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# Structuralist Contributions – and Limitations? – to the Study of Scientific Reduction[*]

*John Bickle*[†]

## Abstract

Structuralism provides useful resources for advancing our understanding of the intertheoretic reduction relation and its place in the history of science. This paper begins by surveying these resources and assessing their metascientific significance. Nevertheless, important challenges remain. I close by arguing that the reductionism implicit in current scientific practice in a paradigmatic reductionistic scientific field –"molecular and cellular cognition"– is better understood on an "intervene and track" model rather than as any kind of intertheoretic relation. I illustrate my alternative model by describing briefly a recent reductionistic result from the field. It appears doubtful that any structuralist resources will illuminate this newly-recognized type of reduction-in-actual-scientific-practice.

*Keywords:* structuralism - reduction - practice - cognition

## Resumen

El estructuralismo ofrece recursos útiles para avanzar en la comprensión de la relación de reducción interteórica y su lugar en la historia de la ciencia. Este artículo comienza con la topografía de estos recursos y la evaluación de su importancia metacientífica. Sin embargo, quedan aún retos importantes. Cierro el artículo con el argumento de que el reduccionismo implícito en la práctica científica actual en un campo científico reduccionista paradigmático –"cognición molecular y celular"– se entiende mejor en un modelo de "intervención y realización de seguimiento de" y no como algún tipo de relación interteórica. Ilustro mi modelo alternativo al describir brevemente un resultado reciente de reducción en ese campo. Parece dudoso que alguno de los recursos estructuralistas iluminen este nuevo-tipo reconocido de reducción-en-la práctica-científica-real.

*Palabras clave*: estructuralismo - reducción - práctica - cognición

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# 1. A Selective Survey of Quarter-Century's Work on Intertheoretic Reduction

The mistaken assumption that philosophical work on scientific reduction began and ended with Ernest Nagel's (1961) theory of intertheoretic reduction is implicit in much Anglo-American philosophy, especially philosophy of mind.[1] Numerous alternatives to Nagel's account were widely discussed in mid- to late-20th century philosophy of science. So I begin with a selective and very brief survey of some of the key developments in the reduction literature, as background to what the structuralist program contributes.

Nagel (1961, Chap. 11) famously viewed reduction as logical deduction: as derivation of the laws or explanatory principles of the reduced theory from those of the reducing. He characterized theories syntactically, as sets of sentences. Of course, he realized, to the point of explicit mention, that actual scientific reductions often correct the reduced theory. So the premises of the derivations often require counterfactual limiting assumptions and boundary conditions on the scope of the reducing theory. And he realized that interesting cases of scientific reduction often involve theories with non-overlapping descriptive vocabularies. He called these cases "heterogeneous reductions" and noted that his favorite illustration, the reduction of a portion of classical equilibrium thermodynamics to statistical mechanics and the corpuscular theory of gases, was one of these. Hence the premises of the reduction complex also often requires *bridge principles* linking the disparate vocabularies of reduced and reducing theory, in order to permit something more than a trivial, vacuous deduction of the former from the latter.

Nagel's model was philosophically powerful. It fit squarely within the logical empiricist philosophy of science of its time and fully deserved the high regard it acquired. But it was neither the first nor the last word on intertheoretic reduction.[2] At least two influential accounts preceded it. John Kemeny and Paul Oppenheim's (1956) account stressed explanation by the reducing theory of all of the reduced theory's observational evidence (and typically more-observational evidence that the reduced theory could not explain). Patrick Suppes's (1956) account stemmed from his alternative, set-theoretic account of the structure of theories. For Suppes an intertheretic reduction was a discovered isomorphism (in the mathematical, sameness-of-structures sense) between the models comprising the reduced theory and a subset of those comprising the reducing. Within a year of the publication of Nagel's book, Paul Feyerabend (1962) published a now-clas-

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[1] One exception to this common assumption is recent work on "functional" reduction (e.g., Levine 2000, Chalmers 1996, Kim 2005). However, these accounts have only captured the attention of philosophers of mind, and rightfully so since they suffer even more acutely from the lack of precision and applicability to real scientific examples I'll discuss in this paper. I won't discuss functional reduction in this essay. For some details of functional reduction's lack of genuine connection to reduction in actual scientific practice, see my Bickle (2012).

[2] In my Bickle (1998, Chaps. 1 and 2, and 2003, Chap. 1), I provide more details of the accounts and their difficulties surveyed briefly here.

sic paper that characterized reduction as replacement of the reduced theory's ontology by the reducing theory's "incommensurable" alternative.

This first decade of serious philosophical work on intertheoretic reduction was summarized elegantly by Ken Schaffner (1967). In that paper Schaffner also provided his own General Reduction Paradigm (later expanded into his 1992 General Reduction-Replacement Paradigm) that yielded each of the surveyed alternative approaches as a special case. A bit over one decade later, Clifford Hooker (1981) produced another "general" theory of reduction that sought to incorporate some insights of earlier accounts within some new resources

Hooker's account differed from Schaffner's in a handful of crucial ways and it is worth dwelling briefly on some of these differences. For both, deduction remains paramount. But unlike Nagel, neither thought that what gets deduced in an intertheoretic reduction is the reduced theory itself. For Schaffner, it is a corrected version of the reduced theory. His account thus still required bridging principles to connect disparate cross-theoretic descriptive vocabularies. (Schaffner dubbed these "reduction functions" instead of the potentially misleading terms, "bridge laws" or "correspondence rules.") Reduction functions required empirical support and expressed referential identities. The corrected version of the reduced theory, the structure that got deduced, had to make more accurate, experimentally verified predictions than did the actual reduced theory. And the corrected and actual reduced theories had to stand in a relationship of "strong analogy"– although this was a relationship that Schaffner has never succeeded in specifying.

According to Hooker, what gets deduced in a reduction is an explanatorily equipotent image of the reduced theory, already specified within the vocabulary and conceptual framework of the reducing theory. To handle corrections implied by a given reduction, he recognized that counterfactual limiting assumptions and boundary conditions must often be conjoined with the reducing theory as premises in the derivation of the image. But no bridging principles or reduction functions are needed, since the deduced image is already specified within the vocabulary and framework of the reducing theory. There are thus no disparate vocabularies to be "bridged." This image and the actual reduced theory must stand in some relationship of "analogy"– but like Schaffner, Hooker never made much headway toward articulating this relationship.

These comparisons and contrasts between Nagel's, Schaffner's and Hooker's accounts thus reveal the following strengths and weaknesses. The premises of the deductive component of Nagel's account consist of the reducing theory, (often counterfactual) limiting assumptions and boundary conditions, and bridging principles. Schaffner's account dispenses with the problematic counterfactual limiting assumptions and boundary conditions by changing the conclusion of the deductive component to an already-corrected version of the reduced theory. Hooker's account dispenses with the problematic bridging principles or reduction functions by changing the conclusion to an image of the reduced theory already specified within the vocabulary and framework of the reducing theory. On the other hand, Nagel wasn't left with some problematic "analogy" relation between the conclusion of the deductive component of an intertheoretic reduction and the actual reduced theory, as both Schaffner and Hooker were.

To their credit, both Schaffner's and Hooker's general accounts could explain the spectrum of ontological consequences that obtain in historical scientific reductions. These consequences range from identity (the visible light of physical optics is electromagnetic radiation within a range of wavelengths and frequencies) through increasingly significant conceptual revision (the heat of classical equilibrium thermodynamics isn't quite mean molecular kinetic energy of a gas's constituent molecules, at least not for any actual finite molecular ensembles) to outright elimination (there is no such thing as phlogiston or caloric fluid). (See the bottom half of Figure 1.) For both Schaffner and Hooker, the nature of the ontological consequences obtaining in different historical intertheoretic reductions followed from the amount of correction entailed to the reduced theory via the reduction. (See the top half of Figure 1.) For Schaffner, this measure had to do with the strength of the analogy that obtained between the reduced theory and the corrected version of it derivable from the reducing theory. For Hooker, the amount of correction had to do with the strength of the cross-theoretic analogy obtaining between the deduced image and the reduced theory and the counterfactual extent of the limiting assumptions and boundary conditions required to derive the equipotent image. In principle, this result on both accounts constituted an important advance. One standard criticism of Nagel's account, aired almost immediately upon its publication by "radical" empiricist critics such as Feyerabend, pointed out the artificial contortions his account required to handle actual historical scientific reductions that deviated from those near the "smooth," "retentive" reduction endpoint (see the top half of Figure 1). Unfortunately, however, for both Schaffner and Hooker, this "advance" depended heavily on their problematic "analogy" relations, which neither succeeded in articulating with any degree of precision or satisfaction.

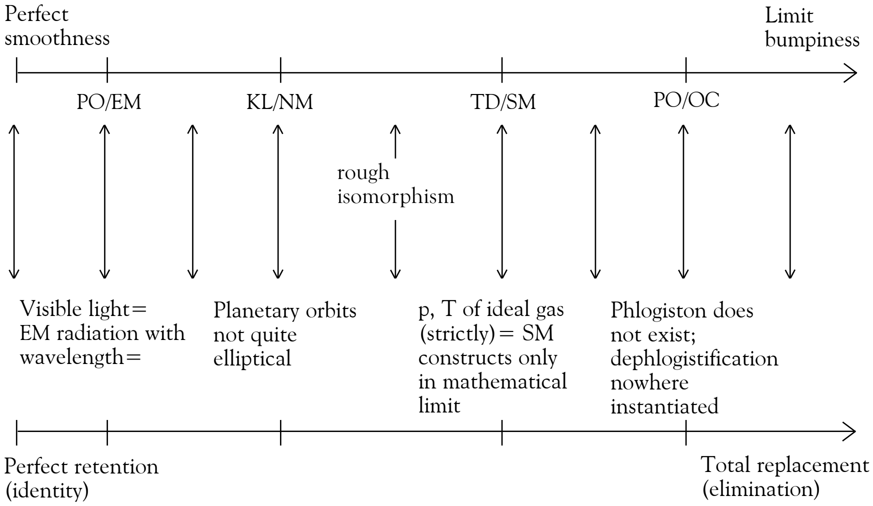

**Figure 1.** Top arrow: the intertheoretic reduction spectrum. Bottom arrow: the ontological consequences spectrum. Historically prominent scientific reductions are aligned on both spectra according to the amount of correction implied to the reduced theory (top arrow) and ontological consequences for key explanatory kinds posited by the reduced theory (bottom arrow). PO: physical optics (wave theory of light). EM: Maxwell's electromagnetic theory. KL: Kepler's laws of planetary motion. NM: Newtonian (classical) mechanics. TD: classical equilibrium thermodynamics. SM: statistical mechanics. PC: phlogiston chemistry. OX: oxygen chemistry. Reprinted from Bickle (1998, Figure 2.1, p. 30), with permission from MIT Press.

Problems for both Schaffner's and Hooker's alternatives didn't end with their shared failures to specify precisely the analogy relation. Numerous key aspects of both accounts remained distressingly programmatic. Hooker's own assessment of his account's shortcomings is notable. He realizes that formal representations of the preservation of the roles of properties and objects in laws across reduced and reducing theories are crucial. He even hints at possible mathematical measures of property and object preservation based on common intuitions about case comparisons: "For example, intuitively the local preservation of Euclidean structure in General Relativity is more nearly a preservation than is the retention of algebraic structure between thermodynamics and statistical mechanics" (1981, p. 224). Yet his upshot is brutally honest: "All of this is very programmatic and as yet lacking in deep yet simple insight" (1981, p. 224). Hooker never returned to his General Account of Intertheoretic Reduction to make good on these lacunae.

## 2. Structuralism to the Rescue

I've argued (Bickle 1998, Chap. 3) that structuralist philosophy of science provides resources to fill these lacunae. Previous work by structuralists had yielded both a Nagel-inspired model of the intertheoretic reduction relation (Balzer, Moulines & Sneed 1987) and a "radical empiricist"-inspired account (Mayr 1976). Both of these accounts were developed using accepted structuralist standards for formalization and rigor.[3] A theory is characterized as an ordered set of potential models, models, and intended empirical applications, with its models characterized via a set-theoretic predicate. Clauses of that predicate's definition specify the theory's entities, relations, and basic laws. Potential models are set-theoretic structures that meet all the non-lawful conditions. Intended empirical applications are real-world systems to which the theory is expected to apply (i.e., potential models that are expected or have already been shown to be actual models). (See Figure 2.) Any model of a theory consists of the theory's real ("empirical") and auxiliary (mathematical) base sets and the relations or functions typified by them. An intertheoretic reduction relation $\rho$ is defined as a set of ordered pairs whose domain is the potential models of the reducing theory and whose range is the potential models of the reduced

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[3] Since the formal details of the ideas discussed in this section have been presented in numerous prior publications, I'll refrain from re-presenting many of them here. Readers interested in those details should consult the cited literature, especially Balzer, Moulines & Sneed (1987) and Bickle (1998, Chap. 3). Many standard features of the structuralist program in philosophy of science will be oversimplified significantly here, since those details don't pertain directly to the points at issue.

theory. The structuralist Nagel-inspired account adds conditions that restrict $\rho$ in ways that mimic Nagel's conditions of "connectability" and "derivability." The structuralist radical empiricist-inspired account adds a set-theoretic definition of "anomaly" and a condition on the reducing theory requiring it to "explain the anomalies" of the reduced theory.

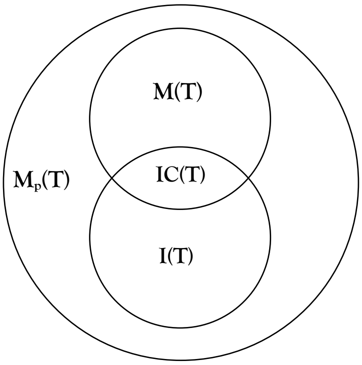

**Figure 2.** The basic structuralist account of theory structure. $\mathbf{M_p(T)}$ is the set of **T**'s potential models. **M(T)** is the set of **T**'s models (where $\mathbf{M(T)} \subseteq \mathbf{M_p(T)}$). **I(T)** is the set of **T**'s intended empirical applications and **IC(T)** (= $\mathbf{M(T)} \cap \mathbf{I(T)}$, possibly $\varnothing$) is the set of intended empirical applications that have been confirmed to be actual models of **T** (i.e., to meet the lawful conditions on **T**'s set-theoretic predicate). Reprinted from Bickle (1998, Figure 3.1, p. 64), with permission from MIT Press.

Two important consequences for any account of reduction follow immediately from even this rudimentary structuralist characterization. First, being a *relation* from models of the reducing theory into models of the reduced, $\rho$ can be many-one. That is, the ordered pairs comprising it can have many distinct models from the reducing theory as the first element conjoined with one and the same model from the reduced as the second. Philosophers of mind and psychology will recognize this feature as "multiple realizability," where distinct reducing kinds can realize one and the same reduced kind. (Kinds are here characterized as empirical base sets and relations composing the models of the reduced and reducing theories.) Multiple realizability has been a popular anti-reductionist argument in the philosophy of mind since the 1960s. Yet by itself multiple realizability is no barrier to reduction on either structuralist account, owing to this relation condition on the reduction relation $\rho$. Second, as I also pointed out before (Bickle 1998, p.73), structuralist accounts show that the precise point of conflict between Nagel's and his "radical empiricist" critics' accounts of reduction holds between the former's "derivability" condition and the latter's "resolution of anomalies" condition – not between Nagel's "connectibility" condition and the latter, as some philosophers of science assumed. Hence many of the disputes about 'bridging principles" that exercised so many mid-20th century philosophers of science were built upon a misunderstanding that structuralist formalization clears up.

An additional structuralist resource also provides both of its accounts of reduction with a way to address a problem Schaffner had first emphasized for Suppes's account:

> My […] contention, that the Suppes paradigm is too weak as it stands, is supported by the fact that different and nonreducible (at least to one another) physical theories can have the same formal structure – e.g., the theory of heat and hydrodynamics – and yet one would not wish to claim that any reduction could be constructed here. The claim then is that isomorphism is necessary, but not sufficient for reduction. Accordingly, I do not think the Suppes approach is one which is workable, without some additional criteria of reduction conjoined to it. (Schaffner 1967, p. 145; emphasis added).

Schaffner's challenge is immediately relevant for structuralist models of reduction. Since the inception of the program, structuralists acknowledged their debt to Suppes. Of course, both structuralist models of reduction add "additional criteria" beyond isomorphism on relation $\rho$. But even these aren't enough to stave off Schaffner's challenge. Prominent structuralist C.U. Moulines raises exactly Schaffner's challenge for the structuralist models (although he doesn't mention Schaffner in raising this challenge):

> I wish to argue that, for a complete picture of a reductive relationship between two theories, one has to take into account some sort of relation between the respective domains. Otherwise, when confronted with a particular example of a reductive pair, we would feel that all we have is an ad hoc mathematical relationship between two sets of structures, perhaps by chance having the mathematical properties we require of reduction, but not really telling us something about "the world." We could have a reductive relationship between two theories that are completely alien to each other. (Moulines 1984, p. 55).

Moulines's point here is exactly Schaffner's, in the latter's "heat-to-hydrodynamics" example in the extended quote at the beginning of this paragraph.

Moulines's solution is to introduce a new structuralist resource in order to analyze the structure of the global reduction relation $\rho$. He construes $\rho$ as being constructed (in part) of cross-theoretic *links* between "ontological constituents" of models of the reduced and reducing theories. Consider some $\rho \subseteq \mathbf{M_p}(\mathbf{T_B}) \times \mathbf{M_p}(\mathbf{T_R})$ (where $\mathbf{M_p}(\mathbf{T_B})$ is the set of potential models partly comprising the reducing ("basic") theory $\mathbf{T_B}$ and $\mathbf{M_p}(\mathbf{T_R})$ the set partly comprising the reduced theory $\mathbf{T_R}$). Moulines defines $\rho$ as being composed of "ontological reductive links" (ORLs) just in case it meets all the conditions on a structuralist reduction relation $\rho$ *and* is composed in part by relations between each of the base sets of entities constituting models of $\mathbf{T_R}$ and at least some of the base sets of entities or relations constituting models of $\mathbf{T_B}$. He offers examples from historical reductions in science to illustrate ORLs. In the reduction of rigid body mechanics to Newtonian particle mechanics, the base sets of space points and time points are linked across the two theories, while the base set of rigid bodies is linked to that of Newtonian particles.[4] In the reduction of Newtonian particle mechanics to special relativity theory, sets of particles get linked across $\rho$-related models of the two theories. Elements of the separate Newtonian base sets of space points and time points get linked (heterogeneously – see footnote 4) to the single base set of Minkowskian spacetime points. In some reductions, "atomic" base sets of the reduced theory get linked to structured combinations and sequences of entities and relations of the reducing. For example, in the reduction of Mendelian genetics to molecular genetics, elements of the base set of genes in models of the former get linked (heterogeneously again) to sequences of organic molecules and the relations that make up the processes of gene expression, protein synthesis, and genotype-to-phenotype development and transition.

Adding the condition that $\rho$ must be composed of ORLs to the conditions on structuralist reduction relations meets Schaffner's challenge. Any actual or contrived cases of cross-theory relations that happen to meet the conditions on $\rho$ but which are not genuine reductions will also fail to be composed of genuine ORLs. In Schaffner's example, there are no ORLs – homogeneous or heterogeneous – between elements of the base sets of our accepted theory of heat and those of base sets or relations of our accepted theory of fluid dynamics. Elements of those base sets obey similar mathematical equations, but they aren't linked "ontologically" across the two theories to compose (partly) any reduction relation $\rho$. A similar explanation holds for all Schaffner-inspired counterexamples to structuralist reduction relations, in which the non-ORL conditions on $\rho$ obtain but the reduction isn't "genuine."

ORLs also provide an account of how concepts of reduced theories sometimes get "structured through reduction" (Bickle 2002). This result obtains in cases where an unstructured "atomic" base set of entities of the reduced theory get linked by an (heterogeneous) ORL to combinations and sequences of entities and relations of the reducing theory. Such reduced kinds, characterized functionally by the reduced theory (that is, only by way of its relations and laws/generalizations), get related in a domain-eliminating way to sequences of entities, relations, and processes characterized by the reducing theory. To use some of the scientific examples presented above: there are no rigid bodies, separate space points and time points, or Mendelian genes subsequent to the accomplished reductions of rigid body mechanics, Newtonian particle mechanics, and Mendelian genetics – at least not in the way that there remain particles in special relativity theory and planets in Newtonian celestial mechanics. The former examples aren't part of how the reducing theory "carves up the world." However, the roles these reduced base sets play in the relations and laws/generalizations of the reduced theories bear interesting structural similarities to the roles played by the various elements of the reducing theory to which they are ontologically linked. As Moulines puts it, "the amorphous basic entities of the reduced theory become structured through reduction" (Moulines 1984, pp. 67-68), based on the specific ORLs they stand in to components of $\rho$-related models of the reducing theory.

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[4] Notice that while the first two links are identity relations, the third is not: elements of sets of rigid bodies constituting models of the reduced theory are not elements of any set of particles of any potential model of Newtonian particle mechanics. This is the basis of Moulines' (1984) distinction between "homogenous" and "heterogeneous" ORLs. See also Bickle (1998, Chap. 3) for a presentation and use of this distinction.

Finally, structuralism even provides a resource that generates a measure of the "amount of correction" that a given reduction implies to the reduced theory – and hence a precise account of the spectrum of intertheoretic reduction relations stressed by Schaffner and Hooker. I developed this resource (in Bickle 1998, chapter 3) explicitly to illuminate Hooker's account, but the results could easily be adapted to illuminate Schaffner's account. An image $\mathbf{T_R}^*$ is a subset of potential models of the reducing theory $\mathbf{T_B}$, in particular the ones meeting the specific limiting assumptions and boundary conditions necessary to mimic the explanatory power of the reduced theory $\mathbf{T_R}$. (See Figure 3.) Notice that if any of these assumptions and conditions is counterfactual, as occurs in reductions that imply even small corrections to $\mathbf{T_R}$, then at least some elements of $\mathbf{T_R}^*$ will not be actual models of $\mathbf{T_B}$. In cases that imply significant correction to $\mathbf{T_R}$, all of $\mathbf{T_R}^*$ might lie outside of $\mathbf{M(T_B)}$. (See Figure 4.) (I describe some historical scientific cases of this type in Bickle 1998, Chap. 3, Secs. 4 and 5.)

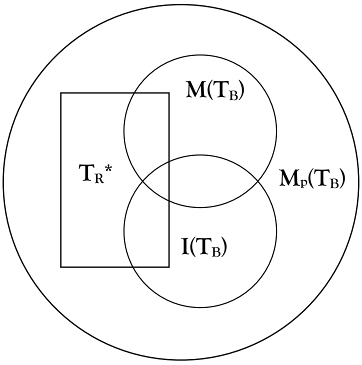

**Figure 3.** Image $\mathbf{T_R}^*$. The extent to which $\mathbf{T_R}^*$ intersects the models (and intended empirical applications) of the reducing theory $\mathbf{T_B}$ depends upon the extent and nature of the counterfactual limiting assumptions and boundary conditions required to mimic the structure and explanatory power of the reduced theory $\mathbf{T_R}$. Adapted from Bickle (1998, Figure 3.2, p. 66), with permission from MIT Press.

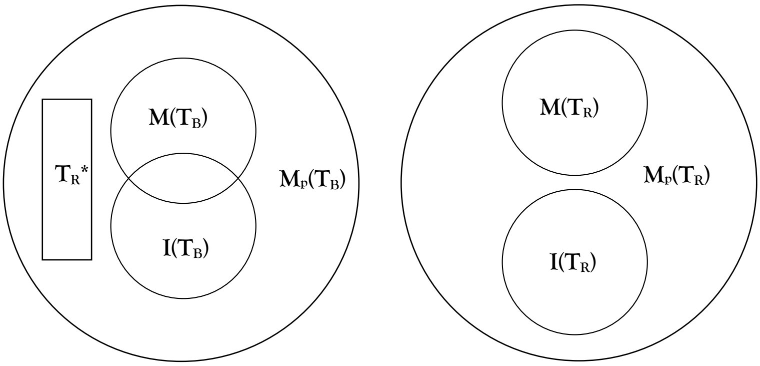

**Figure 4.** A "worst case" schema of a reduction implying significant correction to the reduced theory $\mathbf{T_R}$. The counterfactual limiting assumptions and boundary conditions required to construct an "equipotent" image $\mathbf{T_R}^*$ in the reducing theory $\mathbf{T_B}$ are so extensive and extreme that the intersection of $\mathbf{T_R}^*$ and the actual models (and intended empirical applications) of $\mathbf{T_B}$ is empty, as is the intersection of the actual models and the intended empirical applications of $\mathbf{T_R}$ (i.e., none of $\mathbf{T_R}$'s intended empirical applications turn out to be confirmed). Adapted from Bickle (1998, Figure 3.7, p. 89), with permission from MIT Press.

With this additional subset of the potential models of $\mathbf{T}_B$ characterized, I then redefined the conditions on both structuralist models of $\rho$ in terms of Hooker-inspired images $\mathbf{T}_R{}^*$ (Bickle 1998, Chap. 3). The question then arises: how "far away" from the set of actual models (and intended empirical applications) of $\mathbf{T}_B$ must $\mathbf{T}_R{}^*$ be located in order to meet the redefined conditions on $\rho$ in a given corrective intertheoretic reduction? Intuitively, the "closer" that $\mathbf{T}_R{}^*$ lies to $\mathbf{M}(\mathbf{T}_B)$ (and $\mathbf{I}(\mathbf{T}_B)$), the smoother the reduction. But how can we make sense of these distance metaphors? Here the structuralist notion of a *blur*, as part of an account of theory approximation, is suggestive (Balzer, Moulines & Sneed 1987, Chap. 7). Blurs are elements of *uniformities*, relations that impose topologies on unstructured sets. By extending the structuralist concept of *intra*theoretic blurs across theories, I showed how one can blur the models (and the intended empirical applications) of $\mathbf{T}_B$ into $\mathbf{T}_R{}^*$ (and the models into the intended empirical applications of $\mathbf{T}_R$), to meet all the redefined conditions on the two structuralist models of $\rho$ (Bickle 1998, Chap. 3, sections 4 and 5). (See Figure 5.) The extent of the blurs required on any given corrective reduction to capture all of the conditions on $\rho$ depends on specific details of the case. As an illustration I constructed the blurs required to handle corrections entailed by van der Waal forces in the reduction of a portion of classical equilibrium thermodynamics to statistical mechanics and the kinetic theory of gases (Bickle 1998, pp. 93-95).

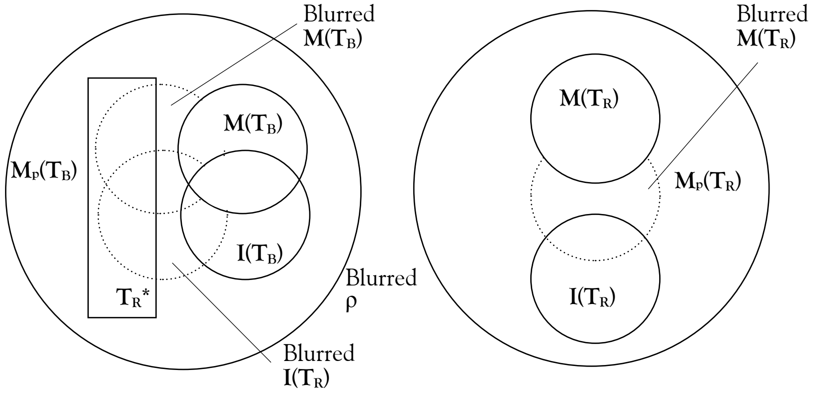

**Figure 5.** Blurring $\mathbf{M}(\mathbf{T}_B)$ and $\mathbf{I}(\mathbf{T}_B)$ into $\mathbf{T}_R{}^*$, and $\mathbf{M}(\mathbf{T}_R)$ into $\mathbf{I}(\mathbf{T}_R)$, in order to meet conditions on $\rho$ in significantly corrective intertheoretic reductions. Blurs are elements of uniformities, which impose a topology on unstructured sets, and thus have cardinality depending on the upper bound required to relate elements of the blurred sets to meet the conditions on $\rho$. Smoother intertheoretic reductions require blurs with smaller cardinality; bumpier reductions require blurs with greater cardinality. See text for additional details. Adapted from Bickle (1998, Figure 3.8, p. 90), with permission from MIT Press.

The spectrum of intertheoretic reduction relations stressed by Schaffner and Hooker is now subject to a quantitative measure of the "amount of correction" that obtains in particular cases. Being elements of uniformities, and hence sets of ordered pairs, each blur will have a cardinality based upon its upper bound. Larger blurs will be required to capture conditions on $\rho$ in more corrective reductions. The topology imposed by the uniformity will locate the elements of $\mathbf{T_R}^*$ related by $\rho$ to appropriate elements of $\mathbf{M_p(T_R)}$ further from $\mathbf{M(T_B)}$ (and from $\mathbf{I(T_B)}$); similarly for $\mathbf{M(T_R)}$ and $\mathbf{I(T_R)}$). The required blurs will have greater cardinality – they will contain more ordered pairs from the structures being blurred into one another. This feature provides a natural measure of the relative smoothness of an intertheoretic reduction. Smoother reduction require smaller blurs on $\mathbf{M(T_B)}$, $\mathbf{I(T_B)}$, and $\mathbf{M(T_R)}$ in order to meet the conditions on $\rho$. Bumpier intertheoretic reductions require larger blurs. Referring to the top half of Figure 1 above, we can say that the location of a given case on the intertheoretic reduction spectrum depends on the sizes of the bounds of the blurs required. A brief examination of the cases illustrated in Figure 1 bears out this suggestion (see Bickle 1998, pp. 97-98).

Let's take stock of these various ways that structuralism and its resources advanced our understanding of intertheoretic reduction.

- By characterizing intertheoretic reduction as a (set-theoretic) *relation* – and hence possibly many-one – from potential models of the reducing theory into potential models of the reduced theory, the resulting account strips multiple realizability of any anti-reductionistic force.
- Contrasting a Nagel-inspired structuralist account with a radical empiricist-inspired structuralist alternative reveals the actual point of conflict between these two general approaches to reduction: Nagels' condition of "derivability" (not his condition of "connectability") and the radical empiricists's emphasis on "incommensurability."
- Adding a condition that the structuralist intertheoretic reduction relation $\rho$ must be composed (partly) of ORLs addresses Schaffner's "too weak to be adequate" challenge to Suppes-inspired accounts of reduction.
- Adding the ORLs also provides a resource for characterizing precisely the way that a reduced theory's concepts often get "structured" through an intertheoretic reduction.
- With the help of a modified structuralist concept of a "blur," structuralist accounts of intertheoretic reduction can capture more precisely Hooker's (and Schaffner's) insights about the spectrum of reduction cases lying between the "smooth/retentive" and "bumpy/eliminative" extremes.
- Initial attempts to reconstruct actual scientific cases using structuralist resources strongly suggested that psychology-to-neuroscience cases were amendable to this treatment, and that reconstructing potential psychoneural reductions in this way rendered reductionism immune to some standard philosophical challenges (see especially Bickle 1998, Chaps. 4 and 5).

By any measure, these are substantial accomplishments. "New wave" psychoneural reductionism, borrowing and adapting much from structuralist work on reduction, looked very promising.

## 3. One Criticism That Stuck

Of course, not everybody was so convinced. (I surveyed a number of criticisms in Bickle 2003, Chap. 1.) One criticism, from proponents of the "new mechanical" philosophy of science, had real force because it appealed to actual scientific practice. Does neurobiology really produce *theories* in anything like the sense that new wave reductionism requires for structures to stand in the intertheoretic reduction relation $\rho$? Admittedly new wave reductionism abstracts away from actual scientific practice, as does the structuralist program it borrowed from. No one presents neuroscientific theories by first defining a set-theoretic predicate. But did new wave reductionism abstract away from actual science so far that nothing in the latter answers to the basic concept – that being 'theory' – in its account of intertheoretic reduction?

William Bechtel (2008, Chap. 4), for example, pointed out that a full characterization of the laws of the reducing theory is required on all accounts of intertheoretic reduction. Yet he is dubious that the required features are present in science's actual products. One reason has to do with what a "complete" reducing theory would have to provide. According to Bechtel, such a theory would have to specify how an entity would behave under all possible circumstances, for all the different mechanisms that it might be a component of. The problem with this requirement, however, is that it doesn't accurately reflect the way that a theoretical account is developed and used in any actual causal-mechanistic science. Typically scientists only seek to discover and use regularities in an entity's behavior under a very restricted range of conditions: what a protein does, for example – how it folds, what it binds to – within a narrow range of biological conditions, typically even more restricted than even the biologically possible. Notice that Bechtel's challenge goes beyond the applicability of structuralism's set-theoretic formalism for representing theories and intertheoretic relations, to the underlying notion of how comprehensive a "matured" theory must be to reduce another theory.

There is something excessive in Bechtel's characterization of what intertheoretic reduction demands of "matured" reducing theories. Do "matured" theories in even "developed" sciences – physics and chemistry, say – purport to provide theories in the sense he fails to find in the life sciences: specifying how entities behave in *all possible* circumstances? And more to our point, might structuralism already possess a resource to address a realistic version of Bechtel's challenge: intended empirical applications?[5] Recall that a theory's intended empirical applications are those real world possible models that scientists expect to be demonstrated to be actual models of the theory. This subset of possible models specifies the limits of the theory's intended application to the real world systems that scientists are willing to investigate, to determine if the theoretical conditions of the theory's set-theoretic definition hold. Its elements seem to be exactly the limited instances of a reducing theory's components and activities in actual scientific practice, the very ones Bechtel insists that intertheoretic reduction models can't stop at. Perhaps account-

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[5] I thank an anonymous referee for pointing out the logical strength of Bechtel's criticism and the possible use of structuralist intended empirical applications to address a realistic version of his challenge.

ing for Bechtel's challenge should be listed as yet another point in structuralist reductionism's favor, rather than as a criticism?

Unfortunately, the structuralist intertheoretic reduction relation $\rho$ does not just relate elements of the intended empirical applications of $\mathbf{T}_B$ to possible models of $\mathbf{T}_R$. And in significantly corrective reductions, it cannot: the $\mathbf{T}_R$* must be "blurred" significantly outside $\mathbf{T}_B$'s intended empirical applications. These "blurs" will relate possible models of $\mathbf{T}_B$ to those of $\mathbf{T}_R$ which are counterfactually quite distinct from $\mathbf{T}_B$'s intended empirical applications. So while perhaps hobbled by this structuralist response, Bechtel's criticism of intertheoretic reduction models still seems to have some bite. Reducing theories will have to be much developed in many counterfactual ways to produce a $\mathbf{T}_R$* which stands in $\rho$ to a "significantly corrected" $\mathbf{T}_R$; beyond the typical scope of counterfactual developments in actual scientific practice, even in some cases of acknowledged reductions from science's history.

Second, Bechtel (2008) insists that the picture of cross-theory relations that accounts of intertheoretic reduction (structuralist or otherwise) require is artificial, in that it separates cleanly the components of the "reduced" and "reducing" theories. But in real science, new components are often articulated through theory co-evolution. Interlevel collaborations, even ones championed by reductionists, typically don't start with some previously discovered, intact knowledge base, applied unchanged to new phenomena. Instead, new interlevel theoretical kinds comprise components of a new "interfiled theory" whose kinds cross levels with impunity. One could ignore these actual features of scientific development and practice, as Bechtel insists that reductionists of all varieties typically do. One could simply add all these co-evolutionary, cross-level discoveries into the body of theory at the lower (reducing) level. But that product would not be anything that is recognizably a part of actual science. In fact, that artificial construction would not even resemble knowledge as actually produced by lower level scientific disciplines. It would be instead a new construction – an artifact in the truest sense of the word. And it would be an artifact motivated solely by philosophical reductionistic concerns, not by any scientific demands or needs.

Here too the new wave reductionist has an interesting response, because of his structuralist account of intertheoretic reduction.[6] Nothing in the structuralist account requires base sets and functions for a given theory to all be drawn from the same intuitive scientific "level." The structuralist approach can easily accommodate so-called "interfield theories" (Darden & Maull 1977) if such theories are actually part of some science being formalized. Reductionists tend to part ways with mechanists on the latter's insistence on "nested hierarchies of mechanisms," at least as a correct account of the metascience of reductionistic neuroscience (see Bickle 2012). The structuralist response here can be neutral on this dispute between mechanists and reductionists: *if* multi-level interfield theories postulating nested hierarchies of mechanisms are part of a science, nothing precludes a struc-

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[6] An anonymous referee impressed upon me the strength of this response, too.

turalist reconstruction of them, in terms of the base sets and functions that constitute that theory's models.

The real motivation for moving away from a structuralist account of intertheoretic reduction, however, was *metascientific*. Mechanists helped bring this concern to wider attention with their insistence that we "start first with neuroscience" (Craver 2007). However, the metascientific attitude in philosophy of neuroscience goes back to Bickle (2003). So new mechanists and ruthless reductionists have jointly brought new attention to neuroscientific detail to philosophy of neuroscience, in both explanations and experiments. But new mechanists, like most philosophers, look most closely at results from cognitive, systems, and behavioral neuroscience (Bechtel 2009). There is another branch in contemporary neuroscience, a branch undeniably mainstream in terms of its funding, publications in major science and neuroscience journals, and other measures, which is unabashedly reductionistic. A textbook expression (literally) of this attitude is present in the introductory chapter of one of the discipline's most popular teaching and reference works. The authors, one a Nobel Prize laureate for his work on the molecular mechanisms of learning and memory, inform us that

> This book [...] describes how neural science is attempting to link molecules to mind —how proteins responsible for the activities of individual nerve cells are related to the complexity of neural processes. Today it is possible to link the molecular dynamics of individual nerve cells to representations of perceptual and motor acts in the brain and to relate these internal mechanisms to observable behavior. (Kandel, Schwartz & Jessell 2001, pp. 3-4)

These "links" are nothing less than proposed reductions of mental kinds to molecular mechanisms. There is now a professional society of neuroscientists, the Molecular and Cellular Cognition Society (www.molcellcog.org), one of whose principal goals is to promote the scientific study of "the molecular and cellular basis of cognitive function." "Ruthless" reductionism indeed!

When one adopts a metascientific attitude one begins to look more closely at a field's actual experimental practices. One feels incumbent not to abstract away too far from an analytical description of these practices. If scientific reduction(ism) is one's metascientific interest, one will look to the primary experimental literature of a reductionistic field, acknowledged as such by both the field's practitioners and scientists who work on related phenomena in less reductionistic fields (Bickle 2003, 2009a). Molecular and cellular cognition is an ideal field for metascientific psychoneural reductionist investigations. The metascientist will approach the landmark experimental publications in the field as unadorned with philosophical assumptions, attitudes, or even *tools*, as he or she can. This includes structuralist assumptions, attitudes, and tools. He or she will seek in the field's landmark experimental publications (acknowledged as such by the field's recognized experts) and provide an analysis of the exact experimental practices that distinguish that field's research from work in less reductive fields. If done properly, the result will be an accurate account of real-reductionism-in-really-reductionistic-neuroscience. Armed with such an account, we can then inquire whether it is amendable to more formalistic representation (if we're still so inclined to do so). A decade of doing metascience of molecular and cellular cognition has convinced me that real-reductionism-in-really-reductionistic-neuroscience is not a relationship usefully modeled using structuralist resources – at least not those drawn from structuralist models of inter*theoretic* reduction. Demonstrating this point will require a brief foray into a case study from the recent molecular neuroscience of cognition.

## 4. A Paradigmatic Case Study from Molecular and Cellular Cognition

The field of Molecular and Cellular Cognition emerged in the early 1990s with the application of gene targeting technology to produce engineered mutant mammals ("knock-outs," "transgenics"), especially mice. These mutants were then used in behavioral studies against non-mutated littermate controls to assess specific behavioral deficits. MCC is presently one of the hottest fields in neuroscience, in terms of funded research and published experimental reports in high-impact scientific journals. Space here precludes even a brief survey of this field, but since my latest account of reduction derives directly from this work (references cited at the beginning of the next section), I present a single case as a paradigmatic illustration: an early influential study of the molecular mechanisms of declarative memory consolidation. Consolidation is the process by which a labile, easily disrupted short term memory gets converted into more stable long term form. Ted Abel, working in Eric Kandel's lab in the 1990s, developed a transgenic mouse that overexpressed regulatory subunits of protein kinase A (PKA) (Abel *et al.* 1997).[7] Due to a clever choice of DNA insertion into the promoter region of the transgene overexpressing this protein subunit, even though the transgene was present in every cell in the mouse's body (since it was inserted during the embryonic stem cell developmental stage), its expression was limited to forebrain regions – neocortex, hippocampus, and other parts of limbic cortex to a lesser degree.

Regulatory subunits of PKA were chosen as the transgenic target because of the known role of this molecule in the transition from early- to late-long term potentiation (E-LTP, L-LTP), a well-studied form of activity-driven synaptic plasticity that has long been a candidate for a memory mechanism. E-LTP is a synapse-localized form of plasticity (enhancement) resulting from an initial influx of calcium ions across the post-synaptic membrane through special voltage-gated receptors (N-methyl-D-aspartate, or NMDA receptors). PKA plays little role in the mechanisms of E-LTP. The conversion of some forms of E-LTP to L-LTP requires both new gene expression and protein synthesis. PKA plays a crucial role in this cascade. A rise in post-synaptic cyclic adenosine monophosphate (cAMP) results from a sudden rise in intracellular calcium ions (bound up with calmodulin) and the priming of

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[7] I present an extensive background to this work – a description of the experimental techniques employed and their rationales, and a full report of the results, all written for molecular-biological novices – in Bickle (2003, Chap. 2).

adenylyl and adenylate cyclase molecules driven by activity at activated dopamine receptors. This process converts adenosine triphosphate (ATP) molecules into cAMP. (cAMP is the classic "second messenger" molecule of molecular biology.) These cAMP molecules bind to regulatory subunits of PKA molecules, freeing the PKA catalytic subunits to translocate to the neuron's nucleus. There PKA catalytic subunits phosphorylate cAMP enhancer binding proteins (CREBs), transcription factors that turn on gene expression for regulatory and effector proteins. The regulatory proteins break down the freed PKA regulatory subunits before they can rebind the catalytic subunits, keeping the catalytic subunits in a perpetually activated state even after the initial rise in intracellular cAMP has returned to baseline levels. The effector proteins drive later gene expression and protein synthesis that reconstructs the post-synaptic membrane by freeing "hidden" excitatory post-synaptic receptors and generating new synapses, thus keeping the synapses in a potentiated state for upwards to two weeks.[8]

In Abel's PKA R transgenics, in the regions of the brain where the PKA regulatory transgene is expressed, there is an overabundance of PKA regulatory subunits in the neurons to rebind the catalytic subunits freed initially by the sudden rise in cAMP (Abel *et al.* 1997). This blocks the gene expression and protein synthesis necessary for the transition from E-LTP to L-LTP (since freed catalytic PKA subunits don't reach the neuron's nucleus to phosphorylate the CREB molecules). Transgenic mice were viable and two generations were bred for behavioral testing.

If the molecular mechanisms described above for the transition from E-LTP to L-LTP are also the mechanisms for the consolidation of some memories from short-term to long-term form, then the PKA R transgenic mice provide an interesting experimental test. For memories known to be dependent on brain regions where the transgene is highly expressed, these animals should be intact on short-term tests for such memories (since PKA molecules play little to no role in the mechanisms of E-LTP), but impaired on long-term tests. Abel *et al.* (1997) chose a variety of hippocampal-dependent and amygdala-dependent memory tests for the PKA R transgenics and their wild-type littermate controls (into which the transgene had not been inserted). In one study they simultaneously trained the mice on a hippocampal-dependent contextual conditioning task and an amygdala-dependent Pavlovian fear conditioning task. Mice were exposed for two minutes to a novel environment, followed by a thirty-second exposure to a tone. (Exposure to the environment elicited stereotypic rodent exploratory behavior. Exposure to the tone elicited an orienting response.) Immediately upon the cessation of the tone, mice received an electric shock through a cage floor foot grid. The shock elicited an aversive reaction, followed by a stereotypic rodent fear response, freezing, where the mice crouch down, front paws tucked inward, and display only breathing move-

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[8] See Bickle (2003, Chaps. 2 and 3) for a detailed account of some of the genes and proteins involved in this molecular cascade, a description of some experiments that revealed them, and extensive references to the primary scientific literature.

ments. Experimental (PKA R transgenics) and control (wild-type littermates) mice were then divided into groups, with some being returned to the novel training environment either one hour or twenty-four hours later (short-term and long-term hippocampal-dependent contextual conditioning) and others being exposed to the tone in their home cages one hour or twenty-four hours later (short-term and long-term amygdala-dependent Pavlovian fear conditioning).

Based on the hypothesized molecular mechanisms for memory consolidation, the predicted results from this experiment are the following. PKA R transgenics should be intact compared to littermate controls on measures of short-term hippocampal contextual conditioning. E-LTP does not involve the cAMP-PKA-CREB pathway that the expressed transgene blocks. But they should be impaired on measures of long-term hippocampal-dependent contextual conditioning because the overabundance of PKA R subunits in forebrain regions will block the translocation of significant amounts of freed catalytic PKA subunits to neurons' nuclei, and subsequently block the gene expression and protein synthesis necessary for L-LTP: and by hypothesis, block consolidation of long-term memory. However, the PKA R transgenics should be intact on both short-term and long-term tests of amygdala-dependent Pavlovian fear conditioning because of the lesser expression of the PKA R subunit transgene in that brain region.

These were exactly the results that Abel *et al.* (1997) reported. Both generations of transgenics were completely intact – they spent statistically similar times freezing compared to wild-type littermate controls – immediately after the training shock and when replaced in the training cage one hour later. But they were significantly impaired when replaced in the training cage twenty-four hours later, spending less than half of the time freezing (over a two-minute interval) than their wild-type littermate controls. On all aspects of the amygdala-dependent Pavlovian fear conditioning task, however, the transgenics were statistically similar to controls in their freezing responses to the tone. This result constituted an important experimental control, since it counts against perceptual, attentional, motivational, or motor explanations for the long-term hippocampal-dependent contextual conditioning result. The behavioral deficit induced by the molecular-genetic intervention and subsequent gene overexpression was limited specifically to memory consolidation subserved by the regions affected.

Since metascience takes scientists' published words at face value, it is instructive to consider the conclusions that the scientists themselves drew from these results. Abel and his colleagues concluded that "our experiments define a role for PKA in L-LTP and long-term memory *and they provide a framework for a molecular understanding of the consolidation of long-term explicit memory in mice.* [...] the consolidation period is a critical period during which *genes are induced that encode proteins essential for stable long-term memory*" (Abel *et al.* 1997, pp. 623-624; my emphases). The "framework" to which they refer is exactly the set of "cognition-to-molecules links" referred to in the quote cited above from Kandel, Schwartz & Jessell (2001). These are nothing less than claimed mind-to-molecular pathway *reductions* (Bickle 2006a). Only here, the reductionist vision is being asserted in the Discussion section of a paper published in *Cell* – one of the highest rated journals that publish primary experimental reports.[9]

## 5. Why Arbitrariness is the Vice of Ad Hocness

I've here only described a single experimental example from Molecular and Cellular Cognition. But in other publications I've described many others (Bickle 2003, 2005a, 2005b, 2006a, 2006b, 2007, 2009a, 2009b, 2012). From these metascientific analyses I've derived a new model of Real-Reduction(ism)-in-Really-Reductionistic-Neuroscience. What is common to these experimental examples is a methodology for establishing a direct reductive link between a cognitive phenomenon and molecular mechanisms. This experimental techniques involve:

- *Intervening causally* into some event in the proposed cellular or molecular mechanisms; and then
- *Tracking specific behavioral effects* of the interventions under controlled, widely accepted experimental protocols for the cognitive phenomenon under investigation.[10]

In the Abel *et al.* (1997) study, for example, the causal intervention was into a specific step in the cAMP-PKA-CREB-gene expression-protein synthesis pathway, already known to be a mechanism for the conversion of E-LTP into L-LTP. These interventions used the techniques of molecular biology and bioengineered gene mutation to insert the PKA R subunit transgene into mice and limit its expression to forebrain regions. The behavioral tracking involved standard experimental measures of short-term and long-term hippocampal- and amygdala-dependent conditioning to test the effects of the interventions on independent forms of memory consolidation.

In contrast to the picture of reduction that informs the emphasis on intertheoretic reduction, by both non-structuralists and structuralists alike, real reduction in really reductionistic neuroscience can be diagrammed as in Figure 6. Notice that theories are not the primary relata of Real Reductionism; hypothesized mechanisms and the experimental protocols used to measure the cognitive phenomenon under investigation are. Of course, "theories" *inform* these primary relata. They tell us which cellular or molecular pathways to intervene into, which intervention techniques to use, and which behavioral measures to employ. But theories' components are not what real reductions in real neuroscience relate, at least not in the experimental and methodological practices at work in "ruthlessly reductionistic" Molecular and Cellular Cognition. Perhaps "in the end" some

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[9] In 2005, *Cell* had the 10th highest Journal Impact Factor score among all scientific journals (http://www.sciencegateway.org/impact/if2005c.htm). Journal Impact Factor is calculated from information contained in Journal Citation Report (JCR), a product of Thomson ISI (Institute for Scientific Information).

[10] In Silva & Bickle (2009) and Silva, Landreth & Bickle (forthcoming), we've embedded this account into a more complete account of the experimental conditions deemed sufficient in MCC for a fully justified causal-mechanistic molecular explanation of a specific cognitive function. The reductionistic component of MCC practice only constitutes a part of a fully sufficient experimental case, but it is the part that MCC research contributes distinctively.

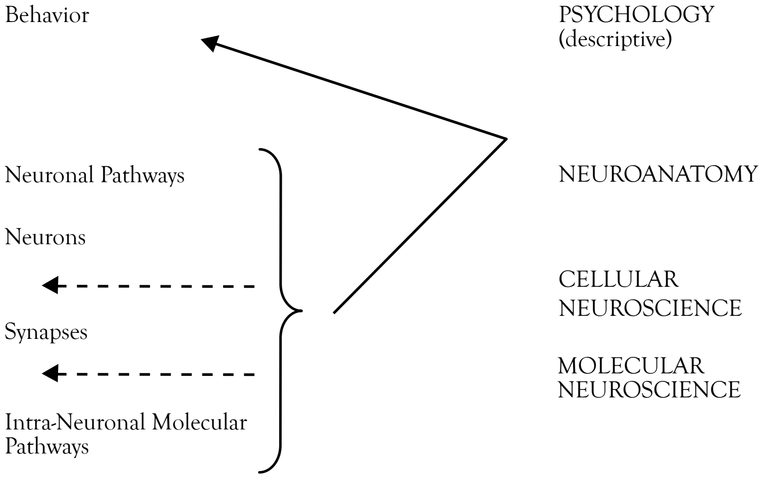

**Figure 6.** Schematic illustration of the "intervene molecularly/cellularly and track behaviorally" account of reduction from Molecular and Cellular Cognition. Dashed arrows represent causal interventions in experimental animals; the solid arrow represents the phenomena with which the effects of these interventions are measured. Psychology is a descriptive discipline, rather than one that offers causal mechanistic explanations.

kind of inter*theoretic* reduction relation will obtain between psychology and molecular neuroscience. But that's pure speculation, and is certainly not an account of what's happening in day-by-day reductionistic laboratory endeavors. Descriptive metascience of Molecular and Cellular Cognition does not find components of the structuralist reduction relation $\rho$. But it still finds Real Reductionism.

Or does it? Can the "intervene molecularly and track behaviorally" account of real neuroscientific reductionism distinguish between a molecule's activity being *causally relevant* for a given cognitive phenomenon – say, pCREB and memory consolidation, in the case study of the previous section – and that molecule's being *constitutively relevant* for it? Reduction "of mind to molecular pathways" would seem to require constitutive relevance. Yet the only such account on offer at present is Carl Craver's (2007), and given Craver's related account of causal relevance, his account of constitutive relevance has been criticized for inappropriately implying causal relevance, something his own account of mechanistic causation forbids (Leuridan 2012). (Constituents of a phenomenon can't be causes of the phenomenon's occurrence.) Does "intervene molecularly and track behaviorally" have anything to offer concerning how scientists distinguish constitutively relevant molecular pathways for a cognitive function – the ones the function thereby reduces to – from molecules and activities that are merely causally relevant for the function, perhaps as background causal conditions? And if not, does the resulting metascientific "intervene molecularly and track behaviorally" account really deserve to be called an account of *reduction*?[11]

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[11] I thank an anonymous referee for stressing the importance of this challenge. It's a huge concern, and well beyond full treatment at this point in this paper, but I hope my comments that follow are helpful for readers to see how I address this worry.

There is a short answer to this question. Metascience purports to bring with it no philosophical assumptions about "what reduction has to be (or do)." That scientific reductionism distinguishes between constitutive relevance and causal relevance is clearly a philosophical assumption about "what reductionism is/does." If metascience doesn't find such a distinction in the actual experimental practices that distinguish reductionistic from non- (or less-) reductionistic scientific fields, the metascientist's conclusion will be that this philosophical assumption about what reductionism must be/do has to go.

There's something unsatisfying about the short methodological answer, however, because clearly reductionistic science does distinguish between relevant and irrelevant molecules and activities. So there must be a longer metascientific answer to this challenge. There is, although this essay is obviously not the place to pursue it in any detail. I contend that these concerns get resolved in actual scientific practice in "ruthlessly reductive" sciences like MCC by the myriad uses of *controlled experiments* in the landmark studies. One who first confronts the MCC literature is overwhelmed by the reported number of control studies described in even the most straightforward experiments. These controls are drawn from molecular biology and molecular genetics, through cell physiology, and on to behavioral assays, typically with numerous control groups. All neuroscience is awash with serious control concerns, but the direct experimental search for the molecular mechanisms of cognitive functions carries control to what will seem to initiates to border on fetish. But it's not fetish –collectively, it's the attempt, directly and on the lab bench, to distinguish relevant active molecular pathways in neurons from irrelevant but equally active ones in behavioral assays that operationalize the cognitive function for experimental test. It's not quite the philosopher's requested distinction between constitutive relevance and causal relevance; but it provides the accepted scientific standards for determining that, e.g., the G-protein-cAMP-PKA-CREB, pathway active in neurons recruited into the memory trace is relevant for memory consolidation behavior, while the active pathways involving activity-related cytoskeleton associated protein, equally active in those same neurons, are not (Han *et al.* 2007). A metascientific analysis of how working scientists do this will start with a detailed study of standardly-employed control experiments in reductionistic sciences like MCC.[12]

## 6. Beyond Structuralist Reconstruction?

The outcome of a metascience of MCC looks rosy for the reductionist project. Its prospects for the structuralist program seem less rosy, however. In the final analysis, my view is that structuralism provides precise analyses of science's "long-term" products. When it comes to intertheoretic reduction, for example, we saw in the first half of this paper some advantages that structuralist-inspired analyses

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[12] So far I've only given talks on this topic. I hope to address it fully, and soon, in a subsequent essay.

have offered. I still contend that these advantages are genuine. At the "end" of extensive scientific development, theories and intertheoretic relations like reduction do seem to obtain. Structuralism provides proven helpful resources for characterizing these end-products of a lot of scientific development. But concerning sciences in earlier developmental stages, like both current psychology and neuroscience, metascientific investigation doesn't find much of anything resembling structuralist reconstructions. Instead, when we turn our metascientific lens on "ruthlessly reductive" Molecular and Cellular Cognition, we find the "intervene molecularly and track behaviorally" alternative account. It is not clear that a structuralist reconstruction of a landmark case of Real Reductionism is possible, or illuminating and helpful to scientists. What would a structuralist analysis of a detailed molecular or cellular intervention experiment look like? What about specific behavioral measures used as operationalized indicators of a cognitive function? Are these components of Real Reductionism susceptible to formal analysis? If so, will a structuralist reconstruction yield interesting new insights about Real Reductionism? We won't know, of course, until some analysis is attempted, so perhaps a structuralist will give it a try. Yet one can be excused for doubting whether such an approach will be fruitful, and for putting one's bets continuing to pursue metascience in attempting to learn the nature of Real Reductionism.

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