

INTERTHEORETIC RELATIONS AND THE FUTURE OF PSYCHOLOGY*

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In the course of defending both a unified model of intertheoretic relations in science and scientific realism, Paul Churchland has attempted to reinvigorate eliminative materialism. Churchland's eliminativism operates on three claims:

- (1) that some intertheoretic contexts involve incommensurable theories,
- (2) that such contexts invariably require the elimination of one theory or the other, and
- (3) that the relation of psychology and neuroscience is just such a context.

I argue that a more detailed account of intertheoretic relations, which distinguishes between the relations that hold between successive theories at a particular level of analysis over time and those that hold between theories at different levels of analysis at the same time, offers grounds for denying Churchland's second and third claims and, therefore, undermines his eliminativism. The paper concludes by suggesting why it is, nonetheless, not unreasonable, given this more detailed model of intertheoretic relations, to expect the eventual elimination of common sense psychology.

I

Eliminative materialism has enjoyed a resurgence recently in the work of Paul Churchland (for example, 1979, 1981, 1984) who argues for that view in the course of defending both a unified model of intertheoretic relations and the minimalist metaphysics of a scientific realist. His arguments, no doubt, offer solace to many scientifically minded philosophers and philosophically minded scientists who are generally sympathetic to the eliminativist program but who have been unwilling to abide the recent anti-realism of its first generation defenders, Richard Rorty and Paul Feyerabend.

Churchland, by contrast, construes the pertinent issues as empirical through and through. His eliminativism is a direct consequence of his detailed analysis of intertheoretic relations in science, a plausible projection about future neuroscientific research, and his realist interpretation of

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scientific theories. According to Churchland, we will rid ourselves of the mental, because neuroscience will offer physical explanations of human activity superior to and thoroughly incommensurable with mentalistic psychological theories in general and our intentional folk psychology, in particular. A neuroscientific approach will replace our psychological theories (and their ontologies) without diminishing our ability to describe, predict, and explain.

Churchland's work explores with great insight the implications of work in the philosophy of language over the past two decades for an account of intertheoretic relations in science. It is on the basis of both this discussion and his scientific realism that Churchland argues for eliminative materialism. I will maintain, however, that although it is not at all unreasonable on at least one level to expect the eventual elimination of intentional psychology, Churchland's arguments fail to justify this expectation, because his analysis of intertheoretic relations in science is too coarse grained. Specifically, he gives insufficient attention to the combined role of considerations concerning levels of analysis and of certain temporal features of intertheoretic relations.

Churchland's emphasis is on the incommensurability of scientific theories as the grounds for an ontic housecleaning. This emphasis is in marked contrast to the traditional microreductive model of intertheoretic relations (for example, Nagel 1961) which holds that ontological economizing is the result of satisfying strict formal and empirical conditions which insure the theories' commensurability. One theory microreduces another when the principles of the latter follow as deductive consequences from the principles of the former with the aid of reduction functions that identify the entities (and their properties) in one theory with those in the other on the basis of part-whole relationships (see McCauley 1981). The reducing theory explains the reduced theory and, thus, in the process demonstrates its dispensability.¹

Eliminative materialists, on the other hand, look to Kuhn (1970) and Feyerabend (1962) who have emphasized the role of revolutionary change in science, where, because of the incommensurability of two theories, one simply *replaces* the other. That the two theories may use many of the same terms can obscure their disparity. In the history of science, shared terms typically prove more troublesome than those each theory employs uniquely, since in the different theories they often differ both intensionally and extensionally. This sort of condition often proves sufficient to short-circuit both the formal and empirical requirements of traditional microreduction. Thus, although both of these accounts of intertheoretic re-

¹Robert Causey (1977) has offered the most comprehensive account of the complexities involved here.

lations seek ontological deflation, they rely on analyses that are opposed on many important counts.

When two theories are so irreconcilable, sooner or later scientists simply abandon one or the other. The upheaval that follows this sort of theory replacement is comprehensive, often overthrowing the entire research tradition associated with the older theory—including its accompanying problems, methods, and ontology. Attending the new theory is a new research program whose specifics emerge as the theory develops—generating in its turn new problems, new research projects, and even new facts. (See Feyerabend 1962, pp. 28–29, 1975, pp. 67, 176–77.) Phlogiston theory is no longer with us, *not* because of its deductive and identity relations with the principles and ontology of modern chemical theory respectively, but rather because it proved incommensurable with the development of that superior theory. For the scientific realist, phlogiston theory is false and modern chemistry is true, so it is *impossible* for the former to follow from the latter deductively. (See Wimsatt 1976a, p. 218.)

Eliminative materialists construe the relationship of psychology and neuroscience along precisely these lines. Churchland is especially clear on this matter:

The eliminative materialist holds that the P-theory [psychological theory], not to put too fine an edge on the matter, is a *false* theory. Accordingly, when we finally manage to construct an adequate theory of our neurophysiological activity, that theory will simply displace its primitive precursor. The P-theory will be eliminated, as false theories are, and the familiar ontology of common-sense mental states will go the way of the Stoic pneumata, the alchemical essences, phlogiston, caloric, and the luminiferous aether. (1979, p. 114)

Among other things, this analysis defuses certain defenses of psychology which seek to demonstrate the impossibility of its microreduction (for example, Fodor 1975, pp. 1–26, 1981, pp. 146–74). It defuses them, because it both embraces their premises concerning the incommensurability of the theories in question *and* offers an alternative model of intertheoretic relations on which those premises suffice to justify the sort of full scale elimination of psychology from which its defenders hoped to protect it! Hence, arguments against the microreduction of psychological theories are superfluous, if theories in neuroscience can simply replace them.

A realist revival of eliminative materialism is particularly welcome on a number of counts, since eliminativism, in any of its forms, has enjoyed a number of advantages over other materialist accounts of mind and over the various forms of the identity theory in particular. For example, the identification of entities across theories is not at issue when one theory

replaces another. Eliminative materialism avoids all of the difficulties surrounding hypothetical identities in science (McCauley 1981). If neuroscience eliminates psychological theories and their accompanying ontological commitments, nothing will remain in the ontology of science to identify neural events with. (See Rorty 1970, p. 424.) These considerations motivate Rorty's characterization of eliminative materialism as the "disappearance form" of the identity theory (Rorty 1965). Nothing, however, *really* disappears, because, in Rorty's earlier view, there was nothing there to disappear in the first place and, in his more recent anti-realistic view, these and other questions which scientific realists pose simply reflect metaphysical confusions. (For example, see Rorty 1979, p. 239 or Rorty 1982, chapters 1, 3, and 5.)

Eliminative materialism also eludes objections to materialism based on the notion of a category mistake. (See, for example, Cornman 1962.) Our conceptual arsenal changes as science progresses. Old categories go the way of the unsuccessful theories that contain them. The category problems those old categories inspire should fall out of sight as quickly as those categories do, which for the scientific realist will be just as quickly as the theories do in which they are embedded. No conceptual consideration should preclude, a priori, the ability of neuroscientific talk to assume both the explanatory and reporting functions of psychological idioms.

Furthermore, these considerations also release the eliminative materialist from both "the ideology of common sense" (Feyerabend 1975, p. 164) and the additional conceptual objections it motivates (such as any of the recent forms of essentialism). Many scientific breakthroughs have come at the expense of common sense. In the face of theoretical progress in science no category is immune to revision. The popularity (or commonality) of common sense is no ground for according epistemic privilege to its categories,² despite the considerable conceptual inertia that the categories of common sense enjoy. "As well as reflecting the wisdom of our ancestors, language also reflects their muddles and mistakes" (Sutherland 1970, p. 104). Experimental work indicates that categories and theories that science has overthrown long ago, nonetheless, often retain a firm foothold in common sense. (See McCloskey 1983.) Typical subjects' mechanical intuitions, for example, often seem to be consistent with accounts unique to physical theories prominent in the late Middle Ages.

²Feyerabend (1962) argues:

No number of examples of usefulness of an idiom is ever sufficient to show that the idiom will have to be retained forever . . . conceptual changes may occur anywhere in the system that is employed at a certain time for the explanation of the properties of the world we live in. (p. 89)

The eliminative materialist holds that the exact same sort of confusions prevail in our commonsense understanding of persons (Churchland 1979, p. 5).

Of course, the persistence of both the ideology and its idioms in psychology (and in mechanics as well) would seem a source of potential embarrassment for eliminativists. Not only do we still employ the language of intention in psychology, but we still use the classical notions of space, time, and mass in our everyday dealings with the world. Rorty and Feyerabend (willingly) and Churchland (grudgingly) (1981, pp. 85–86) admit that mentalistic *talk* may be *so* socially entrenched that it may persist even after neuroscience can supersede it. Feyerabend suggests that we will eventually attribute physical connotations to mental terms (1962, p. 90). Rorty argues that if practicality is the only issue, then the eliminativist has already won the day. For once we admit that “at no greater cost than an inconvenient linguistic reform, we *could* drop such terms . . .” and that in this case “. . . ontological issues boil down to matters of talk,” we can justifiably conclude that “they cease to be ontological issues” (1965, p. 185; see also Rorty 1970, p. 424).

More recently, Rorty has adopted a more explicitly pragmatic form of eliminativism. He now advocates abandoning all “metaphysical comforts” (1982, p. 166), so that the sense in which “matters of talk” cease to concern ontological issues cuts *both* ways now. Neither common sense *nor neuroscience* have epistemic privilege. Rather they offer solutions to various problems human beings face—each of which they handle with greater or lesser effectiveness. Thus “vocabularies are useful or useless . . . they are not ‘more objective’ or ‘less objective’ nor more or less ‘scientific’” (1982, p. 203).³ For Rorty the mind-body distinction is merely a pragmatic response to the extraordinary complexity of our neural hardware. We would have abandoned it long ago (or, perhaps, never even have formulated it), if that hardware had only been more perspicuous. (See Rorty 1979, pp. 242–43.) The issue now for Rorty is exclusively a “matter of talk.” Rorty’s eliminative materialism in its present form is a rather bland consequence of his new (and much more controversial) brand of pragmatism, which “regards all vocabularies,” including that of science, “as tools for accomplishing purposes and *none* as representations of how things really are” (1982, p. xlvi). Thus the first generation eliminativists (Rorty, in particular) have, to a considerable extent, abandoned their scientific and realistic moorings in favor of arguments more “pragmatic” in character.

³More recently, Feyerabend has argued for similar views concerning the status of science (1978). I confine my comments here to the evolution of Rorty’s views, because he has been the more outspoken defender of eliminativism.

II

It is Rorty's (and Feyerabend's) apparent lack of sympathy for scientific realism (see Rorty 1979, pp. 274–84) which has alienated scientifically minded physicalists otherwise attracted to eliminative materialism. It is *that* audience, no doubt, that is most pleased to find Churchland's defense of eliminative materialism firmly grounded in his scientific realism.

Churchland's arguments for eliminative materialism differ on some counts from those of his predecessors. The first difference concerns the status of common sense and of common sense psychology, in particular. Churchland emphasizes that common sense is theoretical to the core and neither merely the repository of past wisdom, heterogeneous intuitions, and the surviving categories of debunked theories, nor the source of epistemically transparent insight. Consequently, he asserts that "the profound complacency most philosophers display concerning the status and/or the staying power of the common-sense conception of reality appears to me to be ill founded in the extreme" (1979, p. 43). With respect to human behavior specifically, Churchland claims that it is a *serious mistake* to construe the categorical framework of common sense psychology "as something *manifest* rather than as something conjectural." Theories in psychology (common sensical or otherwise) are no better than the solutions they offer for our empirical problems concerning human behavior. Churchland insists, therefore, that psychology⁴ ". . . has no special features that make it empirically invulnerable, no unique functions that make it irreplaceable, no special status of any kind whatsoever" (1981, p. 84). In fact, Churchland is convinced that all mentalistic or intentional psychology is just plain false.

The crucial point for now, though, is that once we recognize the theoretical character of intentional psychology, we should bring all of the criteria by which we evaluate theories to bear on this theory as well. Two considerations are particularly important, namely, the theory's relations with our best theories in contiguous and overlapping fields and the theory's overall ability to solve empirical problems in its own field. Generally speaking, a successful theory should organize a body of phenomena, provide insight into the causal and functional relationships which unify the domain, and, as a result, offer some empirical projections about

⁴Although Churchland directs his arguments primarily against what he calls, in his (1981), "folk psychology," it is clear that he takes his arguments to cut with equal force against theories in cognitive psychology as well. In his (1979) Churchland explicitly targets any theory which does not readily lend itself to traditional microreduction. Arguably, this includes virtually all psychological theories (including most forms of behaviorism). For Churchland, so far as science is concerned, both mentalism and intentionality are vicious. There are no benign forms.

similar events at other points in time. In addition, it should minimally cohere with what we know generally and, ideally, reinforce what we know about conceptually proximal areas in particular. (See Churchland 1981, pp. 72–73.)

Churchland's assessment of the relevant theories is unequivocal. He argues that our psychological theories (and our folk psychological theories in particular) are woefully deficient in their treatment of phenomena which are *paradigmatic on their own account* of things! According to Churchland, they offer unconvincing and superficial tales about many aspects of reasoning, emotion, perception, pathology, and (perhaps most importantly) learning (1979, pp. 114–15, 127–37). Such psychological theorizing has neither conquered new territory nor suggested new research. It is sprawling, imprecise, sterile, and even inconsistent at times. In short, “the story is one of retreat, infertility, and decadence” (1981, p. 74). Churchland implies that intentional psychology is the last surviving manifestation of an animistic view of nature inherited from our primitive ancestors and which we have, for the most part, abandoned over time. He holds, therefore, that its days, too, are numbered.

It comes as no surprise that Churchland is comparably pessimistic about the probability of successfully integrating common sense psychology with fertile theories in adjoining sciences. He does, however, think that the story is more complex than the one his predecessors told. The major concern of the eliminative materialist is “how the ontology of one theory (folk psychology) is, or is not, going to be related to the ontology of another (completed neuroscience)” (Churchland 1981, p. 72). Like Rorty and Feyerabend, Churchland thinks: (1) that all forms of intentional psychology will prove particularly recalcitrant to a smooth mapping onto theories in neuroscience, (2) that mapping theories onto one another is basically a special case of translating languages into one another, and (3) that the failure of neat translation in this case reveals a radical incommensurability between the two theories involved. Specifically for Churchland, it is a function of our inability to map those propositions of psychology which are semantically and systemically most important onto the claims of neuroscience. What Churchland denies, however, is that cases of radical incommensurability pose any serious threat to either scientific rationality or scientific realism—a denial based on his detailed analysis of intertheoretic relations.

Churchland argues that although intertheoretic relations in science span a wide variety of cases, they do fall on an identifiable continuum. Theoretical relations stretch from the radically incommensurable to the thoroughly continuous, with numerous positions in between. The point, quite simply, is that some intertheoretic relations are considerably more straightforward than others. Consequently, “we must be prepared to count

reducibility as a matter of degree. Like translation, which may be faithful or lame, reduction may be smooth, or bumpy, or anywhere in between" (1979, p. 84). The degree of reducibility is a direct function of (1) the number of the reduced theory's propositions that are important both semantically and systemically that we can map onto the propositions of the reducing theory and (2) the ease with which they can be mapped.

On this account of intertheoretic reduction a true theory may even be able to reduce a false one, if it maps the latter sufficiently well (for example, Churchland argues that relativity theory offers a sufficiently clean mapping of classical mechanics). The emphasis is on the preservation of a reasonably faithful image of the central claims of the reduced theory in the conceptual structure of the reducing theory, and this requires neither establishing deductive relations between the two, nor preserving either the truth of most of the reduced theory's claims or the truth of the reduced theory's most (observationally) basic claims. (See 1979, pp. 84–85.) Thus "a successful reduction is a fell-swoop proof of displaceability; and it succeeds by showing that the new theory contains as a substructure an equipotent image of the old" (1979, p. 82). Because the reducing theory does offer an account of the pertinent domain that is generally continuous with that of the reduced theory, and because we can straightforwardly ascertain the superiority of one theory over the other on the basis of traditional criteria for theory choice, such reductions require few, if any, revisions in our general background knowledge (see 1979, p. 82) and, thus, pose little threat to accounts of scientific rationality. Generally, the reducing theory not only does most, if not all, of the work of the reduced theory, but more as well. In addition, it is capable of outlining where and why the reduced theory's limitations appear. Here, the process of theory change is apparently more evolutionary, and it is not too difficult to trace the adaptations which inform the reducing theory's success. The adjustments are sufficiently small that the ontology of the old theory is reduced and, thereby approximately preserved, in the ontology of the new. (Although in the remainder of this paper I will occasionally employ evolutionary notions in the explication of intertheoretic relations, that should not be construed as an endorsement of any of the versions of evolutionary epistemology. The claims are *only* analogical and like all analogies do not carry over in all respects. It is not the tradition of evolutionary epistemology which inspires this talk, but rather the fact that evolutionary talk offers the most natural contrast to the well-entrenched tradition of *revolutionary* talk. It is in this contrastive sense that I mean to employ it.)

Typically, translations are least convincing where the belief systems of cultures radically differ. Analogously, when theories in science are radically incommensurable, they seem to describe different worlds. (See Kuhn 1970, chapter 10.) In such revolutionary contexts we are able to map very

few of a theory's important propositions onto those of its (eventual) successor, for example, in an attempt to map late medieval celestial mechanics onto a Keplerian (or Newtonian) view. The two theories are so disparate that all translations are bad translations to the extent that they fail to reproduce many of the most crucial claims of either theory in terms of the other. In periods of rapid change, partisans for competing views seem to talk past one another—not because they deal with different worlds, but because they employ widely divergent conceptual frameworks to describe the world with which we all must deal. In these contexts the two theories find little ground for agreement. They need not concur about the relative importance of problems, the boundaries between disciplines, the appropriateness of methods, the relevance of various observations, or even the range or interpretation of the facts to be explained. Thus, the standard criteria for theory choice are less helpful here because initially, at least, neither theory has the resources to encompass those of its competitor. When this happens whole theories and their ontologies replace one another. Decisions between the theories must rely on more considerations than just empirical tests, since the competing theories may profoundly disagree about the status of the empirical evidence.

Although Churchland is not particularly forthcoming at this point, he would, presumably, argue that revolutionary changes in science, also, need not impugn its rationality. (See Churchland 1979, sections 4 and 6.) Two or more scientific theories compete as accounts of some domain. Inevitably, over time they will differentially cohere with the rest of what we know and, thereby, garner more or less empirical support.⁵

Whether they result from evolutionary or revolutionary changes in science, our currently best theories deserve our ontological allegiances. Scientific realism for Churchland can be summarized as adherence to the tenet that “excellence in theory is the measure of ontology” (1979, p. 43). What our best science takes to be real is what *should* be taken to be real (because of all that stands behind the notion of “our best science”). Indeed, for Churchland, science provides the criteria of what *is* real. Churchland's eliminative materialism follows immediately: “the empirical virtues of the P-theory are sufficiently meagre that it is unreasonable to expect that it will reduce with sufficient smoothness to float an ontological reduction, and more reasonably to expect that it will simply be dropped, forsaken, as it were, for a prettier face” (1979, p. 115), namely, that of neuroscience. All the evidence indicates that neuroscience will *not*

⁵Whether political, social, and/or psychological biases underlie the emergence of one theory over another is the subject for students of particular historical episodes (see Maull, 1977, p. 151, note 19). Rarely, if ever, are the victories so abrupt *and* the incommensurabilities so radical that in the long run they overshadow a story told in terms of empirical considerations and scientific problem solving effectiveness.

offer a very faithful translation of intentional psychology, which is to say that we will not ask it to provide any translation at all. We will not reinterpret our psychological claims in the light of new neuroscientific findings; rather, we will eliminate them in favor of those neuroscientific findings.

III

Churchland's model of intertheoretic relations (to some extent anticipated in Schaffner 1967) offers a unified account of the two traditional strategies for ontological deflation in the philosophy of science. Rather than taking microreduction and revolutionary science as contradictory and mutually exclusive accounts of intertheoretic relations, he construes each as extreme points on a continuum of possible semantic relations between theories. He characterizes neither of these positions in such extreme terms as their champions have. Our best intertheoretic mappings are not as rigorous as the traditional microreductionists claimed, nor are our worst as completely discontinuous as Kuhn and Feyerabend seem to imply. Churchland's temperance enables him to develop a model of intertheoretic relations which is both more generally applicable and more detailed than theirs. In addition, he provides a relatively clear strategy for assessing particular cases in terms of our ability to plausibly map theoretically important sentences from one theory onto the other.

Churchland's model of intertheoretic relations suffices so far as it goes, but it does not go far enough. The basic story is even more complex than the one Churchland tells. Although I too anticipate the replacement of commonsense psychology, I disagree with Churchland about how that will be accomplished.

All eliminative materialists achieve their stark results on the basis of three assumptions. The first is virtually uncontroversial; namely, that the exploration of at least some intertheoretic contexts involves the comparison of substantially incommensurable theories. The second is that such contexts invariably require a nearly complete elimination of one theory or the other. (The successes of the superior theory entitle it to stake its conceptual claim on the disputed ontological territory.) Finally, the eliminativists' third assumption is that the relation of psychology and neuroscience is just such a context.

A more detailed account of intertheoretic contexts, however, offers grounds for denying the eliminativists' second and third assumptions. Although commonsense psychology and contemporary neuroscience *are* thoroughly discontinuous, that need not (and *in this case does not*) necessitate the elimination or replacement of one by the other. Although Churchland has properly emphasized the relative conceptual continuity of

theories, he and most other writers on intertheoretic relations⁶ have focused almost exclusively on the structural relations between both theories and their domains. This general trend is, certainly in part, the result of philosophers' traditional preoccupations with the logical structure of theories generally, the deductive relations that hold between theoretical principles, in particular, and the part-whole relations which hold between entities in theories' respective domains. An examination of the functional dimensions of intertheoretic relations, however, proves equally revealing. A discussion of these functional relations, though, awaits the examination of two important preliminaries, the notion of levels of analysis and the role of temporal considerations in theory comparison.

At some point virtually every discussion of intertheoretic relations presupposes distinctions between levels of analysis. It is certainly implicit in all microreduction talk, where the view is that a lower-level theory and its ontology reduce a higher-level theory and its ontology. Microreductionists hold that if we can exhaustively describe and predict upper-level (or macro-) entities, properties, and principles in terms of lower-level (or micro-) entities, properties, and principles, then we can reduce the former to the latter (and dispense with the upper-level analysis). The key to the notion of levels of analysis is a view (arguably necessary for doing science) of nature as organized into parts and wholes, at least, and, hence, that to some extent all entities are subject to componential analysis. (See Bechtel 1984.) Crucially, however, this does not mean that we must necessarily describe components structurally. Functional accounts not only suffice for many theoretical purposes in such fields as ecology and physiology, they are typically preferable when dealing with systems of substantial complexity. Corresponding to the simple hierarchy of levels of organization in nature is a similar, roughly hierarchical, arrangement of levels of analysis in science.

Broadly speaking, chemistry is a higher level of analysis than subatomic physics, since it concerns larger units and events which stand in causal relationships most economically described in chemical terms and to some extent susceptible to systematic analysis without reference to subatomic particles or principles. Again, broadly speaking, biology is an even higher level, and psychology higher than that. The altitude of a level of analysis is inversely proportional to the size of the domain of events with which it is concerned, so cell biology, for example, proceeds at a higher level of analysis than does biochemistry, since it deals with only a subset of the phenomena that the latter addresses. The altitude of a level of analysis is also directly proportional to the complexity of the systems with which

⁶Two important exceptions are Wimsatt (1976a, 1976b) and Nickles (1973). I am particularly indebted to the work of the former.

it deals. Thus, higher-level sciences deal with increasingly restricted ranges of events having to do with increasingly organized physical systems. Consequently, purely structural considerations less perspicuously distinguish levels the higher in the hierarchy we go. The higher levels generally deal with more complex systems and a wider range of variables. The behavior of such systems is more diverse. Different parts are functionally equivalent and the same part can serve more than one function. In such contexts functional ascriptions assume greater importance. Frequently, however, we can usefully idealize (in physiology, for example) many processes as the products of relatively closed, self-regulating functional systems operating at a particular level.

It is also worthwhile to distinguish between diachronic and synchronic features of intertheoretic relations. At times Churchland's treatment reflects his sensitivity on this point (1979, p. 81), at other times it does not (1979, p. 107). Most specifically, it is important to distinguish the relations that hold between successive theories at a single level of analysis over time as opposed to those between theories at different levels of analysis at the same time. The former, following Wimsatt (1976a), I will call intralevel or successional contexts, the latter, interlevel or microreductive⁷ contexts. Intralevel contexts include Kuhnian revolutionary situations (such as the relation of Chomsky's theories of language to his structuralist and behaviorist predecessors). On the other hand, the relation of genetics and biochemistry in the early 1950s is a particularly revealing illustration of the importance of interlevel relationships. These two sorts of contexts involve relations between theories that differ considerably on both functional *and structural* grounds. Consequently, unified models of reduction (like Nagel's [1961] or Schaffner's [1967]) and even unidimensional models (like Churchland's) oversimplify and therefore obscure the diversity of actual relationships between theories in science. (See Wimsatt 1976a, pp. 214–15.)

Churchland's model is helpful on a number of counts. It is not, however, a reliable guide concerning the sufficient conditions for theory replacement in science and it is, of course, precisely that goal of Churchland's model on which his and most other versions of eliminative materialism rest.

Churchland's continuum of theory commensurability captures an important dimension of intralevel relations. His model (in contrast to Feysabend's [1962], for example) preserves a systematic account of the conceptual and empirical proximity of classical and relativistic mechanics,

⁷Describing these contexts as microreductive is simply to aid in their identification. I will ultimately endorse few, if any, of the microreductionists' conclusions. (See notes 9 and 10 below.)

since it allows superior theories to reduce their predecessors at least some of the time, the falsity and mild incommensurability of these older theories notwithstanding. By eschewing the strong deductive and empirical requirements the microreductionists defended and to which others overreacted, Churchland offers a more subtle reading of the range of relevant cases. There is, however, more.

In intralevel contexts where the mapping between theories is reasonably good, the new theory typically *corrects* the old. It *explains the older theory* in the sense that it offers a principled account of when and why it fails. Kepler's laws enabled him to explain and predict the successes and failures of Copernican astronomical calculations that assumed that the heavenly bodies moved in circles. The new theory typically possesses greater precision, a wider domain, or both. It includes the accomplishments of its predecessor as special cases where certain parametric values fall within some range which does not confine the new theory. Within that restricted domain the old theory constitutes an approximation of the new and serves as an effective *heuristic of calculation*, sufficient, at least, for the purposes of engineers. (See Wimsatt 1976a, p. 174 and McCauley 1986.)

In *these* intralevel contexts where succeeding theories are, for the most part, continuous, we rarely, if ever, claim to have eliminated the older theory's ontology. Rather, terms and propositions undergo reinterpretations in light of their new positions in the conceptual framework of the new theory. These reinterpretations can have both intensional and extensional consequences. The continuity of the theories, though, is precisely a function of intensional and extensional overlap between the old and new. Generally, new theories retain old terms when possible. We have retained terms such as "planet," "evolution," and "gravity" and propositions about rectilinear inertial motion through numerous reinterpretations, because the effects of these reinterpretations taken individually were not especially severe. Most of the bodies that the ancients called "planets," for example, still are. (See Brown 1979, p. 118.) We can take some successive theories to be talking about at least some of the same things, to be making some of the same claims about those things, and to be offering explanations about some common explananda. So long as the relevant changes have reasonably local effects that do not destroy larger conceptual patterns, to claim that incommensurability seriously threatens theory comparison is to overstate the case.

Intralevel relations at the other end of Churchland's continuum—genuine, unanticipated (philosophically provocative) scientific revolutions—are more rare than the literature of the past two decades indicates. When substantial meaning changes and wholly new theoretical elements yield fundamental incompatibilities between theories, their conceptual patterns

fail to coincide. Whether these difficulties arise as the result of revolutionary developments *or* accumulate over a series of successive theories, they *do* undermine efforts to map some theories onto some of their successors. Where serious incommensurability arises in a single step, we may be unable to translate an old theory into its *immediate* successor. During such crises scientists decide to replace (and eliminate) one of the theories. Since they offer thoroughly incompatible accounts of at least much of the “same” phenomena, science cannot abide both for too long. If the challenger succeeds, it *explains the old theory away*. Here the superior theory eliminates its competitor (and its ontology) when it replaces it (see, for example, Brewer 1974). Whereas in intralevel contexts with little conceptual friction (and relatively straightforward mapping between theories) although new theories replace old theories, features of the older theory persist, and it can still serve as a calculating heuristic. In addition, a reasonably faithful image of much of its ontology endures in that of its successor.

All intralevel contexts *eventually* result in the total elimination of some theories. Over time in intralevel contexts incommensurability *increases* (and the goodness of mapping between older theories, separated by generations of successors, and the present reigning theory, therefore, inevitably *decreases*). Scientific revolutions block intertheoretic translation within a particular level of analysis, but so does scientific evolution, given enough time. (See Laudan 1977, p. 139.)

A simple illustration of this sort of evolutionary change may help. Arguably, part of Galilean dynamics is continuous with contemporary mechanics in the sense that the latter makes predictions which coincide reasonably well with Galileo’s law of free fall, so long as the fall is not too long and so long as it occurs relatively near to the surface of the earth. Galileo’s notion of natural circular motion, though, is of a piece with the mechanics of his neo-Aristotelian predecessors. At least one aspect of Galilean dynamics, then, maps onto contemporary theory while another maps at least as well on to late medieval theory, but little, if any, of late medieval dynamics maps continuously beyond the seventeenth century and much, if not most, does not survive Galilean innovations. (See Brown 1979, pp. 111–21.) The point, though, is that the revolutionary character of Galileo’s work was not *all* encompassing. When he could, he used traditional concepts as consistently as his new system would allow. It is not too difficult to locate the radical changes Galileo proposed and to trace their consequences. By the time Kepler had undermined the sanctity of circular motion, Descartes had suggested rectilinear inertial motion, and Newton had economically consolidated terrestrial and celestial mechanics and had completely *eliminated* the notion of natural motions, late medieval mechanics had not only been replaced, it had been eliminated.

Whether scientific revolutions (for example, the Copernican) or the cumulative effects of successive theories in a science (for example, in seventeenth- and eighteenth-century mechanics) preclude felicitous theoretical translations, such barriers inevitably arise as the sciences change and develop. With time, theoretical generations inevitably accumulate incongruencies in the mappings of successive theories at a level of analysis, until we can safely say that we have not only replaced some ancestral theories, but that we have eliminated all traces of some as well. Although this is surely not a principled consequence of scientific change, it is a real one.

Interlevel contexts, by contrast, involve no eliminations whatsoever. These are cross-scientific contexts where the goal is to associate theories that operate at different levels of analysis. Scientists explore whether their theories cohere with (and they hope are reinforced by) what is known at contiguous and overlapping levels.

The componential assumptions which underlie levels in science also inform scientists' interlevel concerns. Scientists look to other levels of analysis to gain new explanatory and problem solving angles on what is assumed to be the same phenomena under different descriptions. (See McCauley 1986.) Interlevel explorations establish ties between the concepts of different theories in the continuing process (1) of justifying the assumption that the theories share a common explanandum and (2) of stimulating further research.

Once theories at adjacent levels of analysis are sufficiently developed to address issues on the borders between the two sciences directly, they tend to *constrain one another's form*. The whole of science creates selection pressures on theories at any particular level. (See Wimsatt 1976a, pp. 231–36.) Nearly always, the reigning theories at immediately adjoining levels exert the most profound forces in the local conceptual space. They help to define one another's research problems. Here theories typically do not correct one another so much as they attempt to accommodate one another's demands. The constraints that physical chemistry and biochemistry exert upon one another with respect to accounting for the transfer of sodium ions across cell membranes offers a fitting illustration (see Robinson 1982).

The strongest sense of replacement appropriate in interlevel contexts, where theories map relatively neatly (along lines *approximating* those outlined in traditional microreductive models), is, *at best*, partial replaceability. A theory at one level, well integrated with theories at adjacent levels, can (ideally) do some of their work under certain special circumstances. Generally speaking, when relevant variables at upper levels are held constant, well-integrated lower-level theories ideally offer sufficient accounts of the phenomena for most of our explanatory and predictive

purposes (although, typically, at tremendous calculating expense), for example, in the relation of statistical mechanics and thermodynamics. Upper-level theories, on the other hand, provide the rationale for organizing what is, for all appearances, otherwise disparate lower level phenomena, for example, when physiological concerns inform extremely complex biochemical research.

The borders between levels populated by relatively immature theories and/or those between levels that, for intralevel reasons, have undergone substantial change, represent the other extreme on Churchland's continuum of theoretical continuity applied to interlevel cases. As the relations change within their own levels—a process, incidentally, which interlevel forces constrain, but which intralevel dynamics drive—they *may* become increasingly discontinuous with one another. In the absence of a plausible competing theory, interlevel forces are neither necessary nor sufficient to provoke changes in (let alone overthrow) an established theory at any given level. Consider, for example, the persistence of transformational grammars in linguistics in the face of important counterevidence in experimental psycholinguistics (see McCauley 1984). Interlevel forces only help to define what will count as a plausible alternative; on their own they are incapable of dethroning a reigning theory at some level.

As the translations between theories at different levels become more and more difficult, talk even of replaceability (let alone replacement) becomes less and less justified: “in interlevel reduction, the more difficult the translation becomes, the more *irreplaceable* the upper level theory is! It becomes the only practical way of handling the regularities it describes” (Wimsatt 1976a, p. 222). Interlevel contexts with considerable intertheoretic discontinuity, that is, high incommensurability, should be the least likely (of the four cases considered here) to involve either the replacement or elimination of theories and their ontologies. (See Figure 1.) Radical incommensurability in some intertheoretic contexts, namely, interlevel ones, neither requires the elimination of theories on principled grounds nor provokes such eliminations, in fact.

What lower-level theories (like upper-level theories) attempt to explain in interlevel contexts are the phenomena in question, *not* the upper-level theory. It is the phenomena, not the theories, which are (re)explained. The lower- and upper-level theories share an explanandum for which, in this case, they offer substantially incompatible accounts. They bring different conceptual resources to bear and, consequently, highlight different aspects of the phenomena in question. Nonetheless, intertheoretic excursions in interlevel contexts inevitably tend, over time, to *reduce* the incommensurability of neighboring, incompatible theories. Such interlevel moves are important *heuristics of discovery*. (See McCauley 1986 and Bechtel [forthcoming].) Scientists search for logically related conse-

quences of theories from contiguous levels. Logical conflicts suggest new tests which offer, for the winner, new sources of empirical support, and for the loser, direction for further research and possible adjustments. Specifically, scientists attempt to exploit the componential assumptions underlying the levels framework by postulating hypothetical identities between collections of parts (the entities of the lower level theories) and wholes (the entities of the upper level theories). These identity claims are hypothetical claims which suggest important avenues for empirical research (see McCauley 1981). These hypothetical identities are an important source of interlevel explorations' heuristic power as engines of discovery.⁸

Note that in these interlevel contexts the influences of theories on one another are not unidirectional (the pervasive reductionist biases of many philosophers and scientists notwithstanding). (See Allen 1983 for a discussion of some of cognitive psychology's influence on neuroscience.) Failures of translation "... carry with them no automatic presumption of upper-level guilt" (Wimsatt 1976a, p. 222). Whatever priority any theory deserves should not be grounded in our ontological prejudices, but rather in superior empirical performance. The absence of tidy interlevel

Degree of intertheoretic continuity (Churchland's continuum) Type of intertheoretic context	High (extensive intertheoretic mapping)	Low (little, if any, intertheoretic mapping)
Intralevel (same level over time) theories replace one another	scientific evolution 1. new theory explains (and corrects) immediate predecessor 2. eventual elimination of some earlier predecessors 3. recent predecessors as calculating heuristics	scientific revolutions 1. new theory explains immediate predecessor away 2. rapid elimination of predecessors 3. new paradigm
Interlevel (different levels at the same time) theories accommodate one another	microreductive contexts 1. increasingly unified explanation of phenomena where adjacent theories sharply constrain one another's form 2. partial replaceability of theories at adjacent levels 3. possible interlevel theories	explanatory pluralism 1. multiple, disparate explanations of phenomena 2. no replaceability of theories at adjacent levels 3. interlevel excursions as heuristic of discovery

Figure 1.

⁸See Darden and Maull (1977) on the identification of genes and chromosomes by Mendelian geneticists and cytologists in the early decades of this century.

translations does not constitute, a priori, a reason to accord explanatory and/or ontological priority to lower-level theories or their ontologies. As Laudan (1977, p. 56) has observed, “. . . noting . . . a logical inconsistency or a relation of non-reinforcement between two theories need *not* force scientists to abandon one, or the other, or both.” (See also pp. 50–54.)

Considerable conceptual discontinuity between theories at adjoining levels usually inspires empirical research. If each theory is modified to accommodate the new results, the theories will tend to become increasingly continuous conceptually. Interlevel situations in which theories at proximal levels, whose conceptual structures conflict only mildly, may occasion interlevel theories (Maull 1977). An interlevel theory exploits the descriptive and explanatory resources of theories from more than one level of analysis, for example, in molecular genetics. It incorporates information about causal relations and concepts from various theories in its hypotheses in order to provide a more integrated, informed, and comprehensive account of phenomena. These enhanced descriptive capabilities enable an interlevel theory to explore problems that lower-level theories alone cannot address. The theoretical resources of biochemistry, for example, are not sufficient to generate an account of the mechanisms of inheritance and development. They require guidance and supplementation from cytology and genetics. Molecular genetics has become the focus for such research.

None of these points preclude Churchland's realism; they only undermine *his* (and his predecessors') eliminativism. The relation of psychology (intentional, common sense, mentalistic, and/or cognitive) and neuroscience is one (as Churchland has emphasized) between *theories*. The crucial point is that *these theories operate at different levels of analysis*. Psychology and neuroscience offer fundamentally different accounts of a range of human activities. Psychological systems certainly depend upon neural systems (just as neural systems depend upon biochemical systems). The phenomena that neuroscience discusses are to some extent constitutive of the phenomena which psychology discusses, though as Fodor and others have noted, not in any simple way. Hence, like physiological systems, we typically approach the descriptions of the components of psychological systems functionally. Psychology and neuroscience employ different concepts and principles, and each highlights different aspects of the object of study. Each offers accounts that are more or less effective with different problems. Scientific researchers in both fields (and in philosophy) have eagerly endorsed hypothetical identity relations between components of their ontologies, which, at least in neuroscience, has stimulated some empirical research. Neuroscience is concerned with a wider class of events than psychology, but that is to be expected since

it operates at a lower level. Present, then, are all the earmarks of *interlevel* theoretic relations.

The mistake *all* versions of eliminative materialism have made is to draw their eliminativist conclusions about the *interlevel* relationship between psychology and neuroscience on the basis of an analysis appropriate to *intralevel* contexts. The eliminativists *correctly* claim that theories at the two levels have many important conceptual discontinuities, but they incorrectly conclude (spurred on, no doubt, by strident Kuhnians) that such incommensurability requires the elimination of one or the other. In *intralevel* contexts during scientific revolutions such crises *do* require that sort of radical surgery, but in *interlevel* contexts such a measure would eliminate potentially important stimuli for scientific discovery. *The history of science reveals no precedent for theory replacement or elimination in interlevel contexts.* The only way it might is in those *interlevel* situations at the completely opposite end of Churchland's spectrum. That is a case where all the principles of the upper-level theory follow as *deductive consequences* from those of the lower-level theory and where all the upper-level entities and all of their properties could be *strictly identified* with aggregates of lower-level entities and their properties⁹—which is to say, the case of a classical microreduction. Even this, though, would not constitute a logical warrant for the elimination of the upper-level theory, since it would necessarily employ a set of reduction functions which are not themselves reducible to the lower-level theory.¹⁰

I wish to conclude, however, by endorsing Churchland's defense of scientific realism from skeptical onslaughts, his insistence on the fundamentally theoretical dimension of all common sense, and his prediction that future science will not abide commonsense psychology. The replacement of theories, though, is an *intralevel* phenomenon. Therefore, defenders of the ideology of commonsense psychology ought to be *most* wary, not of developments in neuroscience, but rather of those in experimental cognitive psychology. Although that field shows few signs of revolutionizing our psychological views, in the Kuhnian sense, it does not follow that it is, therefore, incapable of *eventually* replacing (and,

⁹This is a simplified account of what Causey (1977) calls a uniform microreduction. I have discussed elsewhere (1981) the substantial skepticism as to whether such a situation has ever held at any time between two theories from different levels. The reduction of thermodynamics to statistical mechanics is the most plausible candidate that comes to mind, but even here the concepts and ontology of classical thermodynamics have proven sufficiently robust in other sciences that no one has suggested eliminating or denying the reality of gases, pressures, temperatures, etc.

¹⁰As I argued in my 1981, Causey's strategies for avoiding this problem are ineffective.

perhaps, even completely eliminating) commonsense psychology.¹¹ As in the case of Galileo's law of free fall, though, commonsense psychology handles the majority of situations we face from day to day reasonably well for many of our explanatory and predictive purposes. But also like Galileo's law, this suggests nothing more than that a faint image of some of our commonsense psychological views *may* persist in some future psychology.

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¹¹Hence, although I am generally sympathetic to Kitcher's discussion (1984, especially pp. 102–6) of intertheoretic relations so far as it goes, I do not agree that her argument offers an ultimately compelling "defense of intentional psychology."

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