Reinforcing the Three 'R's: Reduction, Reception, and Replacement

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Ronald Endicott
Department of Philosophy & Religion
Program Director for Cognitive Science
College of Humanities and Social Sciences
North Carolina State University
Campus Box 8103
Raleigh, NC 27695-8103

Abstract: I offer a more or less friendly amendment to an influential set of proposals about scientific unification. After clarifying a few preliminary issues, I describe Kenneth Schaffner's "General Reduction-Replacement" model of scientific unification. I then turn to its most recent descendant, the "New Wave" approach to reduction developed by Paul and Patricia Churchland, Clifford Hooker, and John Bickle. Both Schaffner and the New Wave interpret scientific unification very broadly in terms of a continuum from theory reduction to theory replacement. This is good insofar as it goes. But I propose to expand the picture in a way that is more receptive to the role that type irreducible and irreplaceable theories play in a process of partial reduction, specifically, their token reduction. The result is a more comprehensive "Reduction-Reception-Replacement" model of scientific unification. I also suggest a link between token reduction, so understood, and the concept of mechanistic explanation.

I. Introduction

Discussions of reduction and related issues in the philosophy of science are complicated by at least two factors: *ambiguity*, or multiple concepts of reduction, and *ambition*, or the range of cases to which a given concept of reduction is thought to apply. First, regarding ambiguity, "reduction" expresses different concepts to different individuals and intellectual communities. For example, in contrast to evolutionary explanations, the biologist Theodosius Dobzhanski identifies reduction with certain Cartesian methods employed in mechanistic explanation, and he cites the mathematical treatment of automata as a case in point (1968, pp.1-2). Yet, turning to a different intellectual community, many philosophers who are trained in classical computational psychology would not judge the mathematical treatment of automata and various complementary accounts of their mechanistic implementations to be reductionist in any substantial sense, being compatible with the autonomy of computational theory and the nonidentity of computational and physical properties.

Thus, Jerry Fodor -- no friend of “reduction” in the philosophy of mind -- presents the computational model as a grand synthesis of mechanism and psychological explanation (1981, pp.13ff.). For someone like Fodor, the concept of reduction is more closely aligned with ideas that descended from the logical positivist tradition, specifically, the absorption of one theory by another *via* connecting principles that express property identities between two theoretical domains. That notion is not implied by mechanistic explanation *per se*. Indeed, some philosophers claim that notions based upon mechanistic explanation provide a better alternative to the notions of reduction that have dominated philosophical discussions in the post-positivist era (Bechtel & Richardson, 1993; Machamer, Darden, & Craver, 2000).

Second, regarding ambition, after one settles upon a particular concept of reduction, and after one finishes the modest philosophical work of clarifying or refining the concept in question, there is the empirical task of determining the range of cases to which that concept of reduction applies. Here the most serious controversies arise, since virtually every existing concept of reduction applies *somewhere*, if only to marginal but well-chosen cases. So the most important question is whether a suitably refined concept can apply to a satisfactory range of cases which a critical mass of scientists describe as reduction, or, more ambitiously, whether, by the discovery of new information or a reconceptualization of the old, that concept of reduction can be extended to other unanticipated cases. Here the philosopher and the scientist face the danger of being overly ambitious. There is a significant difference between *providing an account of scientific reduction* versus *defending a broad philosophical vision of physicalistic monism* whereby all theories are either reducible to or replaceable by theories in the physical sciences. For example, various psychological theories remain resistant to the concepts of reduction that developed out of the positivist tradition. Hence, theoreticians should be prepared to apply those concepts of reduction to the appropriate range of cases where the world happens to comply even if the world does not always so comply.

With these preliminaries in hand, I begin by describing a concept expressed by Kenneth Schaffner's "General Reduction-Replacement" account of scientific unification. I then turn to its popular descendant, the "New Wave" approach developed by Paul and Patricia Churchland, Clifford Hooker, and John Bickle. Both Schaffner and the New Wave interpret scientific unification very broadly in terms of a continuum of cases from theory reduction to theory replacement. This is good insofar as it goes. But I propose to expand the picture in a way that is more receptive to the role that otherwise and in other respects irreducible and irreplaceable theories play in a process of partial reduction, specifically, their token reduction. The result is a more comprehensive "Reduction-Reception-Replacement" model of scientific unification.

II. Schaffner's General Reduction-Replacement Paradigm

Schaffner's views on reduction are meant to be an improvement upon the classical position that developed within the logical positivist tradition. Consider Ernest Nagel's view of reduction, which represents the paradigm of positivist thinking on the subject. According to Nagel (1961), 'reduction' meant the derivation of a theory from a more basic or inclusive theory, in a heterogeneous case where the theories do not share a common vocabulary, by means of connecting principles that link the theoretical terms of the respective theories. As this tradition developed, the connecting principles were conceived in terms of biconditional bridge laws that express cross-theoretic property identities (Sklar, 1967, pp.120-124; Causey, 1972).

Parenthetically, Robert Richardson (1979) and John Bickle (1998, p.120) claim that Nagel held a more liberal view, believing that the purposes of reduction could be served by weaker one-way conditionals that express mere sufficient conditions in the basic reducing theory. But the justification is based upon a footnote in Nagel which I believe has been misunderstood. [1] After mentioning Kemeny and Oppenheim's observation that connectability would guarantee derivability if the connections between the theories were biconditional in form, Nagel says: "However, the linkage between *A* [the nonbasic or "secondary" science] and *B* [the basic or "primary" science] is not necessarily biconditional in form, and may for example be only a one-way conditional" (1961, p.355, fn.5). Both Richardson and Bickle quote this passage, but they neglect to mention Nagel's remark that immediately follows, namely: "But in this eventuality '*A*' is not replaceable by '*B*,' and hence the secondary science will not in general be deducible from a theory of the primary discipline" (loc. cit.). So Nagel's view seems to be that, for a general account of the scientific practice in question, one *cannot* have the reduction/deduction of a nonbasic theory by virtue of one-way conditionals because they do not guarantee that the nonbasic theory is replaceable by the basic theory.

Of course, Nagel scholarship aside, other positivists held the more liberal view in question (Hempel, 1966, p.105). But, as the tradition developed, many others stipulated that the connecting principles must be biconditional laws in order to justify cross-theoretic identities. As Schaffner put it, "connectability later came to be best seen as representing a kind of 'synthetic identity'" (1993, p.425). Indeed, one cannot achieve an important goal of reduction without such identities – a simplification in the world's ontology. [2]

Yet philosophers of science in the post-positivist era observed that, for many central cases of reduction, the terms of the basic reducing theory only *approximate* the terms of the reduced theory. Using an example from genetics, Schaffner remarked that, until the late 1950s, the gene was typically defined in three ways: "(1) the smallest segment of the chromosome that could undergo mutation, (2) the smallest segment of the chromosome that could recombine with its homologous chromosome in crossing over, and (3) the section of the chromosome functionally responsible for a unit of character" (1967, pp.142-43). However, it was discovered that (1) through (3) have different physical referents, in particular, that (1) and (2) involve smaller sequences of DNA. Accordingly, geneticists in the 1960s began speak of 'mutons,' 'recons,' and 'cistrons,' respectively, where only the cistrons play roughly the role of Mendelian genes. Indeed, the notion of a gene has been further revised to include coding in RNA (see Weber, 2005).

These developments were of some consequence. For it was then apparent that what can be derived from molecular genetics is a *revised* or *corrected* genetic theory and not the original theory developed by Mendel. Thus, Schaffner proposed an account of "approximate reduction," supplying a set of formal conditions for the derivation of a corrected theory that is strongly analogous to the original reduced theory by means of connecting principles which preserve referential identity (1967, p.144). Along with the ideally exact form of reduction, Schaffner's approximate reduction can be conveniently summarized as follows:

> S DEDUCTION: either the original reduced T_R or a strongly analogous corrected T_R* is deduced from the basic reducing T_B.

Moreover, because some scientists use the terminology of reduction to cover cases where the ontology of the original target theory is retained as well as cases where the ontology of the original target theory is replaced, Schaffner later extended his account to incorporate theory replacement. Specifically, in addition to either the direct (for T_R) or approximating (for T_R*) derivability conditions of *S Deduction*, Schaffner proposed that theory replacement involves the derivation of an "experimental arena" for the original target of reduction T_R – a "domain" in Dudley Shapere's (1974) sense -- which is the set of experimental results associated with T_R that are better accounted for by the basic theory (Schaffner, 1977, pp.148-151; also 1993, pp.427-432). Viewing T_R* in terms of a lesser set of experimental results means that, for theory replacement, the deductive consequence of the basic theory is still connected with but no longer strongly analogous to the original T_R. The result is a "continuum of reduction relations" represented by a "General Reduction-Replacement Model" (Schaffner, 1977, pp.148, 149). It can be captured by the following convenient summary of his formal requirements for both approximate reduction and replacement:

> S CONTINUUM: there is a continuum of strong to weak analogies between the reduced T_R and the corrected T_R*, with the strong relations justifying retention and the weak relations justifying replacement of the ontology of T_R.

III. New Wave Permutations on the Schaffner Theme

Much discussion about reduction in contemporary philosophy of science has concerned "New Wave" theories developed and defended by Paul and Patricia Churchland, Clifford Hooker, and John Bickle. [3] These accounts are based upon the framework of Schaffner's General Reduction-Replacement model, but they include a number of distinctive features. More specifically, the Churchlands, Hooker, and Bickle accept the framework of a derivational model, the role of a corrected theory in deduction, and the continuum of cases from reduction to replacement. But they also believe that Schaffner's account is too permissive by allowing the corrected theory to contain elements of the uncorrected target for reduction. Hence, advocates in the New Wave place a condition on reduction that is central to their approach, namely, *a base-level constraint on the resources used in the construction or development of the corrected image T_R**. Paul Churchland says that:

> [A] successful reduction ideally has the outcome that, under the term mapping effected by the correspondence rules, the central principles of T_R (those of semantic and systematic importance) are mapped onto general sentences of T_B that are theorems of T_B. Call the set of such sentences T_R*. This set is the image of T_R within T_B … on the account given above it is not the reduced theory, T_R, that is deduced from the principles of T_B, as some other accounts have it. What is deduced from T_B is rather the set T_R*, an equipotent image of T_R within the idiom of T_B (P.M. Churchland, 1979, pp.81, 83, with a change in the subscripts).

Churchland stipulates that the corrected image T_R* must be part of the basic theory, being "general sentences of T_B" that constitute an "equipotent image of T_R within the idiom of T_B." Or, as Hooker describes it:

> Within T_B construct an analog, T_R*, of T_R under certain conditions C_R such that T_B and C_R entails T_R* and argue that the analog relation, AR, between T_R and T_R* warrants claiming (some kind of) reduction relation R, between T_R and T_B (1981, p.49).

And again, regarding the constraint in question, Bickle labels the corrected image 'I_B' to underscore its base-level nature, and he emphasizes the contrast with Schaffner's view:

> It is important not to confuse Hooker's deduced image I_B with Schaffner's corrected version of the reduced theory T_R*. Hooker's I_B is characterized completely within the framework and vocabulary of T_B; Schaffner's T_R* is a corrected version of T_R, and so is characterized (at least in part) out of the resources and vocabulary of the reduced theory (2003, p.17).

More precisely, as I read Schaffner, his account does not *require* that T_R* be characterized (even in part) out of the resources and vocabulary of a nonbasic T_R, since it is framed in a general way to allow for "homogeneous" cases where the reduced and reducing theories share the same vocabulary (Schaffner, 1967, p.144). Even so, Schaffner's account *allows* that T_R* be characterized out of the resources and vocabulary of a nonbasic and heterogeneously specified T_R. But the New Wave constraint does not. It is uncompromising and exclusionary. It can be expressed thus:

> CH CONSTRUCTION: the language and concepts of the basic reducing T_B, not the original reduced T_R, must supply the resources for constructing the corrected image T_R*.

CH Construction is the main difference between Schaffner's General Reduction-Replacement model and the basic New Wave approach. [4] I have previously argued that this difference should be discounted, since (a) some of *CH Construction's* advertised virtues are shared by competing theories, (b) the exclusionary demands of *CH Construction* appear to conflict with the co-evolution of theories where terms in the corrected theory T_R* have developed out of the conceptual resources of both parent theories T_B and T_R, and (c) the adoption of *CH Construction* is unduly restrictive from a methodological point of view by ruling out reductive strategies that require aid from the concepts and vocabulary of the original reduced T_R (Endicott, 1998a, pp. 60-67). [5]

However, even if *CH Construction* is retained, I want to focus on the similarity between Schaffner's model and the New Wave account, a similarity that exists by virtue of the fact that advocates of the New Wave accept the *S Continuum*. As Paul Churchland puts it, because of the differing degrees of similarity between the original T_R and the corrected T_R* from one scientific case to another, "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" (P.M. Churchland, 1979, p.84). Or, as Patricia Churchland says:

> The evolving unifications seen in science therefore encompass not only smooth reductions with cross-theoretic identifications but also rather 'bumpy' reductions where cross-theoretic identifications are problematic and involve revision of the old theory's concepts, and outright elimination with no cross-theoretic identifications at all (P.S. Churchland, 1986, p.284).

Likewise, because of the variable analog relation between T_R and T_R*, Bickle says that "there is a spectrum or continuum of cases lying between the smooth and bumpy endpoints" from "perfect retention" to "total replacement" (1998, p.30).

IV. An Exhaustive Reading of Reduction or Replacement

Both Schaffner the New Wave appear to make an assumption that initially seems appropriate for the philosophical task at hand, namely, they appear to assume that *a comprehensive account of scientific unification will only describe nonbasic theories that are either wholly reduced or replaced, or broken down into parts that are either reduced or replaced*. Put in a different way, there is no place for irreducible and irreplaceable theories or irreducible and irreplaceable parts of theories on the *S Continuum*. The assumption seems clear in Schaffner's case, since the formal conditions of his General Reduction-Replacement model are a simple disjunction of the conditions for approximate reduction and the conditions for theory replacement (Schaffner, 1977, p.149; 1993, p.429). Of course Schaffner permits "partial reduction" in a sense that will "allow for the possibility of a partially adequate *component* of T_R being maintained" (1977, p.148, with a change in the subscript). But the implication seems to be that the adequate parts are maintained through reduction while the inadequate parts are naturally replaced -- no adequate parts that are unreducible and irreplaceable on the *S Continuum*.

Or consider how the New Wave appears to treat the continuum of cases from reduction to replacement. On the one end there is perfect retention, and on the other end there is outright replacement, while the area *between* those endpoints is occupied by mixed cases of reduction and replacement. For example, Bickle emphasizes the middle ground of theory revision for a number of scientific cases. But *Bickle accepts no dualism* -- be it a dualism of objects, properties, or laws (1998, pp.6-14). Therefore, in cases of theory revision, the elements of the original nonbasic T_R are either retained through reduction or rejected, with a subsequent revision of concepts to reflect the distance between T_R and its corrected base-level counterpart T_R* (ibid., p.200). More precisely, according to Bickle, cases where T_R is closely analogous to its base-level counterpart call for homogeneous ORLs (ontological reductive links) that provide a relatively smooth reduction of the property elements in T_R, while middle cases where T_R is not smoothly reduced but not outright replaced call for mixed ORLs, where some property elements are reduced by homogeneous ORLs and the rest are treated in a way that respects "the eliminativist strand of revisionary physicalism" according to which the properties "are abandoned as lacking actual extension" (ibid., p.202). Again, no irreducible and irreplaceable theories.

But why is this exhaustive reading of reduction or replacement problematic if the goal is to supply an account of *scientific unification* where, in fact, either a reduction or replacement is carried out?

V. Token Reductions as Partial Reductions

The assumption that a comprehensive account of scientific unification will only describe nonbasic theories that are either wholly reduced or replaced or broken down into parts that are either reduced or replaced is problematic even when the goal is to provide an account of scientific unification and even when unification is understood in terms of processes like reduction and replacement that simplify the world's ontology. Simply put, by giving place to type irreducible and irreplaceable theories, one is able to expand the range of partial reductions to include their *token reduction*. My working assumptions are:

(1) Reduction (versus replacement) is ontological unification *via* cross-theoretic identities.

(2) An adequate theory of reduction should provide an account that subsumes the widest range of cases involving ontological unification *via* cross-theoretic identities.

(3) Partial reductions that are accomplished through token identities provide ontological unification *via* cross-theoretic identities.

And hence:

(4) An adequate theory of reduction should provide an account that subsumes partial reductions that are accomplished through token identities.

To begin, there is to my knowledge no exhaustive study of partial reduction. But philosophers have used the term 'partial reduction' in several senses. I will distinguish two. There is (A) a *Mereological* sense of 'partial reduction' whereby not all the *parts* of a target theory are derived from a basic theory, and there is (B) a *Teleological* sense of 'partial reduction' whereby not all the perceived *goals* for reduction have been met when a target theory is derived from a basic theory. Thus, according to the mereological notion, a theory is decomposable into parts, where only a proper subset of those parts is subject to reduction. This notion can be further subdivided into categories that subsume cases of retention or replacement with respect to the entire theory, depending upon whether a significant number of its derivable proper parts are central theoretical structures or merely the experimental arena better accounted for by its successor theory (cf. Sklar, 1967, p.116, on the partial reduction of external sentences in a replaced theory).

In contrast, according to the teleological notion, there may be a partial reduction even when all the parts of a target theory are derivable from a basic theory. For example, Hartry Field (1972, p.362) says that Tarski's semantic theory of truth provides only a "partial reduction" of truth to nonsemantic terms, since Tarski's theory utilizes primitive semantic notions like denotation for names and satisfaction for predicates. To understand why Field has the teleological notion in mind, let T_R represent the set of sentences that English speakers assert to be true, and let T_B represent Tarski's theory of truth for the English language. Field does not deny that all the parts of T_R can be derived from T_B in the sense that every sentence that English speakers assert to be true is a theorem of T_B. Rather, Field believes that, even though every one of those sentences is a theorem of T_B, the physicalist goal of providing a reduction of truth to nonsemantic or physical terms has not been met.

Here I am only interested in the mereological sense of ‘partial reduction.’ More specifically, I am only interested in the kind of partial reduction that occurs when the subset of T_R’s derivable parts describe tokens of the types in T_R. Accordingly, T_R is a type irreducible but token reducible. But T_R is not just type irreducible. That is also true about replaced theories. Thus, my project is to incorporate their token reduction into a broad scheme of scientific unification from complete reduction to outright replacement, where the middle ground is *not* wholly occupied by nonbasic theories that are either reduced or replaced or broken down into parts that are either reduced or replaced, as it appears on the accounts developed by Schaffner and the New Wave.

Proposals about token reduction are not new. Fodor (1974) outlined the general idea for the special sciences, and Steven Kimbrough (1979) developed and defended its application regarding the token reduction of genetics to molecular biology. Specifically, Kimbrough suggests a scheme of reduction functions for token cases. Changing his symbolism slightly, where ‘F’ is a predicate of a nonbasic type irreducible but token reducible theory T_R, ‘P’ is a predicate of a base-level token reducing theory T_B, ‘a’ is an individual constant in T_R, and ‘b’ is an individual constant in T_B, the reduction functions are:

1. (for some x, y) [(Fx and Px) and (x = y)],
2. a = some x such that Fx,
3. b = some x such that Px,
4. a = b (Kimbrough, 1979, pp.403-404).

VI. Schaffner’s View of Token Reduction

Curiously, Schaffner acknowledges token reduction and explicitly cites both Fodor and Kimbrough, but he does not attempt to extend the range of partial reduction by such means. Instead, he says “there is some truth in Fodor’s speculations and in Kimbrough’s contentions,” but he believes that “they are overstated in connection with reduction in the biomedical sciences generally and particularly in the area of genetics”

(1993, p.463). Yet his reservations appear to miss the mark. On the one hand, and perhaps because he is accustomed to thinking in terms of the traditional derivation of types, Schaffner says:

> First, it should be noted that, in genetics, it is not specific individuals demarcated in space and time that are identified; rather, the *lac p* and *o* genes present in many individual bacteria are identified with DNA sequences presumed to be repeated in many (a potentially infinite number of) instances (loc. cit.).

But that surely *is* an overstatement, since (a) a complete formalization of genetic science contains singular terms, bound variables and constants for individuals, and since (b) the broader practice of genetic science must look at specific individuals and their specific genetic materials, otherwise the theory would be useless. At best, Schaffner has only observed that tokens are not types. He has not shown that a complete and fully adequate account of reduction should not encompass token reduction when the type variety is not forthcoming.

On the other hand, Schaffner takes a different line by reinterpreting the proposed token reduction in terms of a more restricted range of (type) generalizations for the kind of middle-sized objects befitting the biological sciences. He goes on to say:

> [T]here is an element of truth in Kimbrough's concerns. Reduction in genetics and, I suspect, much of biology and medicine appears to be more specific; it does not yield the broad generalizations and identities one finds in physics. This, I think, is due in part to the systems one studies in biology, and also to the kinds of theories one finds at what I have termed [in a previous chapter] the "middle range" (1993, p.465).

And again, in his summary remarks: "a less misleading gloss of 'token-token' in theses contexts would be to construe the term 'token' as a 'restricted type' in accordance with the view of theories developed in [previous] chapters" (ibid., p.466). But, as Schaffner explains them, the restricted types in question are simply the types that are not as universal in their application as the types of basic physics, specifically, the types found in biology and the other special sciences (ibid., p.97). So Schaffner comes full circle to the types of biology and the special sciences, and he nowhere explains how *their* tokens can be construed as restricted types, which seemed to be the proposal at issue.

Of course, others like David Lewis (1969) and Jaegwon Kim (1992) postulate more restricted special science types to achieve the purpose of reduction. More accurately put, they postulate species-specific and even individual-time-specific terms in the special sciences that nonrigidly denote physical properties. So, if this narrow reductive strategy is successful, there is no need to speak of Fodor-style token reductions of irreducible types. Rather, there is a complete reduction of types, construed in the species or individual-and-time restricted way. But others have argued, I think successfully, that species-specific terms fail to accommodate the inter-theoretic cross-classification that results from multiple realizability within individuals (see Horgan, 2001). Moreover, I have argued that the more restricted individual-time-specific terms are virtually indistinguishable from singular terms that pick out token events, and hence they fail to meet an essential desiderata for kind terms in the sciences, namely, being generalizable in the way needed for scientific explanation and prediction (Endicott, 1993, p.317).

So Schaffner does not explicitly utilize a scheme of token reduction for type-irreducible theories. However, his philosophical attitude about type irreducible theories permits such a scheme. For Schaffner is a methodological pluralist who accepts higher-level presently irreducible theories on pragmatic grounds but denies their irreducible status "in principle." In his words, Schaffner favors a "pragmatic, holistic, but in-principle reductionist approach" that construes "theories in biology and medicine as essentially (for the present and foreseeable future) interlevel" (1993, pp.413-414). Indeed, Schaffner's more recent work reflects a "reappreciation of the complexity by geneticists in the 1990s" that antireductionists had previously stressed about "many-many" mappings between genetics and molecular theory (2002, p.323). [6] So his "in principle" reductions are far removed from present scientific reality which contains type irreducible theories, and that is why my proposal to incorporate their token reduction, when developed, will constitute a more or less friendly amendment to Schaffner's General Reduction-Replacement model of scientific unification. Keep type-irreducible theories for pragmatic purposes, and interpret the subsequent token reduction functions accordingly -- realistically where the world complies and instrumentally otherwise.

Why did Schaffner not explicitly incorporate a scheme of token reduction into his General Reduction-Replacement model if his methodological pluralism allows it? Perhaps Schaffner thought that the token reduction of type irreducible theories is trivial. As William Wimsatt put the point with respect to psychological theory:

> Without type-correspondence, property identification seems to be ruled out, and about the only kind of identity left is "stuff" identity – roughly, that the stuff with the psychological properties is the same stuff as the stuff with the physical properties. Philosophers, concentrating on ontological dividends, have found this to be uninteresting and trivial. It seems like a common sense conclusion that we hardly need scientific sophistication or data to embrace (1976, p.225).

But one would be wise not to accept Wimsatt's casual record of commonsense, since the token reduction of type irreducible theories is hardly trivial. Without type-correspondence, there is no guarantee that two theories drawn from different scientific levels of inquiry will carve up the "stuff" of the world into the same naturally isolated particulars that are subject to the same explanations and predictions. To use a well-worn example, many tokens of economic theory – monetary transactions, financial institutions, aggregate supplies, the economic cost for Enron from 1997-2000, the U.S. labor force in 2006 – are not naturally isolated particulars subject to explanation and prediction from the vantage point of neuroscience, chemistry, or basic physics. I will return to this problem in the final section. But, in my estimation, the reason Schaffner did not incorporate the token reduction of type irreducible theories into his model of scientific unification is that it is built upon an assumption that excludes viable type irreducible theories. Specifically, Schaffner's model is built upon the assumption that the corrected theory "bears close similarity" to the original target for reduction (Schaffner, 1967, p.144), which, when expanded into a continuum that includes theory replacement, implies the following *Similarity Assumption*:

> S ASSUMPTION: similarity is the parameter which determines the place of T_R along the *S Continuum*, with strong to weak similarity relations between T_R and its base-level derivable counterpart T_R* determining reduction to replacement, respectively.

But note the consequence. The more *dissimilar* T_R is with respect to T_R*, *the more appropriate it is for replacement*. Yet, by virtue of the complex many-many mappings between the kind terms of special science and the kind terms of physical science, *type irreducible special science theories are exceedingly dissimilar to anything derivable from base-level physical theory*. Hence, by the *Similarity Assumption*, these theories must be replaced – surely the wrong result. Happily, in two sections hence I will propose a broader continuum from reduction to replacement where "similarity" is replaced by formal conditions of "connectedness" to base-level theories, which allows viable or at least useful type irreducible theories to be retained in spite of their dissimilarity to base-level theories.

VII. The New Wave View of Token Reduction

At first glance, some members of the New Wave appear to look favorably upon the practice of token reduction. In particular, Hooker (1981, pp. 504-507) outlines an account of "function-to-structure token reduction," and Bickle (1998, pp.155-163) endorses it. But this New Wave token reduction differs from the standard variety proposed by Fodor and Kimbrough. Simply put, unlike the standard account, New Wave token reduction appears to be a guised form of eliminativism. I have presented the details elsewhere, so I will be brief (Endicott, 2001, pp.388-391; see also Wright, 2000). Hooker illustrates New Wave token reduction by employing something roughly parallel to the familiar tripartite taxonomy employed within cognitive science -- the semantic (L_1), syntactic (L_2), and the physical mechanistic (L_3) levels of description. He constructs a target theory T out of higher-level L_1 and L_2 predicates, and the basic-level theory T* out of mechanistic L_3 predicates, claiming that:

> Systems of a type S of class T are contingently token/token identical with systems of type S' in class T* $=_{df}$ every instance (token) of a type S system externally classified as in class T is contingently identical with some instance (token) of a type S' system externally classified as in class T* (1981, 504).

So far so familiar. But the standard idea of token identity involves *the same object exemplifying two distinct properties*, a token identity with types distinct. In an inter-level case, this means *property dualism*, the same object having both an irreducible higher-level property and a lower-level property. Yet the New Wave rejects property dualism. So New Wave token reduction must be something else called "token reduction," and it seems to be a version of eliminativism. When considering the possibility that each functional property is either (i) type reducible by being directly identified with a mechanistic property or (ii) token reducible by having its instance identified with an instance of a mechanistic property, Hooker says that "there are no properties corresponding to predicates falling under case (ii) above if by this one means a single property common to all instances," and that such cases may require "resisting putative L_1 + L_2 semantics" (1981, p.507). Similarly, Bickle says: "In such cases we conclude that the functional-level predicates fail to denote" (1998, p.162). So this is no familiar token reduction where the same object exemplifies two distinct properties. Only one property is retained, and thus only its tokens remain.

The latter point is worth emphasizing. On the New Wave scheme *there are no tokens of the type irreducible properties*. As I stated before, if there is no phlogiston, then nothing is token identical with phlogiston (Endicott, 2001, p.390). Aside from the types and tokens of T's observational consequences that are better explained by T*, there are just the items picked out by mechanistic-level predicates, including perhaps some functional-level predicates that have been semantically "reconstructed" to denote mechanistic-level properties (Hooker, 1981, pp. 507-12). So the label "token reduction" is justified, apparently, by the lingering use of some reconstructed functional predicates.

This eliminative interpretation of New Wave token reduction also accords well with the New Wave tendency to view higher-level irreducible theories as radically false targets for elimination. [7] For example, Churchland (1981) has famously argued that propositional attitude psychology should be eliminated. And, although Bickle (1998, pp.205-206) describes a more nuanced revisionary position according to which folk psychology errs about the fine-grained causal structure but succeeds in describing the "gross abstract structure" of cognition, given the exhaustive reading of "reduction or replacement" discussed earlier, Bickle must believe that the limited set of true and successful descriptions from folk psychology pick out properties that reduce to neurochemical properties.

However, like Schaffner's pragmatic attitude toward presently type irreducible theories, the outlook of the New Wave toward the broader practice of science also permits the use of type irreducible theories for token reduction. Thus, Bickle's seemingly ruthless claim that psychological explanations "become otiose" and are rendered "impotent" by neuroscientific theories presupposes an "*accomplished* (and not just an anticipated) cellular/molecular explanation" (2003, p.110, his emphasis). Moreover, even when base-level (presumably reductive) explanations have been achieved, Bickle allows "some residual, purely heuristic tasks" (loc. cit.). Consequently, it is consistent with Bickle's views that when base-level reductive explanations have *not* been achieved, one may retain presently type irreducible theories for heuristic reasons. But this kind of philosophical outlook is no eliminativism. For example, instrumentally interpreted theories are neither reduced nor replaced. Like their realist counterparts -- property dualist theories -- they are retained in their presently irreducible forms precisely because of their convenience and overall utility. My proposal will thus make room for more conciliatory pragmatic and instrumental attitudes by expanding the *S Continuum* so that it is no longer exhausted by theories that are either wholly reduced or replaced or broken down into parts that are reduced or replaced.

VIII. The Space Between Reduction and Replacement

To avoid confusion with the kind of "retention" that occurs with type reduction, let "reception" designate the space between reduction and replacement where type-irreducible theories are retained for the purpose of token reduction. Moreover, call the continuum that contains a space for the reception of type-irreducible theories the *Continuum from Reduction to Reception to Replacement*. I will sketch, in programmatic fashion, some necessary conditions that a pair of theories must satisfy for placement along this *3R Continuum*. Intuitively speaking, the *3R Continuum* registers the degree of "connectedness" between basic and nonbasic theories. Being mindful that the meaning of the subscript '$_R$' for theory T now varies in one of three ways -- reduced, received, and replaced -- the conditions for placement are as follows.

R1. Nonbasic T_R is a reduced theory. The general terms of a nonbasic theory positioned at the reductive end of the *3R Continuum* are connected to the general terms of the base-level theory in the way required by *S Deduction*. There is a direct (for T_R) or approximating (for T_R*) derivation from a base-level T_B by means of connecting principles that express property identities between the two theoretical domains. Since type identities are established, the token identities are secure.

R2. Nonbasic T_R is a received theory. Approximating revisions no longer secure type reduction. The general terms of a nonbasic theory positioned at the receptive area of the *3R Continuum* are no longer connected to the general terms of the base-level theory by principles that express property identities between the two theoretical domains. But there exist connections between the properties supplied by nonreductive determinative relations like supervenience (Kim, 1984; Horgan, 1993) and realization (Endicott, 2005). So the properties of T_R, or the properties of T_R* when the former has been corrected, are determined by the properties of the base-level T_B in a way compatible with their nonidentity. Yet there remain connections between the singular terms of the nonbasic theory and the singular terms of the basic theory that establish token identities, and these connections conform to the token reduction functions presented earlier.

R3. Nonbasic T_R is a replaced theory. Whether general or singular, very few terms of the nonbasic theory positioned at the replacement end of the *3R Continuum* are connected to the terms of the base-level theory. The ontology is no longer anchored into the world. The types and tokens of T_R are thus eliminated in favor of the types and tokens of the base-level T_B that, by virtue of its superior resources, provides a better explanation of the observable phenomena originally targeted by the displaced T_R.

So I have replaced talk of strong to weak analogies between nonbasic and base-level derivable theories with a more precise analysis in terms of the kind of connections between them, and I have replaced the exhaustive reading of reduction or replacement with a broader set of conditions that allow the reception of type irreducible theories that are subject to token reduction. However, my aim is *not* to portray a broad nonreductive view of the sciences but to expand the range of partial reductions *via* token identities. Put in a different way, I have not simply reinterpreted "unification" in a nonreductive way that allows for the supervenience or realization of type irreducible theories. That misconceives the point entirely. As a conservative extension of the models under consideration, “unification” still means ontological unification *via* cross-theoretic identities. Thus, in the service of this august reductive goal, and in summary fashion:

> 3R CONTINUUM: there is a continuum of connections between the nonbasic T_R and its base-level derivable counterpart T_R*, with type-identities justifying type reduction, token identities justifying token reduction, and the lack of such identities justifying replacement of the ontology of T_R.

The *3R Continuum* describes a very broad inter-level slice of the world. But this is as it should be. Reduction and other forms of scientific unification cannot be considered in isolation from the larger body of scientific practice in which they occur.

IX. Concluding Remarks

My primary goal has not been to place particular theory pairs on the *3R Continuum*, or contest their placement. Granted, particular cases and general theories must be brought into reflective equilibrium. But my focus has been on developing a general theory of scientific unification from reduction to replacement by exploring how the possibilities of token reduction can be situated within that framework. Yet the matter of token reduction is not trivial, and there are a number of problems that should be resolved once the general outline of a model has been presented. Thus, I will end by citing one problem, then briefly indicate how it might be addressed, leaving a fully adequate discussion of these points for another time.

Simply put, there is a problem about *token correspondence for inter-level theories*. As previously mentioned, without type-correspondence, there is no guarantee that two theories drawn from different scientific levels of inquiry will carve up the world into the same naturally isolated particulars that are subject to the same explanations and predictions. Focusing on the psychophysical case, Stephen Stich presents a list of authors from Winograd to Minsky who offer models of cognitive phenomena wherein “no single component or naturally isolated part can be said to underlie the expression of a belief or desire” (1983, p.240). [8] Indeed, semantic networks are often thought to display the phenomenon in question, since there may be large areas of computational activity that are shared by several belief states all at once so that, if particular beliefs exist, they arise from the base-level states in a more holistic fashion. Moreover, a more general token correspondence problem seems to be a natural consequence of the fact that, given a mereological account of levelhood from the microphysical to the macrosocial, each level *n* of scientific inquiry typically involves much larger aggregates of particulars than the more basic level *n-1*.

In response, one might endorse a solution that utilizes constructivist procedures, composing aggregates of base-level objects in a way that preserves token identities. The smaller particulars of the base-level theory are combined by logical operations into aggregates that are token identical with the particulars of the nonbasic theory. But, until more is said, these constructed aggregates may not play any role in the base-level theory -- no more than financial institutions and labor forces play in fundamental physics. I prefer a more scientifically inspired solution.

As a start, one might concede that token reductions are rarely accomplished because of the different goals and principles that operate at different levels of scientific inquiry. Perhaps token reductions apply only at the borderline of neighboring disciplines where interests converge. Nevertheless, there is a process that can bring them about, namely, the process of co-evolution when it adjusts two neighboring theories until a mechanism is discovered whereby some ontology of the one theory realizes or implements some ontology of the other.

More specifically, co-evolution is a process where the concepts and languages of two theories at different levels of inquiry develop together and mutually influence each other (Wimsatt, 1976, pp.230-237; see also McCauley, 1996). For example, neuroscientists employ psychofunctional criteria to guide the identification brain structures, while cognitive scientists employ information about brain structures to guide their theories of information processing (there is a nice discussion in Bechtel and Mundale, 1999). This co-evolution may continue until, by the continued calibration of one theory to the other, a base-level mechanism is discovered which supplies the now naturally isolated unit for token reduction. That is, the base-level mechanism yields a token of the pertinent nonbasic realized type. This idea can be captured by a familiar form of functional explanation. Employing the symbolism of the token reduction functions presented earlier, one begins with a *functional analysis*: x has a nonbasic property F = x has something that plays a given functional role R. Then one utilizes *the* *empirical discovery* that a type of base-level physical mechanism P plays the functional role R. And from this one can draw *the conclusion*: if x has the mechanism property P then x has the nonbasic property F. In other words, one can deduce that the *same x* has properties F and P -- the desired token identity. To illustrate with a concrete case, consider the Crick-Koch Hypothesis (1990) that the mechanism for human visual awareness is 40-70 Hz neural oscillations in the human cortex:

> (i) x has visual awareness = x has something that plays the functional role of controlling information from the visual input system in a global fashion with respect to attention, short-term memory, and behavioral outputs.
> (ii) x has 40-70 Hz oscillations of neurons in the human visual cortex => x has something that plays the functional role of controlling information from the visual input system in a global fashion with respect to attention, short-term memory, and behavioral outputs.
> (iii) x has 40-70 Hz oscillations of neurons in the human visual cortex => x has visual awareness. [9]

Again, *the same x* has the neural property and the functional awareness property. Interestingly, the foregoing argument also reveals a merger of the two traditions of reduction mentioned at the outset of this paper. Reduction as mechanistic explanation and reduction as ontological unification *via* cross-theoretic identity meet at the point of token reduction.

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Notes

[1] Sklar gives a more cautious reading, saying that Nagel's text "appears" to signal a change: "Originally Nagel insisted upon theses correlatory hypotheses being universally quantified bi-conditionals, with one side of the bi-conditional containing as its only descriptive term one of the terms peculiar to the reduced theory. Subsequently he appears to have weakened this condition, allowing the laws to take other forms as well" (Sklar 1967, p.118).

[2] Patricia Kitcher (1980) also emphasizes this point in her response to Richardson. I add that both Schaffner's General Reduction-Replacement model and the basic New Wave approach likewise associate reduction with cross-theoretic identities. For Schaffner, the identities surface in the interpretation of the connecting principles that facilitate the derivation of either the original or corrected targets for reduction (1967, p.144). For the New Wave, the identities surface as a consequence of a comparative smooth analogy between the original and corrected theories (P.M. Churchland, 1985, p.11; Hooker, 1981, p.45; P.S. Churchland, 1986, p.284; Bickle, 1998, pp.77-78). Indeed, I have shown that these ontological consequences imply traditional biconditional bridge laws that license cross-theoretic property identities (Endicott, 1998a, pp.67-72). Perhaps for this reason, Bickle (2003, pp.31-39) has recently adopted a Carnapian-inspired, "internalist," "metascientific" attitude that rejects "metaphysical" identity questions altogether. I do not have the space to address Bickle's return to Positivist themes here.

[3] The label was first applied by Bickle (1996, p.57).

[4] Other New Wave proposals are less central in the sense that they vary from one New Wave advocate to the next. For example, both Paul Churchland and Bickle adopt a nonsentential view of scientific theories. But Churchland (1990) prefers to think of them in the connectionist tradition as vectors through an abstract state-space, while Bickle (1998) prefers to think of them in the semantic tradition as model-theoretic structures.

[5] Maurice Schouten has understandably requested that I clarify my charge about co-evolution, since advocates of the New Wave also accept the co-evolution of theories. My earlier argument was that their account of co-evolution appears inconsistent with *CH Construction*, given that the “mutual” interplay of co-evolution includes the top-down role of nonbasic concepts in constructing the corrected theory T_R* (Hooker, 1981, pp.513-514; Bickle, 1996, p.76, 1998, pp.148, 201). In the very least, if one accepts top-down influences of a nonbasic T_R on a resulting base-level T_R*, then, as I stated before, *CH Construction*’s advertised exclusion of the concepts and language of T_R must appear superficial from the historical development of the sciences, since it ignores the interplay between basic and nonbasic theories (Endicott, 1998a, p.66).

[6] Hull (1974, esp. pp.38-42) stressed the complicated many-many mappings for molecular genetics. For a general analysis of inter-level one-many and many-one relations, see Endicott (1994, 1998b).

[7] Compared to the eliminative spirit that seems to underlie his earlier remarks on function-to-structure token reduction, Hooker’s later work expresses a “naturalism” that does not require reduction, being consistent with “dualisms of many sorts” (1987, p.261).

[8] In addition to the sources cited by Stich, see also Horgan and Tye (1985). Given the failure of token correspondence, some have drawn anti-realist conclusions about the ontology of the higher-level theory. But, as Horgan and Tye (1985, p.438, fn.13) observe, that conclusion can be resisted.

[9] Note that the second premise and the conclusion are couched in terms of one-way conditionals that express mere sufficient conditions. If they were biconditionals or the stronger connections of identity, one would have a type reduction. Crick and Koch apparently take the mechanisms to license type reduction, I do not. I think there can be visual consciousness realized by some mechanism other than t40-70 Hz neural oscillations.

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