

Collapse of the New Wave

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Source: *The Journal of Philosophy*, Vol. 95, No. 2 (Feb., 1998), pp. 53-72

Published by: Journal of Philosophy, Inc.

Stable URL: <https://www.jstor.org/stable/2564571>

Accessed: 17-03-2019 01:38 UTC

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# THE JOURNAL OF PHILOSOPHY

VOLUME XCV, NO. 2, FEBRUARY 1998

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## COLLAPSE OF THE NEW WAVE\*

There is a movement in the philosophy of mind recently dubbed “the new wave,” one built upon “a novel general account of theory reduction in science.”<sup>1</sup> Advocates hail Paul Churchland and Clifford Hooker,<sup>2</sup> claiming their model enjoys a number of theoretical advantages over its competitors. While discounting these advertised virtues, I argue that the new-wave model has but one genuinely novel element, and it should be rejected. Moreover, once its ontological consequences are duly noted, the model collapses into the classical theory developed within the logical empiricist tradition, so that it still falls prey to the standard antireductionist argument. The new wave is thus powerless to erode antireductionist sentiment and destined to recede into the distant past, leaving the conceptual landscape largely unchanged.

### I. THE MODEL

New-wave reduction involves three theories: the basic reducing theory  $T_B$ , the original reduced theory  $T_R$ , and a corrected image  $T_R^*$ . With an eye to ideal cases, Paul Churchland describes it thus:

[A] successful reduction ideally has the outcome that, under the term mapping effected by the correspondence rules, the central principles of

\* My thanks to Charles Carr, Jerry Fodor, Thomas Grimes, Terence Horgan, Jaegwon Kim, John Post, and Kent Staley for their helpful comments on an earlier draft of this paper. I owe John Bickle a special debt of gratitude for many discussions and friendly disagreements about the new wave.

<sup>1</sup> John Bickle, “New Wave Psychophysical Reduction and the Methodological Caveats,” *Philosophy and Phenomenological Research*, LVI (March 1996): 57-78, here p. 57.

<sup>2</sup> The model and its virtues are outlined in Paul Churchland, *Scientific Realism and the Plasticity of Mind* (New York: Cambridge, 1979); and “Reduction, Qualia, and the Direct Introspection of Brain States,” this JOURNAL, LXXXII, 1 (January 1985): 8-28; and Hooker, “Towards a General Theory of Reduction, Part I: Historical and Scientific Setting; Part II: Identity in Reduction; Part III: Cross-Categorical Reduction,” *Dialogue*, xx (1981): 38-59, 201-36, 496-529.

$T_R$  (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  $T_B$ .<sup>3</sup>

Emphasizing a key aspect that will loom large in later discussion, he stipulates that the corrected  $T_R^*$  must be part of the basic theory, being “general sentences of  $T_B$ .” He also refers to a “mapping” with “correspondence rules” between this base-level  $T_R^*$  and the original reduced  $T_R$ . Given the nature of  $T_R^*$ , these rules no longer have a familiar classical function. As Churchland goes on to say:

First, 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  $T_B$ . Second, it is important to appreciate that cross-theoretic identity claims, even if they are justly made, are not part of the reduction proper, and they are not essential to the function it performs (*ibid.*, p. 83).

So reduction remains deduction. But unlike the classical account, crosstheoretic identity claims are “not part of the reduction proper,” since it is not the original  $T_R$  but rather  $T_R^*$  that is deduced from  $T_B$ , an image specified in the basic vocabulary. Hence, intertheoretic rules are inessential to both what is deduced and how. Instead, their primary function is to show how  $T_R^*$  can capture the explanatory role of  $T_R$ . As Paul Churchland puts it:

The correspondence rules play no part whatever in the *deduction*. They show up only later, and not necessarily as material-mode statements, but as mere ordered pairs:  $\langle Ax, Jx \rangle$ ,  $\langle Bx, Kx \rangle$ ,  $\langle Cx, Lx \rangle$ ,  $\langle Dx, Mx \rangle$ . Their function is to indicate which term substitutions in the image  $T_R^*$  will yield the principles of  $T_R$ . The older theory, accordingly, is never deduced; it is just the target of a relevantly adequate *mimicry*.<sup>4</sup>

Hooker expresses the emerging picture more formally in this way:

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  $T_B$ . Thus  $(T_B \cdot C_R \rightarrow T_R^*) \cdot (T_R^* \text{ AR } T_R)$  warrants  $(T_B \text{ R } T_R)$ .<sup>5</sup>

<sup>3</sup> *Scientific Realism and the Plasticity of Mind*, p. 81, with a slight change in the variables, henceforth. It should be noted that Churchland no longer speaks of theories in terms of public-language sentences, but prefers connectionist phase spaces. More on this in section IX.

<sup>4</sup> “Reduction, Qualia, and the Direct Introspection of Brain States,” p. 10.

<sup>5</sup> “Towards a General Theory of Reduction, Part I,” p. 49.

Of course, the analogue relation is not always strong, and the terms of  $T_R$  will not always map smoothly onto the terms of  $T_R^*$ . In Paul Churchland's words: "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."<sup>6</sup> So there is a continuum of strong to weak analogies between reduced and reducing theories, one that carries important metaphysical consequences. In the smooth case, though the *deduction* does not contain crosstheoretic identity claims, they are justified nevertheless by the success of the overall *reduction*. Churchland is explicit:

On this view, full-fledged *identity* statements are licensed by the comparative *smoothness* of the relevant reduction (i.e., the limiting assumptions or boundary conditions on  $T_B$  are not wildly counterfactual, all or most of  $T_R$ 's principles find close analogues in  $T_R^*$ , etc.). This smoothness permits the comfortable assimilation of the old ontology with the new and thus allows the old theory to retain all or most of its ontological integrity. *It is smooth intertheoretic reductions that motivate and sustain statements of cross-theoretic identity, not the other way around.*<sup>7</sup>

On the other hand, when the analogy is weak and the particular case is not smooth, the ontology of the original theory is replaced, not reduced. As Patricia Churchland<sup>8</sup> 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 (*ibid.*, p. 284).

Or, again, mindful of the metaphysical consequences, John Bickle<sup>9</sup> describes it thus:

[T]his alternative account of intertheoretic reduction recognizes a spectrum of possible outcomes, and insists that the appropriate ontological consequences depend upon where on this spectrum a given case falls. We can lay out this spectrum (informally) as follows: at the left-most endpoint lie the perfectly smooth reduction pairs, where  $T_R^*$  is the ex-

<sup>6</sup> *Scientific Realism and the Plasticity of Mind*, p. 84.

<sup>7</sup> "Reduction, Qualia, and the Direct Introspection of Brain States," p. 11. He then defines "property" reduction: "A property  $F$ , postulated by the older theory or conceptual framework  $T_R$ , is reduced to a property  $G$  in some new theory  $T_B$  just in case (1)  $T_B$  reduces  $T_R$ ; (2)  $F$  and  $G$  are correspondence-rule paired in the reduction; and (3) the reduction is sufficiently smooth to sustain the ontology of  $T_R$ , and thus to sustain the identity claim, ' $F$ -ness =  $G$ -ness'" (*ibid.*).

<sup>8</sup> *Neurophilosophy: Toward a Unified Science of the Mind-Brain* (Cambridge: MIT, 1986).

<sup>9</sup> "Revisionary Physicalism," *Biology and Philosophy*, vii (1992): 411-30.

actly equipotent isomorphic image of  $T_R$  .... At the right-most endpoint lie the extremely bumpy or replacement cases. And separating these two extremes is a continuous spectrum of cases, approximating more or less closely one of the two endpoints (*ibid.*, p. 417).

So, putting together the various points, new-wave reduction involves four main features:

- (i) New-wave construction: the basic reducing  $T_B$ , not the original reduced  $T_R$ , supplies the conceptual resources for constructing the corrected  $T_R^*$ .
- (ii) New-wave deduction: the corrected  $T_R^*$ , not the original reduced  $T_R$ , is deduced from the basic reducing  $T_B$ .
- (iii) New-wave relation: there is a required analogical relation, not bridge laws, between the reduced  $T_R$  and the corrected  $T_R^*$ .
- (iv) New-wave 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  $T_R$ .

Viewed as a process, the account can be summarized in two stages: first, the *intratheoretic deduction*, which focuses on conditions (i) and (ii); second, the *intertheoretic mapping*, which focuses on conditions (iii) and (iv). Once the second stage is complete, meaning once the analogue relation between  $T_R^*$  and  $T_R$  has been established, one can make a claim about the reduction of the original  $T_R$  by virtue of the deduction of its analogue  $T_R^*$ .

## II. AFFINITIES WITH LOCAL AND APPROXIMATE REDUCTION

Certainly, the new-wave features have no echo in the classical model. There were bridge laws, not mere analogies, between the reduced and reducing theories; those bridge laws played an essential role, not a corrected image, in the derivation of the original reduced theory; and even if a corrected image were implied, it need not have been constructed from the basic reducing theory.

Yet the new-wave model is not without precedent. For example, condition (ii) on new-wave deduction is implied by previous accounts of local or domain-specific reduction developed by Jaegwon Kim and David Lewis,<sup>10</sup> according to which a corrected  $T_R^*$  is de-

<sup>10</sup> Kim, "Phenomenal Properties, Psychophysical Laws, and the Identity Theory," *Monist*, LVI (1972): 177-92, esp. p. 190; and his fullest treatment in "The Myth of Nonreductive Physicalism" and "Multiple Realization and the Metaphysics of Reduction," collected in *Supervenience and Mind* (New York: Cambridge, 1993), pp. 265-84, 309-35. Lewis, "Review of *Art, Mind, and Religion*," this JOURNAL, LXVI (January 1969): 22-35, esp. pp. 24-25; and his "Mad Pain and Martian Pain," in Ned Block, ed., *Readings in Philosophy of Psychology*, Volume 1 (Cambridge: Harvard, 1980), pp. 216-22.

duced from the basic reducing  $T_B$ , where the former is a domain-specific version of a more general and otherwise reductively recalcitrant  $T_R$ . Even closer in spirit and ideology, both conditions (ii) and (iii) are explicit in Kenneth Schaffner's<sup>11</sup> earlier model of approximate reduction. Specifically, like condition (ii), Schaffner stipulates "that  $T_{R^*}$  be derivable from  $T_B$ ," a theory which "corrects" the original  $T_R$ ; and like condition (iii), Schaffner stipulates that the "relation between  $T_R$  and  $T_{R^*}$  should be one of strong analogy," the predictions of  $T_{R^*}$  being "close" to those of the original (*op. cit.*, p. 144). Also, the new-wave continuum of condition (iv) is easily generated from Schaffner's account by loosening his strong analogy into a spectrum of strong to weak relations of the same kind. Moreover, on this point there will be no difference at all if one reserves the term 'reduction' for cases at the retentive end of continuum where, by new-wave lights, the analogy is strong (involving "close analogues" and "equipotent images"). Consonant with this well-established usage, Hooker preserves the distinction between 'reduction' and 'replacement' by applying the former exclusively to "cases which fall sufficiently far towards the retention end of the retention/replacement spectrum."<sup>12</sup> So, too, does Patricia Churchland, who employs the same distinction from the opposite direction, saying that disanalogous cases which require massive correction are "better described as having been displaced outright" (*op. cit.*, p. 311).<sup>13</sup>

So, on the matter of conditions (ii) through (iv), and being mindful that the topic is reduction and not any wider scientific practice that includes theory replacement, the Schaffner and Paul Churchland-Hooker models are virtually indistinguishable. Condition (i) on new-wave construction is the genuinely novel element: the basic re-

<sup>11</sup> "Approaches to Reduction," *Philosophy of Science*, xxxiv (June 1967): 137-47. See also William Wimsatt, "Reductionism, Levels of Organization, and the Mind-Body Problem," in G. Globus, G. Maxwell, I. Savodnik, eds., *Consciousness and the Brain: A Scientific and Philosophical Inquiry* (New York: Plenum, 1976), pp. 205-67, esp. p. 217.

<sup>12</sup> "Towards a General Theory of Reduction, Part I," p. 45. The rationale, for Hooker, is that reduction must still achieve some measure of explanatory and ontological unification.

<sup>13</sup> See also Patricia Churchland's remarks about what the reducing theory must explain (p. 283), as well as the distinction between "bumpy reduction" and "outright displacement" (p. 284), all of which presuppose the well-established usage in question. Yet see Paul Churchland (*Scientific Realism and the Plasticity of Mind*, p. 84), and Bickle (pp. 64-65), who describe *nonborderline*, *nonretentive* cases as "bumpy reductions." By standard usage, however, calling outright replacement a "bumpy reduction" is slightly perverse—like calling one's divorced status a "bumpy marriage."

ducing  $T_B$  and not the original reduced  $T_R$  must supply the conceptual resources for the corrected image  $T_R^*$ . Only with this constraint can the older Schaffner conditions appear in a new light, turning what might have been a corrected heterogeneous deduction of  $T_R^*$  from  $T_B$  into a purely intratheoretic one, or, contrawise, turning what might have been an intratheoretic analogy between  $T_R^*$  and  $T_R$  into a purely heterogeneous one.<sup>14</sup>

## II. A MATTER OF DISPLACEMENT

Proponents of the new wave claim their model exhibits a number of theoretical virtues. The first concerns displacement of the original theory. Paul Churchland says of the two-stage process:

The *intra*-theoretic deduction (of  $T_R^*$  within  $T_B$ ), and the *inter*-theoretic mapping (of  $T_R$  into  $T_R^*$ ), constitute a fell-swoop demonstration that the older theory can be displaced wholesale by the new, without significant explanatory or predictive loss.<sup>15</sup>

More precisely, the new and corrected  $T_R^*$  has the resources to *mimic* the explanatory role of the original  $T_R$  in an ideally smooth case, and to *better* the explanatory role of  $T_R$  in a rough and disanalogous case, either way demonstrating the old's replaceability by the new. Surely, the point is intriguing, since, contrary to appearances, if Churchland is correct then the new-wave model does not yield a continuum from retention to replacement after all. No, it is *theory replacement across the board*.<sup>16</sup>

Yet there are problems. First, even if the original "theory" can always be displaced, this is not so for the theory's ontology; for in a

<sup>14</sup> I say 'might have been', since Schaffner does not *require* that  $T_R^*$  be constructed out of the resources of a higher-level  $T_R$ . Instead, his model is defined to include "homogeneous" cases (p. 144).

<sup>15</sup> "Reduction, Qualia, and the Direct Introspection of Brain States," p. 11. In *Scientific Realism and the Plasticity of Mind*, similar remarks were followed by the disclaimer: "Displacement, of course, need not actually take place, however much considerations of unity and simplicity might demand it. Familiarity, entrenchment, convenience, and continuity may together counsel a less puritan course" (p. 82). So it is a form of displacement *in principle*. Even so, Churchland implies that it *can* take place, and certainly "familiarity, entrenchment, convenience, and continuity" do not accord any explanatory privilege to the original reduced theory. On the contrary, in ideally smooth cases the corrected theory "will perform all the same predictive and explanatory functions" of the original (p. 82), and in less than ideal cases it will be "superior" (p. 83).

<sup>16</sup> Curiously, Bickle ("New Wave Psychophysical Reduction," pp. 64-67) chastises Jerry Fodor for claiming that reduction will eliminate (that is, *displace*) the special science theory, failing to note that Paul Churchland made this an explicit platform of the new-wave model. Nor is Bickle's plea for the preservation of special sciences based upon any essential explanatory function that would be lost in the displacement. Instead, like Churchland's concession to the impure demands of "familiarity,

smooth and strongly analogous case, the ontology of  $T_R$  is *retained*, since claims about identity are sustained. So, on this side of the spectrum, there is replacement only in a weak and attenuated sense, a mere exchange of labels with referents and roles largely preserved.

Second, even this weak sense of replacement appears suspect. Even if we take theories as sets of sentences, for example,  $T_R$  should be individuated in such a way as to contain all the sentences adequate to express the objects and properties in its domain. But this will include  $T_R^*$  in ideally smooth cases, since, *ex hypothesi*,  $T_R^*$  is an equipotent image that preserves the ontology of the original. Indeed, Hooker indicates that  $T_R^*$  is the original  $T_R$  in cases of “perfect retention,” which, either way, makes replacement impossible.<sup>17</sup> (It follows, moreover, that there is no uniform theory of reduction, since, at this endpoint of retention,  $T_R^*$  (=  $T_R$ ) is not specified within  $T_B$ —assuming  $T_R$  is not—contrary to what the new wave generally requires.)

Third, and ideal cases to one side, some<sup>18</sup> argue that replacement only makes sense in the intralevel case where theories compete for the same logical space and target the same explanandum phenomena. If so, a higher-level  $T_R$  could not be eliminated by a purely base-level  $T_R^*$ . The novel new-wave constraint would thus show itself not a help but a hindrance.

Fourth, it is no accident that Paul Churchland refers to a “wholesale” replacement of  $T_R$ , since the corrected  $T_R^*$  must be specified entirely within  $T_B$ , excluding any contribution from the original  $T_R$ .<sup>19</sup> But, on the contrary, theories are seldom if ever replaced whole and entire. To cite

entrenchment, convenience, and continuity” (see footnote 15), Bickle is content to say only that: “Reducing theories are typically more general in scope than reduced ones, and so are often much more unwieldy for use in actual scientific practice” (pp. 66-67). There remains, then, a substantive disagreement: the new wave believes that sciences like psychology *can* be displaced without loss of explanatory power, retained only for the sake of convenience, while antireductionists deny it.

<sup>17</sup> Specifically, he says: “where perfect retention fails  $T_R^* \neq T_R$ ” (“Towards a General Theory of Reduction, Part II,” p. 203). Note, too, though Hooker conjectures elsewhere that “the retention extreme of the retention/replacement continuum goes unoccupied” (Part I, p. 45), that is a contingent matter, depending upon how actual theories may fare. So the model itself does not guarantee displacement, neither in principle, nor at the extreme endpoint of retention where  $T_R = T_R^*$ .

<sup>18</sup> See Wimsatt, “Reductionism, Levels of Organization, and the Mind-Body Problem,” pp. 215, 222-23; also Robert McCauley, “Intertheoretic Relations and the Future of Folk Psychology,” *Philosophy of Science*, LIII (June 1986): 179-99; and his “Explanatory Pluralism and the Co-evolution of Theories in Science,” in McCauley, ed., *The Churchlands and Their Critics* (Cambridge: Blackwell, 1996), pp. 17-47.

<sup>19</sup> That one must take the new-wave constraint in this exclusionary sense is clear; otherwise, the deduction of  $T_R^*$  from  $T_B$  would not be intratheoretic in nature, that is, in derivations where elements of a higher-level  $T_R$  remain in  $T_R^*$ .



a well-known case, Copernican theory retained much of the Ptolemaic scheme, including all the heavenly bodies and the same celestial mechanics, only slightly fewer epicycles and eccentrics.<sup>20</sup> As the new wave insists for reduction, so, too, for replacement—it is a matter of degree.

Fifth, and finally, other models of reduction offer a similar benefit: for example, the views developed by Schaffner and Kim, since they also traffic in a corrected theory  $T_R^*$  that can better the explanatory role of the original  $T_R$ , thus establishing  $T_R$ 's displaceability.<sup>21</sup> In fact, they offer a more *convincing* displacement if that phenomenon most plausibly occurs when  $T_R$  and  $T_R^*$  are intralevel competitors, or when the replacement is partial rather than wholesale; for unlike the new wave, these other views allow  $T_R^*$  to be specified by, and thus retain, the conceptual resources of an original higher-level  $T_R$ .

#### IV. REDUCING THE FALSE BY THE TRUE

So consider a second virtue. Proponents of the new wave claim their model can account for the otherwise puzzling case where the original reduced theory is false. Compare the classical view. If  $T_R$  is deduced from  $T_B$ , and if  $T_R$  is false, then it seems to follow that  $T_B$  must be false. As Paul Churchland explains:

Difficulties with this view begin to emerge with the observation that most reduced theories turn out to be, strictly speaking and in a variety of respects, *false*. (Real gases don't really obey  $PV = \mu RT$ , as in classical thermodynamics; the planets don't really move in ellipses, as in Keplerian astronomy; the acceleration of falling bodies isn't really uniform, as in Galilean dynamics, etc.) If reduction is *deduction*, *modus tollens* would thus require that the premises of the reducing theories (statistical thermodynamics in the first case, Newtonian dynamics in the second and third) be somehow false as well, contrary to their assumed truth.<sup>22</sup>

<sup>20</sup> This is Copernicus' considered view, found in the later books of the *De Revolutionibus*. See Thomas Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (Cambridge: Harvard, 1957). Kent Staley has pointed out to me that the historical fact of partial replacement was emphasized long ago by William Whewell in his essay, "Of the Transformation of Hypotheses in the History of Science," *Transactions of the Cambridge Philosophical Society*, IX (1851): 139-47, reprinted in Robert E. Butts, ed., *William Whewell: Theory of Scientific Method* (Indianapolis: Hackett, 1989), pp. 251-62.

<sup>21</sup> Accordingly, I have argued that Kim's model is best viewed as a form of eliminativism via the original higher-level theory. See my "Species-Specific Properties and More Narrow Reductive Strategies," *Erkenntnis*, xxxviii (May 1993): 303-21, esp. pp. 306-10.

<sup>22</sup> "Reduction, Qualia, and the Direct Introspection of Brain States," p. 9. Notice that Churchland comes dangerously close to a fallacy of equivocation: *modus tollens* needs "truth," Newtonian dynamics "assumed truth" (more precisely, "assumed truth at the time proposed," since it is now known to make false predictions when velocities near the speed of light, and so on).

On the new-wave model, however, one deduces a *corrected* version  $T_R^*$  which can be sufficiently *unlike* the original and incorrect  $T_R$  so as to yield a difference in truth value. In Paul Churchland's words: "on the liberal account we are here embracing it is clear that a true theory may reduce a false one."<sup>23</sup>

But matters are far from clear. First, and again, there is nothing particularly "new wavish" about the present virtue, since it has nothing to do with the novel constraint on theory construction. Rather, it concerns the condition on *deduction*, that  $T_R^*$  and not the incorrect  $T_R$  be derived from  $T_B$ . Accordingly, Schaffner's account yields the same result, as does Kim's and any other view that deduces a corrected, approximate, fragmented, or domain-specific counterpart whose truth value can differ from the original.

Even the classical model may yield the same result, since the original  $T_R$  is not deduced from  $T_B$  alone, but from  $T_B$  in conjunction with the pertinent connecting principles and boundary conditions. Any falsehood implied by the derivation could then be located in these other elements, not in the basic  $T_B$  (hypothetical connections and conditions still permit valid deduction, their usefulness being judged by the closeness of the adjoining domains, the amount of falsehood in the reduced theory, and the like).<sup>24</sup> Paul Churchland mentions this type of response, but adds:

This defense will not deal with all cases of falsity, however, since in some cases the reduced theory is so radically false that some or all of its ontology must be rejected entirely, and the "correspondence rules" connecting that ontology to the newer ontology therefore display a problematic status.<sup>25</sup>

This is not convincing, however; for if the original theory is "radically false," then by the classical model and well-established usage, it is simply not a target for reduction. In the old parlance, theories of this kind are subject to "replacement," where this contrasts with reduction in its definitive role as the provider of ontological and explanatory

<sup>23</sup> *Scientific Realism and the Plasticity of Mind*, p. 84. See also his "Reduction, Qualia, and the Direct Introspection of Brain States," pp. 9-10. Parenthetically, Fodor has pointed out to me in correspondence that whether the model allows  $T_R$  to be false while  $T_B$  is true depends entirely on the (unstated) constraints that govern the intertheoretic mapping. For example, if biconditional bridge laws are in the offing (as they will be at the retentive end of the continuum, see section VIII), then the same trouble affects the new-wave account.

<sup>24</sup> This only forces one to reconstrue the nature of the deduction as a kind of "transformation" versus a "sound argument" with true premises. See Thomas Nickles, "Two Concepts of Inter-theoretic Reduction," this JOURNAL, LXX, 7 (April 12, 1973): 181-201.

<sup>25</sup> "Reduction, Qualia, and the Direct Introspection of Brain States," pp. 9-10.

unification. Put differently, since radically false theories do not even approximate the ontology of the supposedly true reducing theory, there will be no correspondence rules to affect the classical derivation and thus unify the two domains. Contra Paul Churchland's final remark, the rules do not then "display a problematic status." No, in radically false cases they simply do not exist! So the point remains: at least for moderately false cases, the classical derivation goes through, the false by the true, hand-in-hand with the necessary *façon de parler*.

Finally, even granting that the classical theory should accord  $T_R$  and  $T_B$  the same truth value, this may not be an untoward result. Suppose a scientific theory is counted true only when all constituent propositions are true, false when at least one proposition is false (a trivial result on the assumption that a theory is defined as the logical conjunction of all constituent propositions). If so, then the reduced and reducing theories will have the same truth value: both will be false. That is, barring the philosopher's ideal science or God's omniscient perspective, every basic reducing theory is likely to contain at least one false proposition. Indeed, well-confirmed induction over past history will testify to the same, that basic reducing theories are false, strictly speaking, and including the very examples Paul Churchland cites, like the now outdated Newtonian dynamics reducing Keplerian and Galilean theories. Hence the desire to reduce the false by the true will go unfulfilled. Error is the way of the world.<sup>26</sup>

#### V. THE EXPLANATORY BURDEN OF CONNECTING PRINCIPLES

So consider a third supposed virtue. Bickle claims that, given the new-wave model, there is no need to explain the status of bridge laws between reduced and reducing theories. Referring to the deduction of  $T_{R^*}$  rather than the original  $T_R$ , Bickle says:

This contrast has a key consequence: it eliminates what was a vexing problem for the orthodox empiricist account of reduction, namely that of specifying the logical and ontological status of the bridge laws, espe-

<sup>26</sup> Terry Horgan has suggested to me that, while my arguments are technically correct, the spirit of "reducing the false by the true" might be better served by saying that new-wave reduction can eliminate one *source* of error—falsity in the higher-level theory—even if there are other sources of error in the reducing theory. Yet Horgan notes that this advantage is not exclusive to new waveism, which is my central point. I also add deep reservations about the underlying epistemic principle; for the one domain is not inherently more error-ridden than the other. So, for example, it is not like the (already suspect) empiricist scruple whereby one chooses "fallible observation" over "fallible observation plus even more fallible commitment to unobservables." If there is no reason to favor low-level error, then impartiality would demand that one either eliminate all sources or retain both levels instrumentally without commitment to their truth.

cially in reductions that imply or reflect significant ontological change (*op. cit.*, p. 58).

Bickle repeats the claim elsewhere, saying: “One advantage of the H-C [Hooker-Churchland] account is that it avoids having to specify the logical status of cross-theoretic identity statements, a problem that becomes especially pressing for the bridge-law account when a reduction entails significant correction to the reduced theory.”<sup>27</sup>

Yet Bickle’s remark that the problem is especially acute for cases that reflect “significant ontological change” echoes Paul Churchland’s confusion about the status of correspondence rules when the original theory is radically false. On the classical view, significant ontological change calls for theory replacement, not reduction, meaning that there will be no system of bridge laws to affect the classical derivation, and hence no question about their logical or ontological status.<sup>28</sup> Moreover, when there is no significant change, then the status of the bridge laws is infamously clear—they are *identity* statements, the considered opinion came to be, an opinion originally offered to solve (among other things) the very problem at issue.<sup>29</sup> Surely, the new wave cannot complain, since their model is also committed to crosstheoretic identities as a consequence of relatively smooth cases at the retentive end of the reductive spectrum.

But the important point, for purposes of explaining bridge laws, is that the tables can be turned. A defender of the new wave inherits a weightier burden on grounds that she has *more* intertheoretic principles to explain, not just those implicated by ontologically retentive cases, but an intertheoretic mapping across the entire spectrum from smooth reduction to bumpy replacement. That is, the pertinent new-wave mapping also encompasses “correspondence rules” or “ordered pairs” in rough and disanalogous cases, their function,

<sup>27</sup> “Mental Anomaly and the New Mind-Brain Reduction,” *Philosophy of Science*, LIX (June 1992): 217-30, esp. p. 223. See also his “Multiple Realizability and Psychophysical Reduction,” *Behavior and Philosophy*, xx (1992): 47-58, esp. p. 54.

<sup>28</sup> What Bickle could argue is that the status of bridge laws is problematic when there are “minor” ontological changes; for in that case reduction, not replacement, is still in the offing, yet the classical resources of strict identity via bridge laws might be incompatible with these differences. Yet, in my view, such arguments ultimately conflate differences in meaning with differences at the level of reference and ontology. In any case, the new wave is saddled with the same strict identities in ontologically retentive cases. See section VIII.

<sup>29</sup> See Lawrence Sklar, “Types of Inter-Theoretic Reduction,” *British Journal for the Philosophy of Science*, xviii (August 1967): 109-24; Robert Causey, *Unity of Science* (Dordrecht: Reidel, 1977), chapters 4 and 5; and a nice summary in Jaegwon Kim, *Philosophy of Mind* (Boulder: Westview, 1996), pp. 212-16.

again, being “to indicate which term substitutions in the image  $T_R^*$  will yield [mimic or better?] the principles of  $T_R$ .”<sup>30</sup> Nor is this intertheoretic mapping entirely unproblematic, seeing that, in nonretentive cases,  $T_B$  (and thus its subset  $T_R^*$ ) are often *radically incommensurate* with respect to the original  $T_R$ . How, then, can radically incommensurate terms be mapped upon or substituted one for another? Is this an idealization, the classical *façon de parler*?<sup>31</sup> The alleged sins of the old are repeated tenfold by the new.

#### VI. CO-EVOLUTION AND NEW-WAVE CONSTRUCTION

I have examined three central new-wave virtues and found them too common or too objectionable; another acclaimed virtue will be discussed shortly. But I now want to present some positive arguments against new-wave theory. The first draws upon William Wimsatt’s observations concerning “co-evolutionary development” between reduced and reducing theories, a doctrine which members of the new wave wholeheartedly embrace. As Patricia Churchland describes it:

[T]heories at distinct theoretical levels often co-evolve [citing Wimsatt], as each informs and corrects the other, and if a theory at one stage of its history cannot reduce a likely candidate at a higher level, it may grow and mature so that eventually it does succeed in the reductive goal. In the meantime the discoveries and problems of each theory may suggest modifications, developments, and experiments for the other, and thus the two evolve towards a reductive consummation (*op. cit.*, p. 264; also pp. 362-76).<sup>32</sup>

<sup>30</sup> “Reduction, Qualia, and the Direct Introspection of Brain States,” p. 10. There is no question that Churchland means to include rough and disanalogous cases; for he cites the “radically false” cases that make any deduction of the original problematic (pp. 10-11), and says “what cases like these invite us to give up is the idea that what gets *deduced* in a *reduction* is the theory to be reduced” (p. 10). Hence he presents the intratheoretic deduction, and then the intertheoretic mapping with the pertinent remarks quoted in the text.

<sup>31</sup> Paul Churchland also misses the problem. He says “it is possible on this account for a theory to reduce even an *incommensurable* competitor,” and discusses the reduction of classical mechanics (CM) by the special theory of relativity (STR) (*Scientific Realism and the Plasticity of Mind*, p. 85). The reduction is possible, he says, in spite of the difference in how mass is conceived, and so on, because “it is easy to deduce within STR...a highly convincing image of CM” (*ibid.*), which is to say, because of the first stage where one avoids deducing the incommensurable  $T_R$ . But this fails to address the incommensurable “mapping” or “substitution” in the second stage between that same allegedly incommensurable  $T_R$  and the corrected base-level counterpart  $T_R^*$ .

<sup>32</sup> The original source is Wimsatt, pp. 230-37. See also Hooker, “Towards a General Theory of Reduction, Part I,” p. 48. Contra Patricia Churchland, however, there is no reason to assume the theories will always co-evolve toward “reductive consummation.” They may drift further apart or settle into a stable equilibrium. See McCauley’s “Explanatory Pluralism and the Co-evolution of Theories in Science.”

As Hooker and Bickle also emphasize, this co-evolutionary feedback is not unidirectional, or exclusively from low to high levels.<sup>33</sup> There is a top-down flow of information and influence. For example, ‘cistron’ is a corrected image of the Mendelian gene (a term in  $T_R^*$ , and hence a term supposedly formulated within the idiom of  $T_B$ ). Yet it was not created from molecular genetics ( $T_B$ ) *ex nihilo*, but from the pressure of the original Mendelian theory ( $T_R$ ) to find a structure with the function of a gene. So *co*-evolved terms within  $T_B$  or rather its subset  $T_R^*$  are by their very nature dually constrained by the rationales and conceptual resources grounded at both levels. In a word, they are theoretical hybrids, mirror images of the intertheoretic correspondence rules within classical reduction, differing from them only by the cover of a single term. The moral is that, because of the natural ebb and flow between levels of scientific language and scheme, the basic reducing theory becomes permeated with high-level concepts and concerns.

Now the problem is straightforward. The new-wave constraint on theory construction stipulates that the basic  $T_B$  and *not* the original  $T_R$  must supply the conceptual resources for constructing the corrected image  $T_R^*$ . Yet this seems flatly contradicted by the fact that, once co-evolution has run its natural course,  $T_R^*$  has become a mutual product of  $T_B$  and  $T_R$ . How, then, is  $T_R^*$  specified “within the idiom of  $T_B$ ” in any meaningful sense that *excludes*  $T_R$ ?

The answer is not readily apparent. But it is clear what form the answer must take, namely, the new-wave advocate must discount the historical contribution made by the upper-level theory. But such a move will not register any deep semantic fact about the terms in question if historical properties carry some weight, as they do (directly or indirectly) by considerations about the social surround, remote baptismal groundings, linguistic divisions of labor, externalism about conventions, and naturally selected biofunctions.<sup>34</sup>

<sup>33</sup> Hooker, “Towards a General Theory of Reduction, Part III,” pp. 513-14; Bickle, “New Wave Psychophysical Reduction,” p. 76.

<sup>34</sup> Ludwig Wittgenstein, *Philosophical Investigations*, G.E.M. Anscombe, trans. (New York: Macmillan, 1953); Saul Kripke, *Naming and Necessity* (Cambridge: Harvard, 1980); Hilary Putnam, “The Meaning of ‘Meaning,’” in *Mind, Language and Reality: Philosophical Papers*, Volume 2 (New York: Cambridge, 1975), pp. 215-71; Tyler Burge, “Individualism and the Mental,” in P.A. French, T. Uehling, Jr., and H.K. Wettstein, eds. *Midwest Studies in Philosophy*, Volume 4 (Minneapolis: Minnesota UP, 1979), pp. 73-121; and Ruth Garrett Millikan, *Language, Thought, and Other Biological Categories* (Cambridge: MIT, 1984). In fact, aside from current conceptual role, there is probably just one leading semantic theory that ignores historical factors—a covariational theory that depends upon counterfactual relations and not actual causes. See Fodor, *The Elm and The Expert: Mentalese and Its Semantics* (Cambridge: MIT, 1994), pp. 115-19. But even this is no ally

Moreover, the new wave cannot appeal to conceptual or inferential role as a way of partitioning off the contribution of the original upper-level theory,<sup>35</sup> since the model guarantees current inferential links between  $T_R^*$  and  $T_R$ , those secured by the intertheoretic mapping in the second stage of the new-wave reductive process, both inferences that yield identity and substitutions that indicate mimicry. A special class of these inferential links will block the otherwise quite natural suggestion that  $T_R^*$  counts within the idiom of  $T_B$ , exclusively, on grounds that it can be *deduced* from  $T_B$ . Likewise,  $T_R^*$  can be deduced from  $T_R$  in cases of perfect retention, since they are for all intents and purposes the very same theory. Similarly,  $T_R^*$  can be deduced from  $T_R$  in less than perfect but still strongly analogous cases, only now in conjunction with more generous boundary conditions, hypothetical assumptions, and convenient fictions (remember how the classical theory can deduce the false by the true).

#### VII. HISTORY, METHODOLOGY, AND THE INITIAL COLLAPSE

Suppose there is a way to reconcile new-wave theory construction with co-evolutionary development, which is to say, some plausible account of term-to-theory individuation which grants  $T_B$  sole propriety over  $T_R^*$ . Even so, the distinction between constructing  $T_R^*$  out of  $T_B$  versus  $T_R$  can only appear superficial from the perspective of the actual historical development of the sciences, seeing that crosstheoretic evolution guarantees a healthy interplay between the two levels. More important, new-wave theory construction can only appear unduly restrictive from the vantage point of reductionist ideology and methodology, since it rules out strategies that may require aid from above, from the level of concepts in  $T_R$ .

Consider local reduction, whereby a corrected  $T_R^*$  can be generated out of an original and more general  $T_R$  either by "relativizing"

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to the present partitioning strategy mentioned in the text, since, for all cases at the *retentive* end of the new-wave continuum, the properties counterfactually related to  $T_R$  and  $T_R^*$  will be the same, and thus the terms will have identical semantic content.

<sup>35</sup> See Gilbert Harman, "Meaning and Semantics," in Milton Munitz and Peter Unger, eds., *Semantics and Philosophy* (New York: University Press, 1974), pp. 1-16; and Hartry H. Field, "Logic, Meaning, and Conceptual Role," this JOURNAL, LXXIV, 7 (July 1977): 379-409. Note that Paul Churchland has been committed to something like this account when speaking with the vulgar, for example, in *Scientific Realism and the Plasticity of Mind*, pp. 52-80. For his later state-space semantics, see "Some Reductive Strategies for Cognitive Neurobiology," in *A Neurocomputational Perspective: The Nature of Mind and the Structure of Science* (Cambridge: MIT, 1989), pp. 77-110. For detractors, see Fodor and Ernest Lepore, *Holism: A Shopper's Guide* (Cambridge: Blackwell, 1992), and their "Paul Churchland and State Space Semantics," in *The Churchlands and Their Critics*, pp. 145-62.

the original types to a restricted domain or by viewing those general types as “fragmenting” into narrower ones.<sup>36</sup> Either way, it is an operation upon *original* higher-level types. The new concepts thus owe their existence to the old, if not entirely, at least in conjunction with familiar methodological pressures from the basic reducing theory (human pain is a modification of pain, constructed directly out of general psychology, but with an eye to finding some stable nonvariable structure within neuroscience).

The upshot is this: on the worst-case scenario, new-wave construction is flatly contradicted by co-evolutionary facts; on the best-case scenario, it is historically shallow and methodologically restrictive. Sound reason, therefore, enjoins that it must be rejected. Once rejected, the new-wave picture takes on a familiar look; for the corrected  $T_R^*$  may now be specified within the idiom of  $T_R$ , making the deduction of  $T_R^*$  intertheoretic in nature, and bringing in train all the hybrid correspondence rules that a corrected classical derivation must provide. More pointedly, since the constraint on theory construction is the only genuinely novel element in the new-wave model, and the three remaining conditions are either directly implied or easily generated from Schaffner’s account, then the entire project will collapse into its predecessor’s. 1980s new-wave reduction and 1960s revolutionary approximation are one.

#### VIII. BRIDGE LAWS AND THE FINAL COLLAPSE

The most important of all virtues is surely the capacity of the model to resist antireductionist criticism. To that end, all defenders of the new wave unite in holding that their view enables them to counter arguments that originally led to the demise of the classical theory. Without this, all would be for naught. Qua reductionism, the entire project would fail.

So consider those arguments which reject psychological and function-to-structure bridge laws, chiefly Hilary Putnam and Jerry Fodor’s multiple realizability argument, and also Donald Davidson’s<sup>37</sup> norma-

<sup>36</sup> See Causey, *Unity of Science*, pp. 147-49; Kim, “The Myth of Nonreductive Physicalism,” p. 273.

<sup>37</sup> Putnam, “The Nature of Mental States,” in *Mind, Language and Reality*, pp. 429-40; Fodor, “Special Sciences” in *Representations* (Cambridge: MIT, 1981), pp. 127-45; and my summary in “Multiple Realization,” *The Encyclopedia of Philosophy*, Supplemental Volume (New York: Macmillan, 1996), pp. 365-66. For the normativity argument, see Davidson’s “Replies,” in B. Vermazen and M. Hintikka, eds., *Essays on Davidson: Actions and Events* (New York: Oxford, 1985), esp. pp. 245, 249; and Kim, “Psychophysical Laws,” in E. Lepore and B. McLaughlin, eds., *Actions and Events: Perspectives on the Philosophy of Donald Davidson* (Cambridge: Blackwell, 1985), pp. 369-86.



tivity argument, since they brought about widespread antireductionist sentiment with regard to psychology and the functionally specified domains of biology.

Now observe the following new-wave strategy. Responding to Davidsonian arguments, Bickle claims: “the impossibility of psychophysical laws is irrelevant to the new thesis of mind-brain reductionism and the novel account of intertheoretic reduction underwriting it.”<sup>38</sup> Why? Bickle asks and then answers the question:

Having now presented the account of intertheoretic reduction adopted by proponents of the new mind-brain reductionism, what can we conclude about the Davidsonian objection based upon the impossibility of psychophysical laws? It fails, and quite conclusively. For the lack of cross-theoretic laws is of no consequence to whether an H-C reduction is possible, *since an H-C reduction nowhere requires bridge laws* (*ibid.*, p. 224; italics added).<sup>39</sup>

Generalizing, then, whether it be Davidson’s normativity considerations or Putnam and Fodor’s more influential multiple realizability argument, all fail because they challenge reductionism on the point of bridge laws between the reduced and reducing theories. This is a palpable hit when directed against the classical account, but one that falls wide of the mark when directed against the new wave, for “H-C reduction nowhere requires bridge laws.”

But this is mistaken; for new-wave reduction includes an intertheoretic mapping in its second stage, which, when carried out for strongly analogous cases, justifies cross-theoretic identities. As Paul Churchland says: “a successful reduction of the ideal sort described provides an excellent reason for asserting the relevant cross-theoretic identities, the best reason one can have.”<sup>40</sup> Yet property identity guarantees nomic coextension. So bridge laws exist within the new-wave account, being directly implied by retentive cases.

Nor will it do, as a response, to insist on a distinction between the “reduction proper” versus its “consequences,” confining bridge laws to the latter.<sup>41</sup> Consequences are consequences, and to deny them is like

<sup>38</sup> Bickle, “Mental Anomaly and the New Mind-Brain Reduction,” p. 218.

<sup>39</sup> See also his “New Wave Psychophysical Reduction and the Methodological Caveats,” pp. 58-59. Accordingly, this is a basic theme in his *Psychoneural Reduction: The New Wave* (Cambridge: MIT, forthcoming).

<sup>40</sup> *Scientific Realism and the Plasticity of Mind*, p. 83; also “Reduction, Qualia, and the Direct Introspection of Brain States,” p. 11; Hooker, “Towards a General Theory of Reduction, Part I,” pp. 45-46, and Part II.

<sup>41</sup> Bickle has suggested something like this in correspondence, telling me: “I only acknowledge [new-wave conditions] (i) and (ii) as part of the intertheoretic reduction relation proper. (iii) and (iv) are attempts to relate intertheoretic results to ontological conclusions.” Granted, Paul Churchland also speaks of a “reduction

a smuggler caught in the act whose only defense is: "I meant there was no contraband *on my person!*" Declared or no, up front or trailing behind in tow, the goods are there; and so the problem is clear:

- (i) If a case falls at the retentive end of the new-wave continuum, then crosstheoretic property identities exist between reduced and reducing theories.
- (ii) If crosstheoretic property identities exist between reduced and reducing theories, then biconditional bridge laws exist between reduced and reducing theories.
- (iii) Therefore, if a case falls at the retentive end of the new-wave continuum, then biconditional bridge laws exist between reduced and reducing theories.

The conclusion is inescapable: the retentive end of the new-wave spectrum will collapse into the classical account of reduction, replete with its intertheoretic bridge laws, and subject to all the slings and arrows of antireductionist argument against them. If some of these arguments are sound, as many believe, then the new wave is forced into a familiar position;<sup>42</sup> that is, to return full circle:

- (iv) It is *not* the case that biconditional bridge laws exist between intentional psychology or functionally construed theories in biology and more basic physical theories.
- (v) Therefore, it is *not* the case that intentional psychology or functionally construed theories in biology will fall at the retentive end of the new-wave continuum.

There is no type reduction. Rather, a defender of the new wave must locate all such theories at the replacement end of the new-wave

proper." Still, he did include the intertheoretic mapping and its ontological conclusions in the reduction (*Scientific Realism and the Plasticity of Mind*, p. 81; "Reduction, Qualia, and the Direct Introspection of Brain States," p. 11; and so on). Indeed, he counts "reducibility as a matter of degree," which can only mean the intertheoretic relations of conditions (iii) and (iv), surely not the intratheoretic deduction of (i) and (ii). Regardless, if we have a reduction of the original  $T_R$  at all, it must be tied to the relation between  $T_R$  and its image  $T_R^*$  which conditions (iii) and (iv) address; otherwise, the deduction of  $T_R^*$  from  $T_B$  would be completely irrelevant to the status of  $T_R$ .

<sup>42</sup> For defense of the Putnam-Fodor genre of argument, see my "On Physical Multiple Realization," *Pacific Philosophical Quarterly*, LXX (September 1989): 212-24. See also Terence Horgan, "Nonreductive Materialism and the Explanatory Autonomy of Psychology," in S. Wagner and R. Warner, eds., *Naturalism: A Critical Appraisal* (Notre Dame: University Press, 1993), pp. 295-320; and "Multiple Reference, Multiple Realization, and the Reduction of Mind," in F. Siebert and B. Preyer, eds., *Reality and Humean Supervenience: Essays on the Philosophy of David Lewis* (forthcoming). For the biological case, see Harold Kincaid, "Molecular Biology and the Unity of Science," *Philosophy of Science*, LVII (December 1990): 575-93.

continuum.<sup>43</sup> Surprisingly, then, matters remain much as they were before the new wave arrived upon the shore. In the absence of some other account, one must either accept antireductionism or embrace type eliminativism.

#### IX. CONCLUDING POSTSCRIPT

The astute reader will have gathered that the new-wave model has collapsed on several fronts. By rejecting its constraint on theory construction, the model collapses into approximate reduction. By observing bridge laws implied by its intertheoretic mapping conjoined with their ontological consequences, the retentive end of the model collapses into classical reduction. Finally, by accepting standard arguments against those bridge laws, the retentive end of the model collapses, period.

Let me add, in conclusion, a few remarks about certain newer, new-wave proposals that might be deemed relevant. In particular, Paul Churchland<sup>44</sup> no longer speaks of theories in terms of sentences or sets thereof, but rather as vectors through connectionist phase space. Bickle<sup>45</sup> has recently moved to a nonsentential account of theories understood in terms of model-theoretic structures. Therefore, one might claim that these *nonsentential* accounts will enable the new wave to avoid intertheoretic correspondence rules and thus burn the bridge with classical reduction.<sup>46</sup> But not so; traditional correspondence rules are not avoided, only relocated.

<sup>43</sup> Of course, this is not to deny other cases of smooth reduction where type identity is preserved—for example, physical optics, Kepler's laws of planetary motion, or a domain-restricted reduction thermodynamics. Also, my remarks concern the retention versus replacement of *types*. I have not addressed Hooker's function-to-structure *token* reductions ("Towards a General Theory of Reduction, Part III," pp. 504-05; also Bickle, "Multiple Realizability and Psychophysical Reduction," pp. 55-56; and his *Psychoneural Reduction*). I feel there is no pressing need to address this view here, since the historical debate over reduction has always concerned types and not tokens, the *antireductionists* being the token physicalists.

<sup>44</sup> The deep reason is that Paul Churchland has moved from a deductive nomological account of explanation to a psychological theory of prototype understanding grounded in the neurocomputational framework. See his "On the Nature of Theories: A Neurocomputational Perspective," and "On the Nature of Explanation: A PDP Approach," in his *A Neurocomputational Perspective*, pp. 153-56, 157-230. Similarly, Hooker has moved to a "regulatory systems" view of science which complements the connectionist paradigm. See C.A. Hooker, H.B. Penfold, and R.J. Evans, "Control, Connectionism, and Cognition: Toward a New Regulatory Paradigm," *British Journal for the Philosophy of Science*, XLIII (1992): 517-36; and Hooker, *Reason, Regulation, and Realism: Toward a Regulatory Systems Theory of Reason and Evolutionary Epistemology* (Albany: SUNY, 1995).

<sup>45</sup> See his *Psychoneural Reduction*; but compare Paul Churchland's less than enthusiastic view of the model-theoretic approach in "On the Nature of Theories: A Neurocomputational Perspective," pp. 157-58.

<sup>46</sup> So Bickle tells me that "no bridge laws or correspondence rules exist anywhere in my [forthcoming *Psychoneural Reduction*, not the earlier] new wave account of in-

No one wishes to deny the existence of public-language sentences, or their use by scientists when announcing, demonstrating, and publishing their theories (whatever the ultimate nature of theories might be). Accordingly, William Bechtel<sup>47</sup> proposes what amounts to a two-factor theory, supplementing internal neurocomputation with publicly manifested deductions and diagrams and “various external representational systems” (*ibid.*, p. 126). Paul Churchland,<sup>48</sup> too, accommodates these plain facts of institutional science, though he now accords them a secondary role within a broader scheme of theoretical activity that encompasses animals, protohumans, children, and nonscientific lay persons.

In fact, the public domain of sententialist epistemology seems absolutely necessary for the preservation of the new-wave model. Specifically, and in light of Paul Churchland’s proposal, the condition that  $T_R^*$  be deduced from  $T_B$  cannot be internalized within neurocomputational-level systems, since prototype activation in a connectionist net does not involve rule-based deduction (ironically, traditional syntactic-driven architectures fare better on this score).

Worse still, there seems to be no plausible neurocomputational corollary for the one novel new-wave constraint that  $T_R^*$  be constructed out of the idiom of  $T_B$  rather than the original  $T_R$ . Would it mean that, as a matter of brute fact, all human brains are so constituted that  $T_R^*$ ’s activation vector is always a subsection of  $T_B$ ’s, and that  $T_R$ ’s activation vector never overlaps either on  $T_B$  or its subsection  $T_R^*$ ? That should depend entirely upon how the theories become calibrated within each individual, which is to say, upon the vicissitudes of the several input histories cum variable connections and weights.

So, belabored but important, the public language of science remains. Yet, also, advocates of the new wave have not retreated from the position that their model delivers important ontological conse-

tertheoretic reduction. How could they? Bridge laws and correspondence rules are sentential items, and my account of theory structure and intertheoretic relations nowhere employs sentences. Theories are sets of models with a certain set-theoretic structure.” Bickle is right. If narrowly focused on the models and their set-theoretic conditions, then no sentences can be found. But, as I argue here, taking a wider view of scientists and new wavers discoursing about the nonsentential items, coupled with new-wave ontological commitments for those items in retentive cases, reveals an old and familiar, that is, classical story.

<sup>47</sup> “What Should a Connectionist Philosophy of Science Look Like?” in *The Churchlands and Their Critics*, pp. 121-44.

<sup>48</sup> “Bechtel on the Proper Form of a Connectionist Philosophy of Science,” in McCauley, ed., *The Churchlands and Their Critics*, pp. 265-70, esp. pp. 266-67.

quences, namely, property identities in strongly analogous cases. Hence the problem surfaces again: property identities conjoined with the public language expressions of the hypothesized nonsentential vector spaces/sets of models  $T_R$  and  $T_B$  (or its subset  $T_R^*$ ) will yield nomic coextension for the predicates used in those public language expressions. The result is a set of intertheoretic bridge laws, and such is the classical model, in full sententialist dress. From this newest perspective, old bridging principles emerge at the metalevel, within public-language descriptions which all new wavers employ and to which all new wavers are unavoidably committed (whereof one must speak, one cannot remain silent).

To end with a fitting metaphor, the “collapse of the wave function” in quantum mechanics refers to the fact that a particular wave can be treated in a distinctly un-wave-like fashion, in terms of classical physics as a particle with specific location and velocity. Similarly, here the “collapse of the wave function” refers to the fact that a particular new wave can be treated in a distinctly un-wave-like fashion, in terms of classical reduction with bridge laws between reduced and reducing theories.

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