

**DISCUSSION:**  
**HYPOTHETICAL IDENTITIES AND ONTOLOGICAL  
ECONOMIZING: COMMENTS ON CAUSEY'S PROGRAM FOR  
THE UNITY OF SCIENCE\***

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Simplicity in most theoretical contexts is regarded as a virtue. Consequently, many scientists and philosophers are doubly attracted to proposals for the unification of science; doubly attracted because the success of such proposals will not only simplify particular theories, but the whole of science as well.

In a number of places Robert Causey has discussed what he sees as the logical and ontological requirements for successful microreductions. He holds that reduction functions must be synthetic identity statements in order to satisfy both the explanatory and ontological expectations that we have foisted upon microreductions. Though Causey never clearly states how much ontological economizing he thinks can ultimately be justified, he holds that "the program for the unification of science . . . consists of the attempt to microreduce successively the theories of all levels, except the lowest, down to the theory of the lowest level" and that this program is "an important guiding principle for present and future scientific research." (Causey 1972b, p. 177)

Causey's model offers an account of the constraints on intertheoretic relations necessary for such microreductive maneuvers which is both formidable and comprehensive. Nonetheless, in the last fifteen years a number of philosophers have pointed out that few, if any, of the classic examples of reduction, in fact, conform to the demands of those especially rigorous models proposed to date. Even such a standard example as the reduction of the theory of chemical bonding to atomic-molecular theory is plagued with difficulties. (Bantz 1976) William Wimsatt has suggested (1976) that, as a matter of fact, what we usually have are analyses of a

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few crucial lower level mechanisms (explicated in  $T_1$ , i.e., the lower level, reducing theory) and a lot of in principle promises. Such admonitions notwithstanding, Causey seeks "rules of thumb" for scientists that will delineate reasonable limits on the modification of theories in proposed microreductions. By more accurately describing the structure of microreduction, Causey hopes to offer eminently practical advice. Ironically, he proposes the most restrictive model to date.

Causey insists that "most actual reductions which have been performed are uniform microreductions or nearly uniform microreductions. . . ." (1977, p. 54) A uniform microreduction conforms to Causey's general derivational model, which employs synthetic identity statements as the means for connecting the ontologies of the pertinent theories, with the added feature that those ontologies also satisfy certain assumptions about homogeneity. The first and most important is Causey's General Homogeneity Principle which holds that any two entities in the domain of a theory are of the same kind if and only if their classifying attributes are the same. Let us not concern ourselves with the problems surrounding the notion of classifying attributes, but instead attempt to ascertain what Causey is driving for here. He demands that theories in proposed reductions conform to the General Homogeneity Principle in order to insure that each theory is sufficiently structured so that its domain can be easily partitioned into natural equivalence classes. Causey has this keen interest in natural kinds because he also insists that uniform microreductions have nothing but straightforward one to one, whole-part mappings of the entities of  $T_2$  (the upper level theory being reduced) on to those of  $T_1$ . (Causey 1972b, pp. 191-93)

On the face of it, these assumptions about homogeneity have many attractive features. First, Causey seems to avoid the sort of difficulties that are presented by the complexities that so often arise in ontological mappings between actual theories.<sup>1</sup> After all, a uniform microreduction is *by definition* a reduction that must have synthetic identities as its reduction functions, and these identities directly connect the natural kinds of its respective theories in part-whole relations. Also, these homogeneity requirements only reinforce Causey's claims about the need for synthetic identities as the connecting principles in successful microreductions by revealing just how extensive the resulting simplifications can be.

Every account of reduction has required that the reducing theory have some sort of explanatory relationship with the reduced theory. This explanatory relationship is what reduction is all about. Part of this explan-

<sup>1</sup>For a discussion of these issues in genetics and evolutionary biology, see Hull (1972), (1974), (1976), Ruse (1976), Schaffner (1974), (1976), Wade (1978), and Wimsatt (1980). On these problems generally, see Wimsatt (1974).

atory obligation, according to Causey, includes the need to account for why the natural kinds of the reduced theory are what they are. This is particularly urgent if microreductions are to justify ontological reduction.

*Dom*<sub>2</sub> [the domain of *T*<sub>2</sub>] is partitioned . . . into natural equivalence classes, which are homogeneous. A successful reduction is required, among other things, to provide an understanding of the homogeneity of these classes. The way this understanding is obtained is *by identifying* these classes with homogeneous classes in *Dom*<sub>1</sub> [the domain of *T*<sub>1</sub>] . . . homogeneous classes of *Dom*<sub>2</sub> must be identified with homogeneous classes of *Dom*<sub>1</sub>. (Causey 1972b, p. 192)<sup>2</sup>

Apparently, the sort of explanation to be given for why *T*<sub>2</sub> operates with the entities it does is that these entities are identical with the (configurations of) entities discussed in *T*<sub>1</sub>. Thus the *complete* explanation of *T*<sub>2</sub> (it should be emphasized that Causey is not satisfied merely with its derivation from *T*<sub>1</sub>) depends in an even stronger sense on the certification of the relevant synthetic identities. Their importance in the present discussion is not in virtue of their logical character as connecting principles but rather in virtue of the *empirical claims* they make. The homogeneity conditions minimize the practical problems of establishing identities in reduction contexts, since these identities are presupposed in the model.

As noted above, Causey's confidence as to both the possibility and relative frequency of uniform microreductions stands in some contrast to the findings of other researchers. In holding out for uniform microreductions, it seems that Causey has either chosen to ignore some recent analyses of alleged microreductions (e.g., Wimsatt 1976) and opted for an unreasonably restrictive model or that he has declared the problem of distinguishing and establishing identities solved (without disclosing the practical strategies involved in the solution). Since Causey would deny his need to address the first horn, he must surely contend with the second.

Jerry Fodor has made a general point which indicates some of the limitations on the range of intertheoretic relations to which Causey's model can apply. Fodor points out, quite simply, that "not all statements of identities are identities of composition." (Fodor 1968, p. 112) Causey demands part-whole identities between the natural kinds (and/or specified configurations of natural kinds) of *T*<sub>1</sub> and *T*<sub>2</sub> in order to achieve uniform microreductions, but Fodor offers three related reasons for thinking that it is unlikely that these identities will hold in the reduction of what Fodor calls the "special sciences" to the physical sciences:

<sup>2</sup>Emphasis added.

. . . (a) interesting generalizations . . . can often be made about events whose physical descriptions have nothing in common, (b) it is often the case that whether the physical descriptions of the events subsumed by these generalizations have anything in common is, in an obvious sense, entirely irrelevant to the truth of the generalizations . . . or, indeed, to any of their epistemologically important properties, and (c) the special sciences are very much in the business of making generalizations of this kind. (Fodor 1974, p. 103)

Fodor's main point is that the special sciences divide up the world to fit their explanatory purposes, and more often than not, these divisions can not be captured by simple statements about the natural kinds of the physical world. For example, functional analyses seem to offer the simplest characterizations of the relations between some psychological and neurological phenomena. At least some psychological states can apparently have a multiplicity of underlying physical realizations, and mentalistic idioms offer an efficient means of capturing their functional equivalence. (See Pattee 1973 and Simon 1973.) Functional analyses, unlike compositional characterizations, are not amenable to uniform microreduction, since they do not specify identities (or correspondences for that matter) between natural kinds. The first payment on the behavioral psychologists' promissory note is due, but the microreductions they await seem to be plagued with the same conceptual difficulties that their own analyses incur.

Fodor argues that in the reduction of a special science any compositional identities that might hold between the entities of  $T_1$  and  $T_2$  are, at best, quite complex, but also that in some cases they are succinctly specifiable by functional means only. It is not clear how unwieldy a compositional characterization of possible correspondences must be before it ceases to be informative either theoretically or ontologically. Fodor suggests that they need not be too extensive before most scientists opt for functional talk. (Also, it should perhaps be mentioned at this point that Fodor's account of functional analysis has been attacked as, if anything, oversimplified. Basically, the charge is that he confines his account to the inner workings of the black box. He treats functional systems as if they are completely closed, and consequently, in their specification he ignores the states of the larger systems in which they are imbedded.)

For Fodor these points are collectively sufficient to undercut the strong versions of the identity theory and any comprehensive program for the unity of science. Apparently, the strongest thesis the materialist can defend is what Fodor calls "token physicalism", which asserts only that "all the events that the sciences talk about are physical events." (Fodor

1974, p. 100)<sup>3</sup> Presumably, what Fodor means by this is that every event within the explanatory scope of science has a physical description. This thesis vindicates neither attempted microreductions of mentalistic psychology nor Causey's strongest claims for the unity of science by means of uniform microreductions.

The logical import of Causey's plea for synthetic identities as reduction functions is apparent enough, viz., to achieve the microreduction of  $T_2$  from  $T_1$  exclusively and not from the conjunction of  $T_1$  and some additional set of irreducible bridge laws. But whether or not this analysis justifies the ontological economizing Causey ascribes to uniform microreductions is quite another matter.

Causey has advocated the following four theses: (1) identity statements are necessary as the connecting principles in successful microreductions, (2) these identity statements do not require explanation (in the strong sense that they are in principle not subject to causal explanation), but (3) they do require empirical justification (because even though they are noncausal, they are, nevertheless, synthetic statements), and finally (4) "the justification of the identity connecting sentences will be accomplished within the entire framework of a successful reduction." (1977, p. 93) What demands further discussion is exactly how claims about hypothetical identities, necessary for the success of the microreductions that in turn seem to justify them (from (1) and (4)), can *guarantee* such thoroughgoing ontological simplifications. Of course a satisfactory solution for this difficulty would go a long way toward dispensing with the second horn of the dilemma mentioned above.

First of all, Causey needs to provide some criterion by which identity statements can be distinguished from weaker claims about nomological coextensionalities. In at least one place he claims that (2) is such a criterion. "Suppose that we have empirically justified ' $A$  iff  $B$ ', where  $A$  and  $B$  are attributes. Thus ' $A$  iff  $B$ ' is at least a correlation. I claim that it will be a contingent identity iff it does not require explanation." (1972a, p. 417) So it seems that attribute identities (and derivatively thing identities) *are* distinguishable from mere nomological coextensionalities, and that the reductions they facilitate contribute the requisite documentation.

There are, however, obvious problems. In light of his practical recommendations that *after* considerable progress in a science, "eventually there will remain a residual set of connecting sentences which will resist

<sup>3</sup>Wimsatt (1976) makes a similar claim: "Without type-correspondences, property identifications seem to be ruled out, and about the only kind of identity left is 'stuff' identity—roughly, that the stuff with the psychological properties is the same stuff as the stuff with the physical properties. Philosophers, concentrating on ontological dividends, have found this to be uninteresting and trivial."

continued efforts at their explanation,” and “it will be natural to hypothesize that this residual set is a set of identities,” (1977, pp. 98–99) it seems Causey is claiming not only that identity statements do not require explanation, but, moreover, that they are just those statements that are not explainable. But because the criterion is formulated negatively, it is impossible to definitively satisfy. Science regularly generates new explanations for what are often previously regarded as inexplicable phenomena. Causey has not missed this point, though, for at least in some passages he asserts that there can be no positively reliable criterion for identities. (cf., 1977, pp. 99, 150) Science is constantly changing and improving its explanations, and with each theoretical advance its categories, in part, change as well. Unfortunately, in some of Causey’s work these insights are apparently suppressed.

In their critical article Ager, Aronson, and Weingard (1974) also raise objections to Causey’s claim that a statement will be a contingent identity if and only if it does not require an explanation. They argue that this claim is either trivial or false. Since, in his reply, Causey assures us that what he intended by “explanation” was causal explanation, he avoids the charge of falsity, as his critics themselves concede. He then goes on to argue that his criterion is more than just trivially true. (1976, p. 335)

In his reply to Ager, et al., Causey (rightly) denies that he advocates the derivation of an identity from a biconditional. What he neither notices nor answers, however, is their larger accusation of which this is but a consequence, viz., that unless he can achieve just such a derivation, his criterion appears to be circular. They summarize the problem (between (1) and (4) above): “If you can join a postulated identity with a reducing theory, you cannot use the reducing theory to support the identity.” (Ager, et al. 1974, p. 131)

There is something right about this charge. There is also something wrong with it. What is wrong with it is that it misinterprets the aim of Causey’s program at this point. In citing their resistance to explanation, Causey is offering a means for *distinguishing* identities from nomological coextensionalities, *not* “supporting” them. His proposal is not some criterion by which we can establish the truth of identity claims.

What is right about their charge, however, is that such conceptual distinctions are only as good as the practical strategies they engender, especially when Causey explicitly claims as his ultimate goal just such practical advice. The point is that Causey does owe us a fuller account of how scientists do, in fact, proceed to justify their hypothetical claims about identities.

Causey never seems to think that the identification of entities should be motivated by anything more than their ability to promote ontological economizing. He explicitly states that “the aim of microreduction is to

*explain*  $T_2 \dots$  in terms of  $T_1$  *by means of identifying* the elements of  $Dom_2$  with elements of  $Dom_1 \dots$ ' (1972a, p. 411)<sup>4</sup> and that a full "understanding" of  $T_2$  and its domain is predicated upon just such an identification. Positing such identities, however, does not abrogate the reductionist's explanatory responsibilities. In Causey's account of uniform microreduction identity statements are crucial links in the explanations that in turn justify their use—hence the worries about circularity.

Causey's pleas for hypothetical identities, motivated exclusively on the grounds that they can generate ontological simplifications by means of what are otherwise unanticipated microreductions, more likely than not, will obscure very important explanatory issues. This is, for example, precisely the status of many materialist views according to Popper:

... appeal to simplicity can hardly be accepted as decisive. ... In particular, we should not deprive ourselves of interesting and challenging problems—problems that seem to indicate that our best theories are incorrect and incomplete—by persuading ourselves that the world would be simpler if they were not there. But it seems to me that modern materialists are doing just this. (1977, p. 62)

Popper's comments seem especially appropriate in light of Causey's claim that most of the reductions that have actually been achieved are uniform microreductions where all of the natural kinds at the various levels of analysis just happen to have neat, part-whole, identity relations.

However attractive it may seem, Fodor has argued that the microreduction of psychology is one of those microreductions that is completely "unanticipated otherwise", since among other things the mappings of psychological on to neural structures do not seem to display such uniformities. We have no compelling reasons to think that a hypothesized identity's promises of simplicity, whether theoretical or ontological, are a thoroughly reliable index for the plausibility of a microreduction. Suggestive source, perhaps, guarantor, hardly! Whatever plausibility the proposed microreduction and its accordant hypothesized identities can muster depends in the end upon their explanatory dividends.

Though Fodor is adamant that the microreduction of psychology can not be defended on ontological grounds, the functional characterization of psychological states (as mentioned above) is compatible with a weak form of the identity theory, viz., token physicalism, where the identities do not involve physical natural kinds, but rather the overall states of complex functional systems. If this version of the identity theory is acceptable to the materialists, then the battle over the role of identities in reductions is misplaced, since from token physicalism "it does not follow that the

<sup>4</sup>Some emphasis added.

natural kind predicates of psychology are co-extensive with the natural kind predicates of any other discipline (including physics).'' (Fodor 1974, p. 105) Token physicalism embodies identity claims which, because of the functional characterizations involved, are so nonspecific (at least from a compositional standpoint) that no one would be tempted to initiate a microreduction on the basis of their authority. Causey, however, in his reply to Fodor leaves little doubt as to both his dissatisfaction with this suggestion and his complete confidence in his model's propriety for the microreduction of any theories in psychology. (Causey 1977, pp. 142–58) Many materialists, including Causey, are generally convinced that a thoroughgoing microreduction of psychology can be accomplished.

Causey asserts that the superiority of the stronger versions of the psycho-physical identity theory is guaranteed by the rigorous homogeneity constraints which apply in a uniform microreduction. (This is the cash value of his response to Fodor.) The question, however, is not abetted by reasserting the validity of philosophical legislation. Commenting on identity theorists generally, Wimsatt pinpoints Causey's problem:

They are right in assuming that there must be something more to reduction than undifferentiated stuff-identity, but they are wrong in assuming that this something more is a stronger *identity* condition, such as property identity. The something more that makes a stuff-identity theory nontrivial is its explanatory import. (1976, p. 225)

Considering Hull, Schaffner, and Wimsatt's comments on reduction in genetics, Fodor's on the reduction of psychology, and other's on the various other "classical" cases of reduction,<sup>5</sup> Causey's insistence upon uniform microreduction surely deserves reexamination. His model seems to risk being skewered on the second horn of the dilemma. For Causey, though, this may well be a small price to pay for his model to retain its eliminative advantages.

The crucial point is that Causey fails to appreciate just *how* hypothetical identity attributions generally are in scientific theorizing. Instead of identities being assigned late in the game to those coextensionalities which prove persistently recalcitrant to explanation, they are often proposed relatively early, initiating wholly new lines of research. When in doubt (many scientists) assume the truth of a proposed identity until empirical research clearly indicates otherwise. The postulation of identities is a research tool for extending the *explanatory range* of theories.<sup>6</sup> They are not proposed as the grounds for justifying eliminative moves in microreductions (even if, after the fact, they may be cited as such).

<sup>5</sup>For an excellent bibliography of the relevant literature, see Wimsatt (1978).

<sup>6</sup>See Darden and Maull (1977), Maull (1977), Nickles (1976), and Wimsatt (1976).



These identity statements are genuinely hypothetical.<sup>7</sup> The comprehensive identification of all of the various entities' attributes is usually unnecessary, since only a small set of these are pertinent to the immediate explanatory goals of the reduction. The attributes of interest are the *causal* properties of these entities which are independently specified in the relevant theories. Consequently, though synthetic identities may not require causal explanation themselves, this does not preclude the need for employing causal theories in their justification.<sup>8</sup> Causey is quite right to claim that there is no causal explanation for why two entities are identical. He is wrong, however, in concluding from this that we need not attend to causal explanations when justifying why we would hold that certain entities are identical. As Causey recognizes, successful explanations constitute our best source of evidence for such identity claims. (1977, pp. 98–99) But this should not be regarded as some rear guard maneuver carried out after the fact. We posit synthetic identities in order to consider the plausibility of a proposed reductive explanation. Synthetic identity statements cannot *deus ex machina* certify these explanations.

The ontological economies promised by uniform microreduction can easily distract us from the fundamental explanatory issues. The transitivity of interlevel reductions is the product of the explanatory abilities of the relevant theories and not of the identities which may be postulated as reduction functions, since it is precisely these explanations that ultimately justify those postulations and define their range. Because identities *can* be hypothesized and because they *might* facilitate theoretical or ontological simplifications is no reason for holding that these hypotheses are, in fact, true. Those decisions are made on the basis of the explanatory strength of the theories in which they are imbedded and are, therefore, ultimately empirical matters. (Consequently, Causey's reply to Fodor seems a bit doctrinaire.)<sup>9</sup>

Perhaps this whole discussion has been somewhat misleading, though. Ontological economizing is not really all so central an issue in interlevel reduction. The goal of such intertheoretic research is to increase the explanatory power of science; it is not to decrease the size of our ontologies. Everyone agrees that bridge laws and identity statements have comparable logical muscle in reduction contexts. My point, however, has been that neither have sufficient strength ontologically to legitimize Causey's eliminativism. The identity claims in most (interlevel) reductive explanations are generally quite restricted and go no farther afield than the identification of the causally (read "explanatorily") interesting properties of

<sup>7</sup>On this point see Nagel (1965).

<sup>8</sup>On this point see Maull (1977).

<sup>9</sup>In his (1979) Richardson defends the possibility of ontological simplifications in interlevel reductions similar to those that Causey foresees. He does so, however, without making the sorts of in principle assumptions that seem so prevalent in Causey's work.

the entities involved. This may not reduce the size of our ontologies very much, but it is quite adequate from the standpoint of explanatory suggestiveness.

## REFERENCES

- Ager, T., Aronson, J., and Weingard, R. (1974), "Are Bridge Laws Really Necessary?", *Noûs* 8: 119–34.
- Bantz, D. (1976), "Does Physics Explain Chemistry", Ph.D. dissertation, Committee in the Conceptual Foundations of Science, University of Chicago.
- Causey, R. (1972a), "Attribute Identities in Microreductions", *The Journal of Philosophy* 64: 407–22.
- Causey, R. (1972b), "Uniform Microreductions", *Synthese* 25: 176–218.
- Causey, R. (1976), "Identities and Reduction: A Reply", *Noûs* 10: 333–37.
- Causey, R. (1977), *Unity of Science*. Dordrecht: Reidel.
- Darden, L. and Maull, N. (1977), "Interfield Theories", *Philosophy of Science* 44: 43–64.
- Fodor, J. (1968), *Psychological Explanation*. New York: Random House.
- Fodor, J. (1974), "Special Sciences (Or: The Disunity of Science as a Working Hypothesis)", *Synthese* 28: 97–115.
- Hull, D. (1972), "Reduction in Genetics—Biology or Philosophy", *Philosophy of Science* 39: 491–98.
- Hull, D. (1974), *Philosophy of Biological Science*. Englewood Cliffs: Prentice-Hall.
- Hull, D. (1976), "Informal Aspects of Theory Reduction", in *PSA 1974*, Cohen, R., Hooker, C., Michalos, A. and van Evra, J. (eds.), Dordrecht: Reidel, 633–52.
- Maull, N. (1977), "Unifying Science Without Reduction", *Studies in the History and Philosophy of Science* 8: 143–62.
- Nagel, T. (1965), "Physicalism", *The Philosophical Review* 74: 339–56.
- Nickles, T. (1976), "Theory Generalization, Problem Reduction, and the Unity of Science", in *PSA 1974*, Cohen, et al. (eds.), Dordrecht: Reidel: 33–74.
- Pattee, H. (1973), "The Physical Basis and Origin of Hierarchical Control", in *Hierarchy Theory: The Challenge of Complex Systems*, Pattee, H. (ed.), New York: George Braziller, 71–108.
- Popper, K. and Eccles, J. (1977), *The Self and its Brain*. New York: Springer International.
- Richardson, R. (1979), "Functionalism and Reductionism", *Philosophy of Science* 46: 533–58.
- Ruse, M. (1976), "Reduction in Genetics", in *PSA 1974*, Cohen et al. (eds.), Dordrecht: Reidel: 653–69.
- Schaffner, K. (1974), "The Peripherality of Reduction in the Development of Molecular Biology", *Journal for the History of Biology* 7: 111–39.
- Schaffner, K. (1976), "Reductionism in Biology: Prospects and Problems", in *PSA 1974*, Cohen, et al. (eds.), Dordrecht: Reidel: 613–32.
- Simon, H. (1973), "The Organization of Complex Systems", in *Hierarchy Theory: The Challenge of Complex Systems*, Pattee, H. (ed.), New York: George Braziller: 1–27.
- Wade, M. (1978), "A Critical Review of the Models of Group Selection", *Quarterly Review of Biology* 53: 101–14.
- Wimsatt, W. (1974), "Complexity and Organization", in *PSA 1972*, Cohen, R. and Schaffner, K. (eds.), Dordrecht: Reidel, 67–86.
- Wimsatt, W. (1976), "Reductionism, Levels of Organization, and the Mind-Body Problem", in *Consciousness and the Brain*, Globus, G., Maxwell, G., and Savodnik, I. (eds.), New York: Plenum Press, 205–67.
- Wimsatt, W. (1978), "Reduction and Reductionism", in *Current Problems in Philosophy of Science*, Asquith, P. and Kyburg, H. (eds.), East Lansing: Philosophy of Science Association.
- Wimsatt, W. (1980), "Reductionistic Research Strategies and their Biases in the Units of Selection Controversy", in *Scientific Discovery Volume II: Case Studies*, Nickles, T. (ed.), Dordrecht: Reidel.