

11-22-2010

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### Recommended Citation

Riccardi, Sebastian (2010) "Should Eliminativism Be Eliminated?," *Macalester Journal of Philosophy*: Vol. 7: Iss. 1, Article 9.  
Available at: <http://digitalcommons.macalester.edu/phil/vol7/iss1/9>

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## Sebastian Riccardi

### "Should Eliminativism Be Eliminated?"

Eliminative materialism is the thesis that our common-sense conception of psychological phenomena constitutes a radically false theory, a theory so fundamentally defective that both the principles and ontology of that theory will eventually be displaced, rather than smoothly reduced by completed neuroscience (Churchland 1981).

Eliminative materialism is a radical thesis which asserts that many of the basic concepts we use to categorize mental phenomena are extensionless. Chief among the targets of the eliminativist's criticisms are the intentional idioms of the propositional attitudes. According to the eliminativist thesis there literally are no such things as beliefs, desires, hopes, etc. Arguing for this thesis has become something of a pet project for the husband and wife duo of Paul and Patricia Churchland. While this thesis seems absurd at first, the Churchlands have devoted a number of cogent arguments in support of this position. As stated by the Churchlands, the thesis is not tantamount to an elimination of the realm of the mental, as some of their critics maintain (see Searle 1992). Instead, the Churchlands defend a radically different vision of mental phenomena where the propositional attitudes are replaced by non-linguistic states of a massively parallel connectionist network. In this paper, however, I will not seek to defend the Churchlands from their critics.

The most definitive statement of the Churchlands' views on eliminativism comes in Paul Churchland's 1981 article, "Eliminative Materialism and the Propositional Attitudes." In this article (and in many subsequent publications) he attacks the body of theory that he calls folk psychology. Folk psychology is "our common sense conception of psychological phenomena." He argues that not only is folk psychology a theory, it is a bad one, and neither it nor its theoretical entities, the propositional attitudes, should find a place in future theories of the mind/brain. Most of the article is filled with rhetoric designed to make those who oppose the elimination of propositional attitudes from serious science look like the clergy that resisted Galilean mechanics or atomic chemistry. However, there are two serious arguments in favor of the elimination of folk psychology from cognitive science.

The first argument, which I will call the degenerating research program argument, relies on the history and scope of our folk psychological categories. Churchland points out the fact that our folk psychology has not really changed in the past two millennia. The same principles that we use to predict and explain our neighbor's behavior are essentially the same ones used by the ancient Greeks. This fact explains how we understand Sophocles' and Shakespeare's plays, as well as Neil Simon's. The argument also points out the limited range of application of our folk psychological theories. They only apply to normal humans and have limited explanatory resources; we cannot turn to folk psychology to explain learning, perception or a host of other

interesting psychological phenomena. For these reasons, we might expect that our folk psychological "research program" may be on its way out. When we replace our folk psychology with a more robust theory, there is little or no reason to expect that the categories of belief, desire, etc., will be preserved in the transition.

Churchland's next argument is equally simple: while Aristotelian final causes have been eliminated from physics, and vitalism from biology, the propositional attitudes' central place in psychological theory has made it impossible for psychology to be included in the growing scientific synthesis. (I will refer to this argument as the argument from failure of integration.) The categories of folk psychology do not, nor are likely to, have any parallel in the physical sciences; that is, there are no likely prospects for the reduction of our folk psychological ontology to any physical one. In the absence of progress or hopes for reduction to legitimize our folk psychological theory, elimination is the only option.

Patricia Churchland presents a similar argument in her ode to reductionism, *Neurophilosophy: Toward a Unified Science of the Mind/Brain* (1986). Her argument (which I will dub the argument from irreducibility) is similar to the argument from failure of integration in that it starts from the innocent premise of materialism--that the mind is the brain at some level of description. In her book, Churchland lays out a model for how the future of cognitive science should progress, which she dubs the "co-evolutionary research ideology." According to this model, rather than simply going their separate ways, neuroscience and psychology should inform each other and attempt to knit the theories of the mind and the brain into one.<sup>1</sup> This model accurately describes some of the most successful research programs in biology, chemistry and physics, and so by analogy, following this model in cognitive science ought to lead to success. The co-evolution model as it applies to cognitive science advises that, as each discipline develops, it should aim to approximate the theories of the other, with the eventual aim of a reduction of psychological theories to the more basic science of neurobiology. If it turns out that there is nothing in the brain that answers to the descriptions given by folk psychology, which is the way it looks at the moment, then so much the worse for beliefs and desires.

Before carefully examining the relevant arguments for eliminativism, I would like to note the systematic ambiguity that is inherent in the eliminativist thesis. The eliminativist thesis states that folk psychology, as a theory, is false and its ontology is chimerical. But at times in the arguments for eliminativism, "folk psychology" refers to a practice that has been engaged in since the time of Homer, while at others it refers to a particular methodology in cognitive science that refers to beliefs and desires in order

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<sup>1</sup> In this paper I will be using the term "co-evolutionary research ideology" in this strong sense. There are some passages in Patricia Churchland's book that suggest a more moderate view of this research methodology, namely that neuroscience and psychology ought to keep informed of developments in each other. However, since this claim is simply commonsense and does not provide any support for eliminativist arguments, I will not examine it in this paper.

to explain behavior. If folk psychology is taken, in the first sense, as the psychological theory of the folk, then of course it will not be a serious candidate for scientific psychology. However, this conclusion is neither interesting nor important for cognitive science. As Kitcher (1996, 52) points out, "the folk never get anything right." Only if "folk psychology" is meant in the second sense, as a methodology of offering explanations of behavior in psychology, by citing a person's beliefs and desires, does the eliminativist thesis hold any interest. Now we can see that these considerations not only defuse the debate over whether folk psychology is really a theory or not (Wilkes 1984; Churchland 1989, chap. 6), but it also undermines the degenerating research program argument. This fact can be seen by noting that while the "psychology of the folk" may not have made significant progress since the time of the Greeks, the same claim is obvious false when applied to "traditional" cognitive psychology (Kitcher 1996). These considerations also seriously weaken the arguments from irreducibility and failure of integration. This fact is obvious once we realize that progress in the field of psychological explanation can legitimize the use of propositional attitudes in psychology. Now in order for those arguments to have force, as the Churchlands require, they must claim that any theory--not just ones that show no progress--must be reducible in order to justify their acceptance.

The Churchlands' pessimistic conclusions about the future of our folk psychological concepts in the serious study of the mind/brain are motivated primarily by their acknowledgment of the broadly non-sentential nature of most cognition and the probable lack of any determinate mapping from propositional states to neurological ones. These considerations are largely noncontroversial, and are accepted by many who would not endorse the Churchlands' radical eliminativist claims (Dennett 1987; Smolensky 1988, Clark 1989); for a dissenting view see Fodor and Pylyshyn (1988). Why, then, do the Churchlands make the leap from the irreducibility of any theory that makes essential use of the propositional attitudes to neuroscience, to the far stronger conclusion that there is no such thing as belief? I believe that this giant step from premise to conclusion includes (as way stations) dubious views about the proper methodology for psychology and the role of reduction in the sciences.

First, I will deal with the Churchlands' views about the methodology of psychology. In order for their arguments for the elimination of propositional states from psychology to work, they must accept some sort of methodological principle such as methodological solipsism (Fodor 1980) or Stich's principle of psychological autonomy (Stich 1978). Imagine what Gilbert Ryle's reaction to the Churchlands' arguments from failure of integration or reduction would be. Ryle, it should be noted, was committed to a methodology for approaching "mental" phenomena that could be considered the exact opposite of methodological solipsism. However, he was a materialist nonetheless, and would gladly affirm that it is necessary that "mental" terms such as "belief or desire" be reducible to physical phenomena to legitimize their use. But that is about as far as our imaginary Ryle would agree with the real Churchlands. We can imagine him saying, "Sure, the propositional attitudes must be either reducible or dispensed with, but why should they be reducible to *neuroscience* of all things?" We can imagine him looking incredulous or laughing out loud, and leave it at that.

This little imaginary walk down memory lane ought to remind us that forty years ago the suggestion that psychology and neuroscience dealt with the same subject (even on different levels of abstraction) would not have been taken very seriously. Only if neuroscience and psychology are considered to be studying the same thing, namely the functioning of the brain, does something like the co-evolutionary research ideology even make sense. Try to take seriously the claim that geological and zoological theories should attempt to approximate one another in the eventual hopes of a single theory of animals and rocks. Only if it is antecedently accepted that psychology should reduce to neuroscience would the co-evolutionary methodology be recommended. But only if something like methodological solipsism, studying only the processes that occur within a subject's brain, is the proper research strategy in psychology should the psychological be reduced to the neurological. And this claim is dubious for two reasons. First, many of the most important and interesting phenomena in the domain of cognitive science (such as language acquisition, problem-solving, perception, and learning, to name a few) involve an essential environmental component. The relation between the environmental contributions and those components contributed by the organism to psychological processes is of interest to psychologists in its own right. (It could even be argued that this relation is of more interest than the mere goings-on in people's brains.) And secondly, the claim fails to reflect the way psychology is actually practiced. Psychologists rarely restrict their attention to only such processes as occur within people's heads (Kitcher 1985).

Without the added premise of methodological solipsism or some equivalent notion, there is no reason why the failure of the propositional attitudes to reduce to neural states should count against them. We can see why this is the case, since the propositional attitudes, paradigms of intentionality, are inextricably linked with semantic concepts like truth, reference and content. These semantic notions are in turn features of the organism's relationship to the environment, and of course it would be an odd form of idealism to insist that the external world should be reducible to neurophysiology.

Now I will look at the Churchlands' views on reduction, and the role it plays in the relations between theories in the methodology. To repeat, the general form of their arguments is that of a dilemma-reduction of the propositional attitudes or elimination of them: there will be no reduction of the propositional attitudes; therefore, they should be eliminated. Implicit in the construction of this dilemma is the idea that the only profitable relationship between any two theories in science is a reductive one (this implicit idea becomes explicit in Churchland 1989, chap. 13); otherwise, one of them ought to be eliminated. This view of the relationship between theories is a very radical one that is, for the most part, a minority view. This extreme view entails two conclusions: first, that every true theory in all of the special sciences is expressible in a true theory of physics (enriched with bridge laws, of course). This is entailed by the Churchlands' view, because every true theory is reducible to a more basic one, physics is the most basic science, and the relation of "reducible to" is transitive. And second, since one function of a theory (perhaps the most important one) is explanation, it follows that the true theory of physics, that all other accurate theories are reducible to, can in principle explain all of the phenomena that the reduced theories can.

There are *prima facie* embarrassments to this view that some physical theory is adequate to explain all the relevant phenomena which can be dealt with at higher levels of abstraction. For instance, to take an example of Dennett's (1987), what physical properties do all and only American dollars possess? How can physics explain economic phenomena? We must take it on faith that the social sciences are reducible to physics, since no plausible account of such a reduction has even been attempted. The same also goes for many biological phenomena, such as the process of natural selection (Dennett 1995), and any biological phenomena whose explanation rests on evolution (and there are many). There is also Putnam's famous example, that no explanation of why a square peg cannot fit into a round hole of equal area can be given in terms of microstructural details. We can take these considerations as a *reductio* of the concept of radical reductionism, or as a sign that a large portion of the social sciences and biology are slated for elimination.

Whether the above argument against the Churchlands' reductionism is convincing rests on how sympathetic to eliminativism you are, and so it cannot really be used as an argument *against* eliminativism. But it does lead to a method of using the eliminativist methodology to evaluate sciences where issues of reduction and elimination come into play but the propositional attitudes do not. The test case for the eliminativist methodology that I will use is genetics. In genetics, there are two stories to be told. The first begins with Mendel and leads to classical and population genetics. The second begins with Watson and Crick and leads to molecular genetics. Classical genetics is where the term "gene" originally comes from, and where the sense in which I will use the word comes from. The gene is the unit of heredity, and classical genetics studies its dynamics by examining the relations between the input of genotypes by parents and the output of the phenotypes of the offspring. Molecular genetics deals with strands of DNA and the biochemical mechanisms that lead from strands of DNA to proteins. The case of classical and molecular genetics is an important testing ground for the argument from irreducibility, because both theories that we will be examining are generally accepted well-confirmed scientific theories that have been very progressive in recent decades. It is an important test case for the argument from irreducibility because it deals with the same methodological issues without prejudging the issue of eliminative materialism. The crucial question now is: Is classical genetics reducible to molecular genetics?

Now it is important to note that reduction is a strong relationship between theories. If a theory  $T$  is reducible to  $T'$ , that implies that there is a one-to-one mapping between the laws and ontology of  $T$  to  $T'$ , such that  $T'$ , amplified by bridge laws, can deduce all of the true sentences of  $T$ . By now there are very good reasons for thinking that classical genetics cannot be reduced to molecular genetics. There are numerous instances of heritable components to (or genes for) such phenotypic traits as intelligence, schizophrenia, and depression, discovered using the methods of population genetics, that almost certainly do not have a single molecular counterpart. The few exceptions to this general rule, such as the discovery of a DNA sequence that codes for hemoglobin (whose absence results in sickle cell anemia), are cases of extremely simple developmental mechanisms that must be rare considering the multi-purpose functions of most proteins (Kitcher 1984). Phillip Kitcher argues that no possible revisions to the molecular genetic

theory could capture the generality of the classical genetic theory.<sup>2</sup> He interprets this fact as showing that the level at which classical genetics operates is distinct from that of molecular genetics.

Patricia Churchland (1986, 364-7) takes almost the opposite view, arguing that the lack of fit between theories requires that major revisions must occur in classical genetic theory. Although she seems loath to draw ontological conclusions in this case, it seems that she is an eliminativist in regard to genes.<sup>3</sup> And if she is an eliminativist about genes, then, it seems that she is at a loss to explain the heredity and variation invoked in the theory of natural selection, which is committed to the classical version of genetic transmission (Dennett 1995). These conclusions follow from her assertion, "Very roughly, reduction will still have been accomplished if the input-output effects described by a *suitably evolved* macrogenetic theory [theory of gene transmission] can be explained by molecular goings-on" (Churchland 1986, 367; italics mine). However, it is already known that the "molecular goings-on" in some organisms (like viruses) are of a different kind than others, even though many of the same "macrogenetic" principles are followed (Kitcher 1984). Sacrificing the generality of a theory like classical genetics in order to reduce it is poor scientific methodology, because it decreases the number of phenomena for which you have an explanation.

So we can see from the consideration of the case of classical and molecular genetics that the co-evolutionary research ideology required to support the argument from irreducibility leads to highly dubious methodological strictures. We can see why the reductionist approach failed in the case of genetics by noting that the class of "Mendelian systems" (systems whose behavior can be accurately described and explained by classical genetics) is wider than the class of "molecular genetic systems." Because of its wider applicability, classical genetics is not reducible to molecular genetics and, for the same reason, is not eliminable. The same can be said for propositional attitude psychology in relation to neuroscience. The class of intentional systems is wider than the class of "neurological systems" (the class of systems whose behavior can be accurately described and explained by neurophysiology). This form of argument is the familiar functionalist argument from multiple realization, one to which the Churchlands have yet to adequately

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<sup>2</sup> This fact is easily appreciated by noting that classical genetics can explain not only the patterns of phenotypes in organisms that use DNA to transmit genetic material, but also the patterns of phenotypes in viruses that use RNA. Classical genetics can even be applied to "artificial life" that do not use chemical means to replicate.

<sup>3</sup> Her view on the ontological status of the Mendelian gene is the same as her view on "virtual governors," which are appealed to by engineers to describe the uniform output of a set of generators whose independent outputs fluctuate randomly. This attitude is either one of eliminativism or of a Dennettian pragmatic stance towards ontology, which would undermine her eliminative arguments against folk psychology.

respond.<sup>4</sup> The important moral to draw from these considerations of reduction and methodological solipsism is that the argument from irreducibility has lost its force.

Most of the discussion in the preceding pages applies only to Patricia Churchland's arguments for eliminativism, since my criticisms apply to the strict reductionist paradigm and her reliance on neuroscience to explain psychology. Although Paul Churchland seems sympathetic to his wife's arguments for eliminativism, and probably subscribes to many of the same ideas that can be used to refute her argument, I think his argument from failure of integration is a subtler one. Integration with the rest of our materialist science does not entail reducibility, nor does he specify which science it is that is supposed to legitimize the use of the propositional attitudes. I think his argument for the elimination of the propositional attitude, while motivated by many of the same empirical considerations that led to the argument from irreducibility, is also motivated by distinctly philosophical ideas. His assertion that there will be no union of the propositional attitudes with materialism is not new; it has been made before, by Brentano and Quine. Brentano's thesis, if true, entails that the realm of the mental, of which the propositional attitudes are constitutive, is irreducible to the physical. And Quine's indeterminacy thesis entails that, for any individual, there are numerous incompatible belief ascriptions that are all equally true. However, the recognition of the fact that there is indeterminacy of the content of propositional states does not lead directly to eliminativism.<sup>5</sup> This fact only leads to eliminativism if one's tolerance for indeterminacy between scientific theories is minuscule. We have seen that, relative to the molecular level, there is indeterminacy in terms of genes as well as indeterminacy of other Darwinian workhorses such as selection, phenotypic trait, and species (Dennett, 1995). To eliminate these terms and their attendant laws and ontologies as unexplanatory would be perverse as well as bad scientific methodology. In fact, we can see that this intolerance for indeterminacy between levels of explanation is tantamount to Patricia Churchland's excessively strong requirement of reducibility for respectability. The recognition of indeterminacy, or autonomy, of one domain of explanation with respect to another is simply to give up the need for all theories to be connected to each other by reduction.

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<sup>4</sup> In response to this argument, Paul Churchland (1981) merely says that it could also be used to defend alchemy, which does not really damage the argument. Patricia Churchland responds (1986) entirely unsatisfactorily by claiming that temperature has been reduced (even though only the reduction of the temperature of gases fits any plausible model of reduction) and is multiply instantiated in solids, liquids and gases. For a criticism of this argument, see Kitcher (1996).

<sup>5</sup> Far from being a fatal flaw of belief/desire explanation, the indeterminacy of mental content ascription is a natural consequence for those like Dennett (1987) or Clark (1989) who espouse holism of mental contents.



We have seen here that the main arguments for the elimination of the propositional attitudes from the serious study of psychology rely either on ambiguity in the term "folk psychology" (the degenerating research program argument), or on intolerable forms of reductionism. Once we remove the extreme reductionist premises from the Churchlands' arguments, we find two rather sensible conclusions. First, many of the principles of our familiar "folk psychology" are probably false, but that is a conclusion that anyone familiar with the psychological literature should be familiar with and has no bearing on the importance of neuroscience or the use of propositional attitudes to explain behavior. Second, any psychological theories that use propositional attitudes to explain behavior, as most successful theories that deal with higher mental functions do, will not smoothly reduce to neuroscience. If we relax our views on reduction and ontology, which we must do to avoid throwing the babies of biology out with the "bathwater" of psychology, what appears is a distinctly psychological level of explanation. This level neither is, nor should be, reducible to the neuroscientific level since it deals not only with internal states of the organism but with the patterns of behavior exhibited by organisms as they interact with their environment. These patterns are of interest in their own right, as any psychologist can tell you, and their irreducibility attests to this fact. The autonomy of psychology does not mean that psychologists should ignore neuroscientific advances: of course they should not. But it is a giant leap from the premise that psychologists should pay attention to neuroscience to the conclusion that they should attempt to reduce (rather than merely ground) their theories to neuroscience. Consider an analogous case: psychologists should pay attention to theories in statistics (which in fact they must), so they should attempt to reduce their theories to (rather than merely ground them in) theories in statistics. Once we take a more pragmatic view toward ontology and reducibility, we can see that the cooperation of psychology and neuroscience (surely a good thing) has little or no bearing on issues of eliminativism. As for whether the eliminativist thesis is true, I see no reason why it should be, but ultimately the question will be settled by future generations of psychologists.

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