finally, cultural systems that the socio-cultural sciences investigate date from a few million years ago on the most optimistic estimates and, perhaps, no more than some tens of thousands of years ago on more demanding criteria.

Figure 1 summarizes how these two criteria organize the analytical levels of science. Even jointly, they hardly provide a compelling, definitive, or precise account of these matters.

This is only a preliminary sketch for rehabilitating something like the standard assumptions about how the sciences hang together. The hope is that it leaves room, at least tentatively, for:

(a) examining recurring concerns about reduction in both scholarly and popular discussions,
(b) differentiating successional from cross-scientific settings (and, hence, for sustaining both the first, long-standing objection to New Wave reduction as well as new concerns that section 3 addresses), and
(c) advancing a broader explanatory pluralism for framing some of the investigative morals underlying the mechanists' approach.

## 3. Cross-Scientific Relations, Scientific Research, and Diachronic Theorizing about Long Term Processes

When addressing cross-scientific relations all-purpose models of reduction are remiss in other respects. The first, long-standing objection faulted them for providing no means for characterizing the distinctive dynamics and implications of intertheoretic relations in cross-scientific settings. That gap points to others. The logical empiricist and New Wave models of reduction are less well-suited to accommodate an entire class of scientific projects and theories and, consequently, offer little insight about any cross-scientific comparisons involving them.

This is a function both of what these models do and of what they do not do. First, *their fixation* on the translation of theories' explanatory principles and on the *structural* relations between the things that theories talk about (i.e., what these models do) and, second, *their neglect* of scientists' opportunism with regard to sources of evidence available at alternative levels of analysis (i.e., what they do not do) both hobble them when cross-scientific comparisons require coping with research and diachronic theorizing about long term processes concerning distributed, large scale systems. This section explores some of the omissions associated with what they do. The next section will briefly review an example of cross-scientific research at the borderlines between psychology and neuroscience to illustrate what the traditional and New Wave reductionist models do not do.

Both forms of integrative research in science, reductionist and contextualist, prompt explorations into diachronic matters. Modern reductionists and the philosophers of mind they have inspired have headlined the compositional relationships between things. Reduction looks downstairs, decomposing a system into its parts. Tracing the spatial relations and the connections among those parts can provide a richer understanding of the behaviors the system exhibits. This is the essence of reductionism.

As the mechanists' work illustrates, explicating the organization of a system's parts is also one of the most effective ways to gain additional analytical purchase on the short-term diachronic processes in which those parts figure. At biological levels and higher, where selection may operate, scientists include functional analysis among their research tools. This helps them pinpoint a system's integral operations and better understand its organization. The mechanists have illustrated repeatedly[9] how scientists improve and refine their hypotheses about mechanisms on the basis of an on-going interplay between structural and functional analyses—typically, across multiple (local) levels of organization. They have demonstrated how learning more about a system's structure provides clues about how its parts function and how learning more about the functions of the processes in which those parts play a role can spotlight structural details that might have otherwise gone unnoticed. The mechanists' discussions of complicated biological processes, such as the Krebs cycle (Bechtel, 2006), show how working out systems' structures, functions, and operations are mutually supportive inquiries.

Pluralists grant equal billing (to that which reductionists accord compositional factors in scientific explanations) not only to organizational factors but to contextual considerations as well. In many cross-scientific settings, scientists just as readily look upstairs, exploring the targeted item's place and role in more encompassing systems. They examine the item's spatial relations and interactions with other items in its environment, and they can explore the contributions it makes to the characteristic patterns larger systems display. So too, then, do contextualist inquiries—again, especially those where selection may pertain—encourage investigations into *changes* in systems, i.e., investigations of those systems' short term operations.

The crucial point here is that any account of analytical levels in science should include some means for portraying a further mode of analysis covering scientists' hypotheses about systems' *diachronic* features. In the account I am outlining (here and in McCauley, 2007b), this will require the addition, in figure 2, of a third dimension to the picture of the families of sciences portrayed in figure 1.[10]

Figure 2 includes, for each of the families of the sciences, two (transparent) panels for representing distinguishable modes of scientific modeling and research. For each family the frame in front represents a logical space to situate sciences (e.g., anatomy) or models (e.g., the Watson-Crick, 1953, model of the architecture of DNA) that focus on things' structures, while the second frame accommodates scientists'

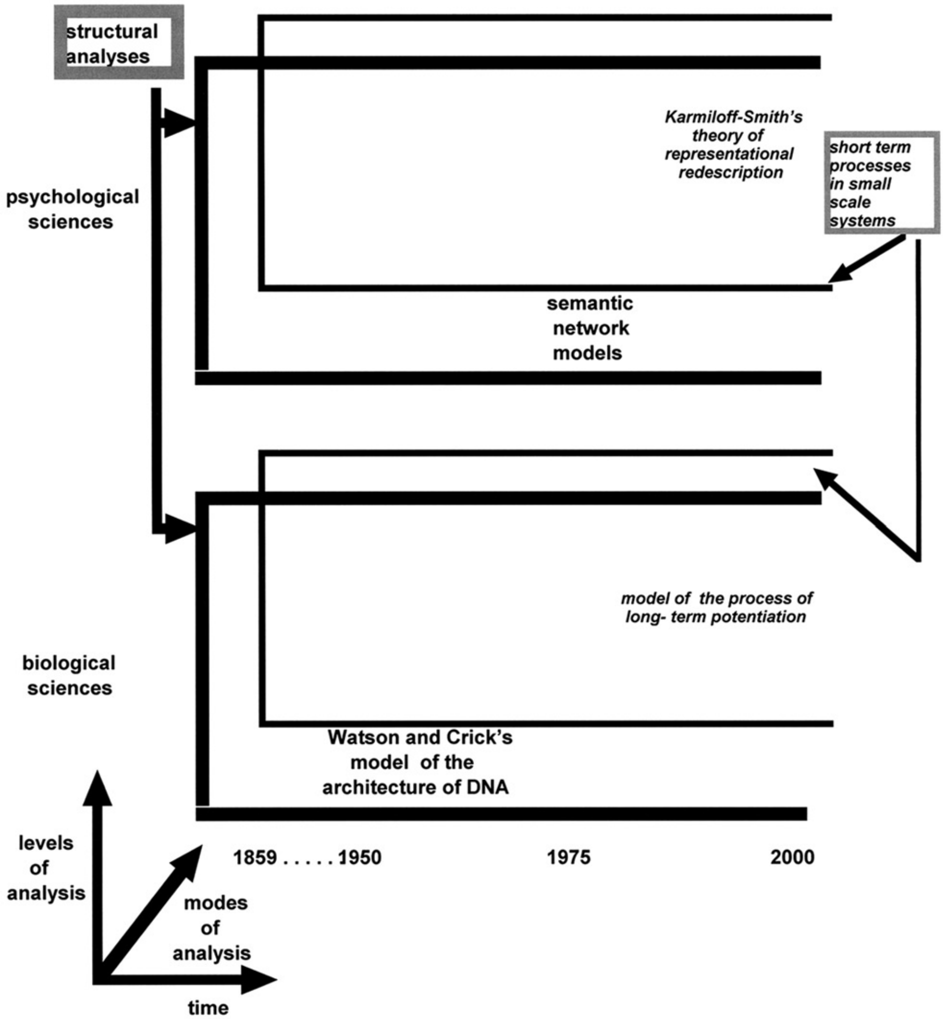


Figure 2 Panels for representing structural and short term diachronic analyses.

accounts of systems' diachronic features (e.g., the model of the processes that make up long-term potentiation). At the psychological level, semantic network models from the 1970s would constitute an example of the former, while Annette Karmiloff-Smith's (1992) theory of children's cognitive development in terms of representational redescription is an illustration of the latter. Even the most casual perusal of scientific work on biological or psychological mechanisms will instantly reveal the integral relations between models of those mechanisms' structures and models of their operations. Those semantic networks, for example, were presumed to involve processes of spreading activation to explain priming effects, lexical access, word associations, and more (Neely, 1977). Or consider neurocomputational models in cognitive neuroscience. See, for example, Mark Johnson's discussion of efforts at modeling imprinting in chicks (2005, pp. 99–103). Scientists' diagrams of mechanisms routinely include information about *both* structures and processes.

Nonetheless, the distinction between two analytical modes—one concerned with structural considerations and one concerned with short term processes—is not wholly abstract. Occasionally, its import is evident in practice. Sometimes, especially at early stages of research on complex systems, diagrams represent structures only (Bechtel, 2006, figure 3.3 on p. 78 and figure 6.3 on p. 205; Jardine, 2000, especially chapters 2, 6, and 7). Korbinian Brodmann's famous cytoarchitectural map of cerebral cortex is a classic example from the history of neuroscience (Johnson, 2005, p. 33). On the other hand, scientists sometimes have a clear view of functions that must be getting accomplished but less knowledge about the requisite structures. Examples include early research on enzymes in the middle of the twentieth century (Bechtel, 2006) and much research on genes, at least until they have been sequenced, even today. Arguably, though, in most cases of this latter sort, scientists carry out these inquiries (about the unknown structures that realize the targeted functions), possessing knowledge about the larger encompassing systems (whether they are cells, tissues, organs or systems or brains, organisms, populations, or groups). Consequently, representations (whether visual, verbal, or both) of processes only, making no references to structures (e.g., Wellman, 1990, figure 4.2 on p. 109), seem more the exception than the rule. If so, that pattern may offer some support for the reductionists' core intuition that, at least cognitively, structure is primary.

In cross-scientific settings traditional and New Wave models of reduction overwhelmingly concentrate on the *structural* relations both of theories' explanatory principles and of the things that they describe, because, in accordance with that core intuition, those are what they concentrate on in every intertheoretic setting. Mapping the structures of theories and things across analytical levels *is* one of the pivotal activities of scientists in cross-scientific contexts, but it does not exhaust the epistemologically and (for philosophical naturalists) ontologically significant connections that can arise between scientific enterprises. The new complaint to be developed here, then, is that with their primary focus on the structural relations of explanatory principles and of the things those principles discuss, these models of reduction are not well-suited for representing cross-scientific connections that involve theories about diachronic matters.

The problems on this front go deeper than these initial observations suggest, for not all scientific speculations about diachronic matters are so closely wedded to the specification of the short term operations of readily localizable mechanisms. Even in the various examples that the mechanists have proffered, the scale of the mechanisms is much smaller, their integration is much tighter, and the time frames of their operations are far shorter than those that evolutionary theory countenances (McCauley, 2007b). On this score, it is not much of an exaggeration to say that Darwin changed everything. Darwin's (1859/1979) theory of evolution by natural selection addresses *substantially longer term processes* that are *probabilistic* in character and that are realized, in part, by substantially larger theoretical entities, which are often *distributed* across enormous numbers of individual organisms, who sometimes inhabit an array of *different environments*. In the middle of the nineteenth century Darwin introduced a form of theorizing about biological processes that presumes such huge expanses of time that, to this day, worries persist in some quarters about whether there has, in fact, been time enough. Moreover, the elaboration of the central process Darwin's theory proposes involves, among other things, the probabilities of countless spatially, temporally, and causally discontinuous events bearing on the fates of those individual organisms and, thereby, on the fates of the large scale, distributed entities of which they are members.

Adding any more panels in the third dimension in the figures representing analytical levels in science is inevitably somewhat arbitrary. After all, in cosmology and astrophysics, the physical sciences study matters involving time frames that are an order of magnitude greater than the longest time frames that evolutionary biology addresses. So, describing this new panel in terms of "long term processes" is purposely vague. The motive for introducing it is simply to clarify that virtually every science, eventually, includes theorizing and research about processes whose durations are *many* orders of magnitude greater than those that are characteristic of the sort of isolatable, integrated biological (or psychological) mechanisms, on which the mechanists' analyses have focused. Moreover, those long term processes do not enjoy the same sort of intimate tie between structures and operations, i.e., between structures and short term processes, that those isolatable, integrated biological mechanisms do. Figure 3,[11] then, includes, for each family of the sciences, an additional panel representing this further mode of analysis.

This third analytical mode deals with comparatively longer term processes, which, at least in the case of the theory of evolution, involves large scale systems. The forging of the neo-Darwinian synthesis is an example of such theorizing and research in recent biological science. With the notable exception of William James (1890/1962), psychologists spent more than a century focused mostly on evidence (whether about minds, brains, or behavior) concerning changes over relatively short durations that rarely even extended to the length of the normal human life span. As with many other sciences, it has taken far longer for diachronic theorizing about the forces that impinge over extensive time frames to surface in a more systematic fashion. It is only in the last few decades that such theorizing at the psychological level has reemerged within the new sub-discipline of evolutionary psychology. Evolutionary psychologists submit hypotheses about the *structure* of the *modern* human mind, based on their conjectures about likely selection pressures that would have shaped the human mind in our species' environments of evolutionary adaptiveness. The evolutionary psychologists maintain that experimental probing of contemporary human behavior and mental life should yield evidence for these conjectures.

The consideration of long-term diachronic processes in large scale systems will call for some customizing of the mechanists' analyses to manage, for example, massive, distributed, historical entities, such as groups and lineages (Barros, 2008). Nor do the varied operations of the mechanism of natural selection appear to involve either the stable, configurations of integrated entities or the regularities of processes and interactions that the mechanists' analyses have tended to highlight. Still, their

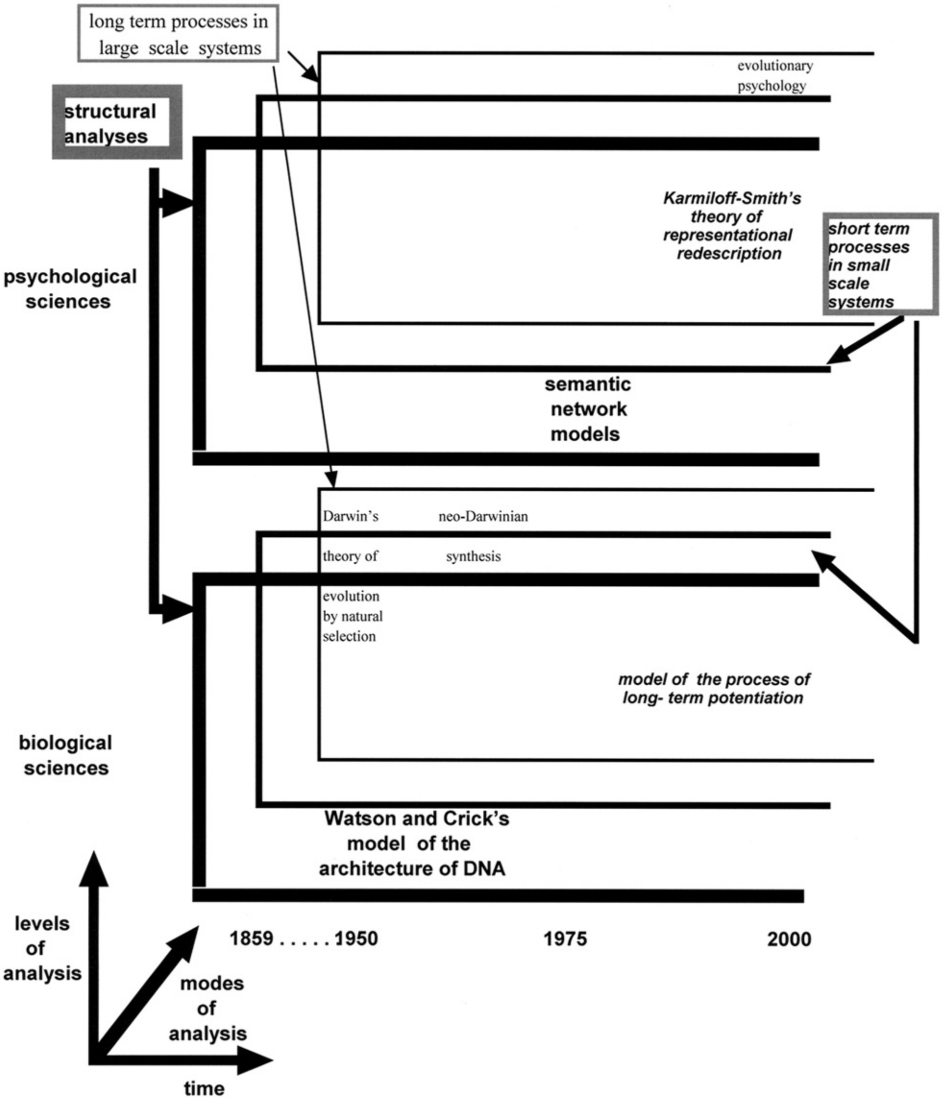


Figure 3 Panels for representing structural and both short term and long term diachronic analyses.

accounts straightforwardly allow for the decisive role that ecological considerations can play in natural selection and nothing in principle bars mechanists from elaborating their models to accommodate the longer time frames, the distributed entities, or the diverse stochastic processes that the theory of evolution by natural selection introduces (Skipper & Millstein, 2005, especially p. 345).

By contrast, managing those ecological contributions, diverse stochastic processes, vastly longer time frames, and, especially, those distributed entities that evolutionary theory addresses (where structures, functions, and operations are not so closely wedded either causally or temporally or spatially) poses more formidable problems for traditional and New Wave models of reductionism. Neither traditional nor New Wave models of reduction were built with managing diachronic theories and their cross-scientific relations in mind (however, see Rosenberg, 2006).[12] They are not well-suited to deal with diachronic theories about distributed, historical entities such as populations and lineages, whose structures are diffuse, at best. Furthermore, collating and responsibly generalizing about the countless phenomena and processes—genetic, developmental, organismic, and ecological (at least)—that regularly play a role in the process of natural selection, let alone achieving even vaguely plausible translations at some lower analytical level of the variety of (stochastic) theoretical principles implicated[13] is a task whose practical challenges are colossal. These limitations acquire some urgency in the face of the renewed interest in appropriating evolutionary thinking in the psychological and social sciences over the past few decades (Boyd & Richerson, 2005; Buss, 2005).

## 4. How Psychological Theorizing about Long Term, Diachronic Processes Has Inspired (1) New Empirical Findings and a Reinterpretation of Old Findings Concerned with Short Term Cognitive Processing and (2) a Productive Collaboration with Research about Brain Structure and Function

This section examines critical features of cross-scientific relations that traditional and New Wave models of reduction neglect, i.e., it discloses some of the things that they do not do. The complaint is that these models give no attention to how cross-scientific interactions involving diachronic theories can provoke research and generate new bodies of evidence that enrich scientific research about systems' structures, including those at lower levels. I will briefly scout a familiar diachronic proposal in the literature of evolutionary psychology concerning long term processes and some of its consequences for theorizing and research about the structure of the modern mind, about short term cognitive processing, and about brain structure and function.

I do not mean to imply here that either evolutionary psychology generally or Leda Cosmides and John Tooby's (1992) take on the matters in question are definitive or uncontroversial (hardly!). Critics abound (for example, Buller, 2005, Mundale, 2003, and Richardson, 2007b), and dissent, in particular, about Cosmides and Tooby's account of hypothetical reasoning has had strong legs for more than a decade (Buller, 2005, pp. 160–190; Fodor, 2000; Sperber, Cara, & Girotto, 1995). My aim is only to show how diachronic proposals about long term processes have implications not only for theorizing and research on psychological structures and short term processes but for conjectures about structures and functions at the biological level too. Such interplay does not settle the theoretical or empirical disputes, but such cross-scientific transactions can drive new experimental findings that serve as *evidence* pertaining to the theory of long term processes that motivated them and to speculations advanced in other analytical modes and at other analytical levels.

The famous implication of evolutionary psychologists' speculations for other *modes* of psychological theorizing is their contention that the structure of the modern human mind is massively modular. They hold that the human mind contains scores, perhaps hundreds, of domain specific systems, which evolved to manage particular challenges to our ancestors' survival in Pleistocene environments. Ascertaining what those challenges were will enable psychologists to compile an inventory of candidate modularized capacities, and, by now, evolutionary psychologists have looked at a few dozen (Barkow, Cosmides, & Tooby, 1992; Buss, 2005). They concern topics as various as the basic physics of solid objects, face recognition, language acquisition, contamination avoidance, mate preferences, sexual jealousy, and discriminative parental solicitude.

Since biological evolution is so much slower than cultural and technological change, evolutionary psychologists argue that contemporary humans manage the modern world equipped with brains that evolved to solve the salient problems affecting human survival during our species' prehistory. (They maintain, for example, that people are more likely to be vigilant or manifest phobias about dangerous things in ancestral environments, such as snakes or spiders, than about dangers that they are far more likely to confront, such as automobile accidents.) Evolutionary psychologists maintain that one consequence of this disparity between the rates of biological and cultural change is that they can test their hypotheses about those putative adaptations to the prehistoric world by examining the structure and functioning of contemporary human minds. Over the past few thousand years of radical cultural change, our brains have altered little, if at all. Consequently, in experiments probing short term cognitive processing, evolutionary psychologists can test their diachronic hypotheses about long term processes, i.e., about the mind's evolution.

Cosmides and Tooby hold that the mind's mental modules include dedicated cognitive machinery for managing social understandings about distributing benefits and costs within a group. Archaeologists (Mithen, 1996), anthropologists (Dunbar, 1996), and psychologists (Tomasello, 1999) are unanimous in their conviction that one of, if not, *the* most important trait distinguishing the members of *Homo sapiens* from other primates is their *social* sophistication. The most elaborate human social groups dwarf other primate groupings in both size and complexity.

The benefits of social cooperation (security, access to resources, etc.) for group members compensate individuals for the considerable costs (military service, taxes, etc.) such arrangements exact. Their social sophistication makes it easy for humans to exploit such arrangements. Because humans understand how minds work, they know how to deceive and create false impressions. Free riders are persons who receive benefits, but who, typically through some deception, avoid costs. If such a strategy becomes widespread, the benefits of belonging will vanish and the coalition will crumble. Since human coalitions arise and persist, but since humans are also eminently capable of cheating, Cosmides and Tooby hypothesize that group success must turn, in part, on people possessing acute abilities to reason about conduct that enables them to detect cheaters. Natural selection would tend to purge individuals who are bad at cheater detection and to favor individuals who are good at it.

In accordance with their hypothesis, Cosmides and Tooby (1992) looked at human reasoning with such social problems. They employed a classical task from experimental cognitive psychology, viz., the Wason (1966) four-card selection task. In its original form, the task specifies that each of four cards has a number on one side and a letter on the other; however, the subjects can only see one side of each card. The subjects are asked to assess the truth of a conditional rule that putatively describes the relationship between the numbers and letters on the opposite sides of the cards. For example, "if a card has a vowel on one side, then it has an even number on the other." Subjects must specify how many and which of the four cards before them would need to be turned over (to see what was on the back) in order to ascertain whether or not the rule was true. From a logical point of view, the four cards collectively constitute instances of affirming and denying each of the conditional's antecedent and consequent. So, in the example, at hand, one card has a vowel showing (affirming the antecedent), a second card has a consonant showing (denying the antecedent), a third card has an even number showing (affirming the consequent), and a fourth card has an odd number showing (denying the consequent). Thus, the correct solution is to turn over the card affirming the antecedent, in accord with the valid inference rule, *modus ponens*, and the card denying the consequent, in accord with the valid inference rule, *modus tollens*, and not to turn over either of the other two cards. That is because (with only these premises available) what is on their opposite sides is irrelevant to ascertaining the truth of the conditional rule. No matter what was inscribed there, they could not falsify the conditional rule in question.

The selection task is famous because most subjects (usually seventy to eighty percent) give various incorrect responses, even though it involves the most elementary rules pertaining to conditional inference. The difficulty most subjects have with the task puzzled psychologists, who offered hypotheses about subjects' unimpressive performance. Wason found, for example, that subjects did better when the problem was posed in terms of the proper postage that should go on the front of an envelope on the basis of whether or not it was sealed at the back (Wason & Johnson-Laird, 1972). This seemed to suggest that in earlier formulations of the problem the difficulty resided in the peculiarity of the materials, yet subsequent research with other familiar materials did not boost subjects' performance. After more than a decade of experimental tinkering with the selection task, to little or no avail, it seemed clear that hypothetical inference was substantially more difficult than psychologists expected.

Enter Cosmides and Tooby (1992). By formulating the selection task as a problem about detecting cheaters, they found that subjects' performance greatly improved. Between sixty-five and eighty percent of subjects performed successfully, if they faced problems like finding out whether the patrons of some establishment were transgressing a conditional rule about the minimum drinking age. They also provided striking evidence that when stated as a problem about cheater detection, the familiarity of the materials in the selection task did not matter. Their subjects exhibited comparably improved performance on problems that Cosmides and Tooby had constructed about exotic social rules among fictional groups.[14] They obtained negative findings about a variety of alternative variables that might explain subjects' elevated performance in cheater detection cases.

Cosmides and Tooby provide considerable evidence of subjects' vastly improved performance on the selection task when the abstract logical problem is cast as a problem about cheater detection. These experiments on short term cognitive processing constitute tests of their hypotheses about the evolutionary origins and the (modular) structure of the underlying cognitive system putatively responsible for these findings. My point is not to champion these as compelling evidence for Cosmides and Tooby's hypotheses but rather to make the far more modest philosophical observation that their diachronic theorizing about long term processes at the psychological level has had a variety of noteworthy consequences for psychological theories, research, and findings bearing on short-term processes and on models of the mind's structure. Perhaps most prominently, their experimental work has occasioned a reevaluation of two decades of findings on the selection task.

Some types of evidence are more convincing than others. Advocates of mental modules have regarded selective deficits in performance, with respect to some targeted domain, as an especially persuasive type of evidence for its modular infrastructure (Fodor, 1983). Such deficits can result from congenital defects, injury, disease, or stroke. Researchers now utilize imaging technologies to look (downstairs at the neural level) for corroborating evidence about characteristic abnormalities in the structures and functioning of patients' brains. Valerie Stone, Cosmides, Tooby, and their colleagues (2002) have undertaken such cross-scientific research as a further avenue for testing their hypothesis about the evolution of a cognitive module dedicated to reasoning about social obligations regarding costs and benefits. They provide experimental evidence for selective impairment in reasoning about social exchange in a patient with bilateral limbic system damage.

In a bicycle accident in 1974 the patient, R.M., suffered bilateral damage to medial orbitofrontal cortex and anterior temporal cortex. The latter was sufficiently severe to in effect disconnect R.M.'s amygdala from the right and left anterior temporal poles, which are its principal sources of input. Although he has retrograde amnesia, R.M. scores in the normal range on a variety of psychological measures, including tests of intelligence, verbal fluency, and visuospatial function. Other than his amnesia, his most obvious impairment was that he "had pronounced difficulty with social intelligence" and "sometimes found social interactions puzzling" (Stone et al., 2002, p. 11533).

Stone and colleagues tested R.M. on a battery of conditional reasoning problems formulated as versions of the selection task. The problems were of three sorts, viz., conditional inferences dealing with social contracts, hazard precautions, and generic materials. The hazard precaution conditionals were of the form: "if you engage in hazardous activity X, then you must take precaution Y'' (Stone et al., 2002, p. 11531). The experiment included hazard precautions for two reasons. First, as evolutionary psychologists predicted, this is another domain in which normal subjects show substantially elevated performance on selection task problems, relative to their performance on problems about generic materials. Second, although it shares important properties with social contracts (both are deontic and involve utilities), hazard precaution, according to the evolutionary psychologists, should be a *separate, dissociable module*, since it would have resulted from different selection pressures. Stone and colleagues further argue that brain damage that would degrade performance on one form of hypothetical reasoning, especially one that shares so much in common with some other form of hypothetical reasoning that is not degraded, would count against alternative proposals that all such forms of reasoning depend upon a single, general-purpose system. Dissociations suggest that more than one system is in play.

Comparing R.M.'s performance on the sixty-five problems with the performance of thirty-seven normal subjects offers evidence that supports these claims. Unlike normal subjects, who performed well on problems concerned with social contracts and on problems concerned with hazard precautions, R.M.'s performance on the two tasks differed substantially. Although he performed at the same level (around seventy percent correct) as the normal subjects on the problems involving hazard precautions, R.M. did much worse than they (thirty-nine versus seventy percent correct) on the social contract problems. These findings are consistent with the character of R.M.'s everyday experience. Stone and colleagues report, for example, that ''his family has said that he does not realize if someone is taking advantage of him'' (2002, p. 11534).

To find if R.M.'s impaired performance with social contract inferences was not simply due to his copious bi-lateral damage, the researchers tested two other patients, B.G. and R.B., on the same tasks. By contrast with R.M., these two patients, like normal subjects, performed equally well on both the social contract and hazard precaution problems. This is the point where attention to *structural information at the neuroscientific level* enters. The experimenters used findings from neuro-imaging about the three patients' structural damage to help disentangle the question of whether or not R.M.'s problems stem from the sheer size of his injury (which would not support the evolutionary psychologists' hypothesis) or from the specific character of his brain damage (which *is* consistent with their hypothesis).

Structural MRI scans of the brains of the three patients reveal that all three have extensive bilateral damage. Crucially, though, the scans also suggest that R.M.'s problems do not arise from the size of his brain injuries, for not only does R.B. have damage to a larger area of his brain than R.M., he both out-performed R.M. on the reasoning tasks and, unlike R.M., his performance on hazard precaution problems did not differ from his performance on social contract problems. B.G.'s injuries are not as extensive as R.M.'s. He too out-performed R.M. and, like R.B. (and normal subjects), he did not exhibit any differences in his performance on hazard precaution and social contract problems. Taken in combination with other studies in clinical neurology on social information processing deficits, these structural MRIs suggest that it is the *specific character* of R.M.'s injury that has impaired his abilities regarding social affairs. It seems that the important difference between R.M. and the other two patients is that he is the only one of the three with bilateral damage that has compromised *both* orbitofrontal cortex and the amygdala. R.B. has the first problem. B.G. has the second. Only R.M. has both, and among *all* of the subjects in the experiment, including the normals, R.M. is also the only one who performed significantly less well on the social contract problems than he did with hazard precautions.[15]

I do not wish to overstate the import of these results. As Stone et al. comment, "single cases are most useful for demonstrating dissociations, not for making strong claims about the function of the underlying lesion areas" (2002, p. 11534). I offer this brief summary to show, first, where diachronic theories about long term processes motivate new research. That the outcomes of such research has *any* evidential import for subsequent inquiry about brain structure and function, about short term cognitive processing, or about the structure and evolution of the human mind, should suffice to make the case that a satisfactory philosophical model of cross-scientific relations should possess resources for accommodating such scientific endeavors. In pursuit of evidence bearing on Cosmides and Tooby's hypothesis about an evolved, task specific, cognitive capacity, this cross-scientific collaboration explored the connections between the structural irregularities of the three patients' brains and the various subjects' performance on reasoning tasks. It yielded evidence that future researchers would be imprudent to ignore, which pertains not only to the diachronic theory about the evolution of the mind that inspired it but also *to conjectures about the structure and functioning of psychological and neural mechanisms*.

A final note: the targets of my critical arguments have been all-purpose models of intertheoretic relations—specifically, traditional and New Wave models of reduction. In section 3, I faulted them for neglecting diachronic theories about long term processes. I argued there that since they neglect such theories, they have inevitably neglected those theories' epistemologically significant contributions to cross-scientific settings. This section has highlighted contributions of diachronic theorizing in cross-scientific contexts. Because I have been interested in making a constructive case for the ability of explanatory pluralism to accommodate these features of cross-scientific settings, I have not developed my first, more fundamental complaint in section 3 at greater length, i.e., the complaint that traditional and New Wave models neglect diachronic theories about long term processes overall. Behind that complaint stands a further concern about the models of ruthless reductionists. Expressing enthusiasm for structural explanations cast at the level of cellular and molecular neuroscience, they might remain unmoved by the illustration above concerning the cross-scientific impact of psychologists' speculations about the evolution of the mind. Bickle (2007, p. 292), at least, holds that *all* psychological explanations are "*essentially heuristic*" and do not contribute "ultimately" to what he regards as the "final" explanations of psychological phenomena at the molecular level in neuroscience. But unless they also wish to contest what I assume is one of this paper's much less controversial claims, viz., that diachronic theorizing and research about long term processes interacts productively with research about structural, functional, and short term, operational matters *at the same analytical level*, then the fact that their models are no better prepared to examine the contributions of theories about the evolution of the *brain* (e.g., Kaas & Preuss, 2003) than they are to accommodate speculations about the evolution of the mind might occasion some unease about those models' adequacy.

## Acknowledgements

I wish to express my gratitude to Markus Eronen for his comments on a related presentation and to Charles Nussbaum and Max Kistler for their comments on this paper. I have benefited in countless ways from conversations with Bill Bechtel about the topics this paper raises. I, of course, am responsible for the problems that remain.

## Notes

[1] For discussions of the first of these problems, see McCauley (1996) and (2007a). For discussions of the second, see those and McCauley (1986).

[2] . . . and, in particular, not across the borders that separate the major families of sciences (see figure 1).

[3] Subsequently, the Churchlands have sometimes seemed to endorse a version of the psycho-physical identity theory (P.M. Churchland, 1986; P.S. Churchland, 1996).

[4] These include Bechtel (2006) and (2008), Bechtel and Richardson (1993), Craver (2001), Glennan (1996), (2002), and (in press), Machamer, Darden, and Craver (2000), and Wright and Bechtel (2007).

[5] Craver (2001, p. 66) offers a helpful diagram of the general relationships.

[6] When, in effect, explicating Nagel's cautions about heterogeneous reductions, New Wave reductionism highlights the variety of forms that bridge principles may take. Recognizing that the strength of the connections that bridge principles establish can vary widely and emphasizing that sometimes plausible principles might not be obtainable rank among the New Wavers' notable contributions. Barriers to bridging the theories' predicates substantially complicate the mapping of their explanatory principles and ontologies.

[7] Machamer, Darden, and Craver (2000, p. 13) seem to subscribe to something more like the traditional account.

[8] Their occasionally stark rhetoric about cross-scientific contexts, notwithstanding, the New Wavers often seem to concur with this (e.g., Bickle, 2003, pp. 116–117) and many of the other diagnoses that the mechanists and the explanatory pluralists advance (McCauley, 2007a, pp. 136–142).

[9] Examples include the mechanisms of fermentation, spatial memory, and the genetic control of development (Bechtel & Richardson, 1993), electro-chemical transmission at synapses (Machamer, Darden, & Craver, 2000), the circulatory system (Craver, 2001), and long-term potentiation (Craver, 2007). Bechtel (2006) provides detailed treatments of cell mechanisms whose discovery and articulation constituted the emergence of modern cell biology.

[10] Further designating that the horizontal dimension represents historical time will permit specific programs of research to be situated in this and the subsequent figure. Note, also, that figures 2 and 3 confine themselves to but two of the families of sciences, viz., the biological and the psychological.

[11] Note that the order of the back two panels in this figure is switched from the order in figure 9.2 on page 216 of McCauley (2007b). In figure 3 the panels proceed, from front to back, as follows: structural analyses (front), analyses of short term processes (middle), analyses of long term processes (back). This change signals the close practical connections between explications of systems' structures, functions, and short term operations.

[12] I do not mean to suggest that these reductionists have *no* resources, but such domains do constitute territories for which their models are less adequately outfitted. Standard reductionism offers a fruitful *analogy* for thinking about cross-scientific relations between diachronic theories and research. That strategy would show how understanding change at a higher level can be enhanced by looking at models of related changes at lower levels. Alexander Rosenberg (2006) proposes to reduce the operations of natural selection to the myriad relevant processes in molecular biology. So far as it goes, this analogical strategy is perfectly reasonable. As the pursuit of contextualist strategies and as the statistical tools scientists deploy suggest, however, differentiating and analyzing the relationships among component *processes* in complex systems (e.g., in ontogeny) are, typically, more complicated than differentiating and analyzing the relationships of component *structures* (chapter 3). That is probably why, in early stages of research, scientists are unhesitant about representing systems' structures, even though they lack much knowledge about their operations. See the discussion of early microscopy in Jardine (2000, chapter 3).

[13] Rosenberg (2006, chapter 5) argues against views of evolutionary theory that would impose this final task on Darwinian reductionists. He offers a notion of "ecological fitness" as a way for reductionists to circumvent this challenge, but that notion may face insurmountable measurement problems.

[14] Arguably, Wason's proper postage problem is about cheater detection.

[15] 15. This suggests that neural structures that might subserve any functionally identifiable system concerned with reasoning about social contracts are not going to be spatially localized.

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