APPROACHES TO REDUCTION*

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Four current accounts of theory reduction are presented, first informally and then formally: (1) an account of direct theory reduction that is based on the contributions of Nagel, Woodger, and Quine, (2) an indirect reduction paradigm due to Kemeny and Oppenheim, (3) an "isomorphic model" schema traceable to Suppes, and (4) a theory of reduction that is based on the work of Popper, Feyerabend, and Kuhn. Reference is made, in an attempt to choose between these schemas, to the explanation of physical optics by Maxwell's electromagnetic theory, and to the revisions of genetics necessitated by partial biochemical reductions of genetics. A more general reduction schema is proposed which: (1) yields as special cases the four reduction paradigms considered above, (2) seems to be in better accord with both the canons of logic and actual scientific practice, and (3) clarifies the problems of meaning variance and ontological reduction.

1. Introduction. There is a logical problem concerning exactly what happens when one scientific theory is "explained" by a theory from a different branch of science. For example, we might ask what (logically) is going on when a molecular biologist attempts to characterize the gene in chemical terms, and purports to account for Mendelian inheritance by alluding to enzyme action and DNA structure. The connection between the terms employed in the different theories is not a self-evident one, nor are there obvious criteria to which we can appeal in judging whether such an explanation has in fact been achieved.

Intertheoretic explanation in which one theory is explained by another theory, usually formulated for a different domain, is generally termed *theory reduction*. In recent years a number of writers have been intrigued by this mode of scientific activity and have attempted to work out in a relatively rigorous manner an account of the logic of reduction. I am going to allude to some of these attempts at rational reconstructions of reduction in an effort to formulate a different conception which I believe to be more in accord with both the canons of logic and actual scientific practice. It turns out that these earlier attempts are special (and extreme) cases of my own more general characterization.

I have grouped the various approaches of different authors under four paradigms for both the sake of simplicity, and because the combined claims of the associated writers more often than not made that common paradigm stronger than any of their conceptions taken individually. Let us consider these four paradigms, first informally and then from a more formal standpoint.

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2. Four Reduction Paradigms. The first of the paradigms which I wish to consider has been presented in slightly different ways by Ernest Nagel [13], [14], J. H. Woodger [26], and W. V. Quine [18], and accordingly will be termed the Nagel-Woodger-Quine reduction paradigm (NWQ for short).

1. The NWQ paradigm. Essentially this account of reduction can be characterized as *direct* reduction—in which the basic terms (and entities) of one theory are related to the basic terms (and entities) of the other, (assuming that the reduced theory is an adequate one) and the axioms and laws of the reduced theory are derivable from the reducing theory. The last assertion must be expanded somewhat, for quite often in intertheoretic explanation terms appear in the reduced theory which are not part of the reducing theory, e.g., the term "gene" does not appear in organic chemistry. Thus we have to conjoin additional sentences to the reducing theory which associate these terms of the reduced or secondary theory with combinations of terms from the vocabulary of the reducing or primary theory. (Cf. [13], pp. 352-54) The exact logical nature of these associating sentences will be discussed later.

As an example of NWQ reduction, I might cite Nagel's example of the reduction of thermodynamics by statistical mechanics which occurred in the latter half of the nineteenth century. ([13], pp. 342-45.)

2. The KO paradigm. This is due to the work of J. G. Kemeny and P. Oppenheim on reduction [11], and might best be termed a paradigm of *indirect* reduction, since one does not obtain a theory T_2 from T_1 in the usual case of reduction, so reducing T_2 by T_1; rather one obtains identical observable predictions from both theories (though T_1 may predict more). We shall bring out the implications of this assertion in discussion below. An example of this type of reduction might be the explanation, by Lavoisier's oxidation theory, of all the observable facts which the phlogiston theory explained. Notice in this case we would not be able to define "phlogiston" in the terms of the oxidation theory. We must also consider:

3. The PFK paradigm. This abbreviation derives from the different studies of Popper [15], [16], Feyerabend [6], [7], and Kuhn [12] on the relation of later scientific theories to earlier ones, and the difficulties involved in obtaining an exact fit between assertions of an older theory and "special cases" of newer theories. As the reader will see below, one might well question whether the PFK paradigm is to count as a legitimate reconstruction of reduction, rather than the denial of the possibility of the occurrence of reductions. I do believe that there are good reasons for considering it a different approach to the problems of reduction, and an approach which is not totally negative.

The claim made in this paradigm is not that T_2 is derivable from T_1 in any formal sense of derivable, or even that T_2 can have its primitive terms expressed in the language of T_1, rather T_1 is able to explain why T_2 "worked", and also to "correct" T_2. The relation between the theories is not one of strict deduction of T_2 from T_1. Nevertheless in certain cases one can obtain T_2 from T_1 deductively: if one conjoins to T_1 certain contrary to fact premises which would in certain experimental contexts (relative to the state of a science) not be experimentally falsifiable, one can obtain T_2.

A relatively uncomplicated example of such a "reduction" is the explanation of the Galilean law of free fall: that the distance an object has fallen is proportional to the square of the time of its descent, by the axioms of Newtonian mechanics plus the law of universal gravitation. The Galilean law is not exactly derivable—rather a more complicated law is derivable which gives experimental results which are quite close to the predictions of the Galilean law. The sentences expressing these laws are still different, however, and could only be said to be formally identical if the earth's radius were infinitely large, which it is not. (Cf. [6], pp. 46-48.) Consequently the reduced theory is only derivable approximately from the reducing theory —and "approximation" introduces serious complications for any general formal analysis of reduction. (Cf. [6], p. 48.)

4. *The Suppes paradigm.*[1] I now turn to the last reduction paradigm which I shall consider. This has been proposed by P. Suppes [23], [24] who has suggested that:

> Many of the problems formulated in connection with the question of reducing one science to another may be formulated as a series of problems using the notion of a representation theorem for the models of a theory. For instance, the thesis that psychology may be reduced to physiology would be for many people appropriately established if one could show that for any model of a psychological theory, it was possible to construct an isomorphic model within physiological theory ([24], p. 5).

Another example of this type of reduction is given by Suppes when he states that:

> To show in a sharp sense that thermodynamics may be reduced to statistical mechanics, we would need to axiomatize both disciplines by defining appropriate set theoretical predicates, and then show that given any model T of thermodynamics we may find a model of statistical mechanics on the basis of which we may construct a model isomorphic to T ([23], p. 271).

Having presented both synopses and programmatic examples of the four most generally held positions on reduction, let me now attempt to characterize these paradigms formally so as to discuss their interrelations with maximum exactitude.

3. Formal Presentation of the Paradigms. It must first be realized that all the authors thus far discussed (with the possible exception of Feyerabend and Kuhn) agree that reduced and reducing theories must be axiomatized. Usually this is done in accordance with the spirit of the logistic method as developed for example in Church's *Introduction in Mathematical Logic* ([5], pp. 47-58). Thus one can economically wrap up the whole of the theory under consideration, and proceed to discuss the theory in terms of the axioms and primitive predicates. We shall assume then that any theories involved are sufficiently well axiomatized, and go on to consider the various formalizations of the paradigms.

A. The NWQ Paradigm. A reduction will be termed an NWQ reduction instance if and only if:

(1) All the primitive terms $q_1 \ldots q_n$ appearing in the secondary theory T_2 appear in the primary theory T_1 (in the case of homogeneous reductions) or are associated with one or more of T_1's terms by a reduction function[2] such that:

[1] In all fairness I should perhaps have called this approach the Suppes-Adams paradigm, as E. W. Adams has worked out some of the implications of Suppes approach. See his [2].

[2] This notion of a "reduction function" is a combining of the concept of the "associating sentences" mentioned on p. 138 above with W. V. Quine's "proxy function," introduced by him in his article on Ontological Reduction [18].

(a) it is possible to set up a one-to-one correspondence between individuals or groups of individuals of T_1 and T_2 or between individuals of one theory and a subclass of the groups of the other. (A rigid body correlated with an aggregate of particles is an example of this.) This one-to-one correspondence can be made more precise by, following Quine, introducing a reduction function whose values exhaust the universe of T_2 for arguments in the universe of T_1.

(b) All the primitive predicates of T_2, say F_i^n are effectively associated with an open sentence of T_1 in n free variables "in such a way that F_i^n is fulfilled by an n-tuple of values of the . . . reduction function always and only when the open sentence is fulfilled by the corresponding n-tuple of arguments."[3]

(c) All reduction functions cited in (a) and (b) above be specifiable and have empirical support. (They will generally be interpreted as synthetic sentences.)[4]

(2) Given the fulfillment of condition (1), that T_2 be derivable, i.e., be a deductive consequence of T_1 augmented by the reduction functions described in (a), (b), and (c) above.

B. The KO Paradigm. A reduction will be termed a KO reduction instance if and only if:

(1) T_2 has among its primitive terms, terms which are not in T_1.

(2) Any part of the observational data associated with T_2 is explainable by T_1.

(3) T_1 is at least as well systematized as T_2.[5]

C. The PFK Paradigm. A reduction will be termed a PFK reduction instance if and only if:

(1) Of the primitive terms $q_1 \ldots q_n$ of T_2, there is at least one q_i which cannot be identified or correlated with a p_i of T_1, or any combination of p's of T_1 without asserting a self-contradiction or a false statement.

(2) Nevertheless theory T_2 can be "explained" by T_1 in the nonformal sense (not the Hempel-Oppenheim sense) that T_1 can yield a deductive consequence T_2^* which may result in predictions numerically "very close" to the predictions of T_2.

(3) T_2^* should "correct" T_2 in the sense of providing more accurate experimentally verifiable predictions than T_2; it should also point out why T_2 was incorrect (e.g., that it ignored a crucial variable, and T_2^* (or T_1 for that matter) should indicate why T_2 worked as well as it did.

[3] This is essentially a quote from Quine ([18], p 215) writing "F_i^n " for "the predicate" and "reduction functions" for "proxy function."

[4] But see Nagel's correspondence rule interpretation for another possible reading ([13], pp. 354-57).

[5] The notion of "systematized" is that of a measure which combines strength with simplicity. A theory which is more complex, but which is much more powerful than a comparable theory is said to be better systematized. The notion is apparently an intuitive one. Cf. [11], p. 11f.

D. The Suppes Paradigm. A reduction will be termed a Suppes type reduction instance if and only if:

For any model M_2 of the reduced theory, we can find a model M_1 of the reducing theory such that one can construct a model M_1^* (M_1^* may be M_1) such that M_1^* is isomorphic to M_2.

Suppes does not give any general definition of isomorphism, and in fact warns us that "a satisfactory general definition of isomorphism for two set-theoretical entities of any kind is difficult if not impossible to formulate" ([23], p. 262). Nevertheless to give some sense of exactness to this term it does seem plausible to use Church's quite general definition to the effect that:

> Two models of a system of postulates are said to be *isomorphic* if there is a one-to-one correspondence between the two domains of individuals used in the two models such that the values given in the two models to any particular free variable occurring in the representing forms of the postulates always correspond to each other according to this one-to-one correspondence. I.e., if in the first model the value a is given to the individual variable *a*, and in the second model the value a′ is given to *a*, then a must correspond to a′ in the one-to-one correspondence between the two domains of individuals; and if in the first model the value Φ is given to an n-ary functional variable *f*, while in the second model the value Φ′ is given to *f*, then the propositional functions Φ and Φ′ must be so related that, whenever the individuals $a_1, a_2, \ldots a_n$ of the first domain of individuals correspond in order to the individuals $a_1', a_2', \ldots, a_n'$ of the second domain, the value Φ $(a_1, a_2, \ldots a_n)$ is the same as the value Φ' $(a_1', a_2', \ldots, a_n')$ ([5], pp. 329-30).

If this definition (or one similar to it) is acceptable, I think it is possible to prove an interesting theorem that indicates something important about the relation of the Suppes approach to the NWQ paradigm. Before I do this however, I should like to cast a glance at the *modus operandi* of the practicing scientist to see how he construes reduction. With this information digested we will not only be able to discuss interesting formal relations among the paradigms, but also to determine to what extent which, if any, of the paradigms is an adequate rational reconstruction of theory reduction in the sciences.

4. Scientific Interlude. There are a number of examples of reduction in the literature of the natural sciences to which we can appeal in order to adjudicate these differing approaches. I am going to refer rather schematically to two such reduction instances in an attempt to argue that in actual practice, reduction conforms best to the PFK paradigm, but with certain important reservations and additions.

First consider the reduction of physical optics by Maxwell's electromagnetic theory. It is not very difficult to construct axiomatizations of these two theories as they were formulated in the late 19th century. Also, one can show that the basic wave equation for the light wave is deducible from Maxwell's equations describing the interaction of electrical and magnetic fields. When the appropriate boundary conditions are specified, it is possible to "deduce" two basic laws of physical optics: Snell's law of refraction and Fresnel's law of intensity ratios. But there are important qualifications on these "deductions."

(1) In the first place we need suitable reduction functions which will identify light waves with electromagnetic waves of a certain frequency range, and the electric vector with the light vector.

(2) Even with these appropriate reduction functions, we will discover that some reduction instances are exact, whereas others are not—i.e., are only approximate. Snell's law comes out without change, but the Fresnel ratios have an additional factor in them when they are derived from Maxwell's theory which does not appear in them originally.[6] The corrective effect of this factor is small, but significant, for it tells us that the behavior of light is dependent on the magnetic properties of the medium through which it passes.

(3) In the late 19th century, there were a number of theoreticians attempting to solve the problem of diffraction by a black screen—a screen which absorbs all the light which falls upon it. It turns out that this concept of a "black screen" is impossible to define in the context of electromagnetic theory since:

> the property "black" cannot be defined by boundary conditions within the realm of Maxwell's theory. Therefore diffraction by a black screen cannot be formulated as a boundary value problem ([21], p. 266).

The conclusion that can be drawn from these examples is that what Maxwell's theory entails is a theory of physical optics which bears close relations to the earlier theory of physical optics—but which certainly is not identical with it.

Similar conclusions are supported by an examination of the partial reductions of genetics to chemical theory. Before turning to this example, however, let me preface the account by noting an example of a change in a scientific theory *not* caused by a reduction. If one examines Mendel's original suppositions regarding the behavior of heredity determiners, he discovers that Mendel was under the impression that the determiners were statistically independent of one another—i.e., that the chance of a pea plant being long stemmed had no effect on the chances of the peas it would bear being wrinkled or smooth. It turns out that this "independence" is an extreme case: the determiners (called genes) are for the most part "linked" with other genes. Now this bit of information is cited not as a piece of information directly relevent to the logic of reduction—but as an instance of the change of a concept in science that results from the interaction of theory with progressive experimentation: as science progresses, our basic scientific concepts evolve.

The tie-in with the logic of reduction is that a similar type of concept evolution and redefinition occurs as a result of reductions. Specifically the concept biologists have of the gene changes as a result of the chemical reduction of segments of genetics. Let me point out how this comes about.

Even as late as the early 1950's, the gene was variously defined as:

(1) the smallest section of the chromosome that could undergo mutation, (2) the smallest section of a chromosome that could recombine with its homologous chromo-

[6] The original Fresnel ratios are $\dfrac{A_{\text{refl.}}}{A_{\text{incid.}}} = \dfrac{\sin(i - r)}{\sin(i + r)}$;
the expression deducible from Maxwell's theory, after using the appropriate reduction function is:

$$\frac{A_{\text{incid.}}}{A_{\text{refl.}}} = \tfrac{1}{2}\left[1 \pm \frac{\mu_1}{\mu_2} \bullet \frac{\sin i \cos r}{\sin r \cos i}\right]$$

(cf. [21], p. 16), which reduces to the above simpler form if $\mu_1 = \mu_2$.

some in crossing over, and (3) that section of the chromosome functionally responsible for a unit character. (One can see in this characterization the influence of cytological observations on the definition of the gene, which originally was implicitly defined as a unit of segregation, and then as a unit of classical recombination (cf. [4], p. 62).) These three descriptions were understood to be extensionally equivalent.

These three alternative ways of defining the gene however turned out to refer to different things, for recent experimental work in bio-*chemistry* has indicated that (1) and (2) above have as physical referents much smaller DNA sequences than does (3). Studies on *E. Coli* bacteria, *Aspergillus,* and several types of bacteriophage, support the thesis that the unit of recombination and mutation is about two nucleotide pairs, whereas the unit of function is about one thousand nucleotide pairs ([22] pp. 128-30). Although, as pointed out above, this need for more careful and corrected redefinition is usually a consequence of the ongoing progress of science, it seems to be the case particularly in sciences which are in the process of being reduced to other sciences.

In point of fact S. Benzer has coined terms for the different senses of gene referred to above: the unit of mutation is called a *muton,* the unit of recombination a *recon,* and the unit of function a *cistron* [3]. The terms seem to have been adopted by most practicing geneticists and molecular biologists.

As biologists now understand the situation, it is the cistron that is most closely identified with the traditional notion of a gene. The cistron is usually characterized by what it produces—not a unit character in the Mendelian sense—but a peptide chain, which "may constitute a complete biologically active protein in itself or which may become secondarily aggregated with other peptide chains to form the active protein. The old slogan 'one gene one enzyme' has accordingly been *replaced* by the more precise formulation 'one cistron, one peptide chain'!" ([9], p. 270).

The logical point of this exposition is to underline the fact that a reduction can give us new information about the reduced science, and change the way we understand the entites of that domain to behave. Often there are changes of definition, and different modes of activity are ascribed consequent to reduction. Whether we call this "meaning change"—as Feyerabend and Kuhn wish to—or whether we do not—as Achinstein and Shapere do not[7]—seems to be an issue that awaits a clear conception of the term "meaning."

At any rate I have shown that there are alterations in the reduced theory—and that the earlier theory usually changes to incorporate these corrections. Note also, to emphasize a point that has not been stressed as yet, that the reduction functions thus far alluded to are to be understood as synthetic identities—somewhat analogous to that synthetic identity expressed in the sentence "The morning star is the evening star."[8] The crucial difference here is that in our account *theoretical* notions flank the "is" of identity.[9]

[7] See Feyerabend [6], [7], Kuhn [12], pp. 100-101, Achinstein [1], and Shapere [19] for arguments relevant to this controversy.

[8] See Feigl [8], pp. 438-39, and Sklar [20] for a discussion of the role of synthetic identities in reduction.

[9] It is only by use of synthetic identities that reduction can decrease the ontology of the universe, without imputing "unreality" to some theoretical entities.

I am now in a position to develop my reduction paradigm and to consider the relations it bears to the previously discussed approaches.

5. The General Reduction Paradigm. Attending to the difficulties confronting simple deducibility and concept invariance discussed above, I offer the following as an explication of reduction which is more general and more adequate than those mentioned thusfar.

Reduction occurs if and only if: (1) All the primitive terms $q_1 \ldots q_n$ appearing in the *corrected* secondary theory T_2^* appear in the primary theory T_1 (in the case of homogeneous reductions) or are associated with one or more of T_1's terms such that: (a) it is possible to set up a one-to-one correspondence representing synthetic identity between individuals or groups of individuals of T_1 and T_2^* or between individuals of one theory and a subclass of the groups of the other, in such a way that a reduction function can be specified which values exhaust the universe of T_2^* for arguments in the universe of T_1; (b) all the primitive predicates of T_2^*, i.e., any $F^n{}_i$, are effectively associated with an open sentence of T_1 in n free variables in such a way that $F^n{}_i$ is fulfilled by an n-tuple of values of the reduction function always and only when the open sentence is fulfilled by the corresponding n-tuple of arguments (After Quine, [18]); (c) all reduction functions cited in (a) and (b) above be specifiable, have empirical support, and in general be interpretable as expressing referential identity. (2) Given the fulfillment of condition (1) that T_2^* be derivable from T_1 when T_1 is conjoined with the reduction functions mentioned above. (3) T_2^* corrects T_2 in the sense of providing more accurate experimentally verifiable predictions than T_2 in almost all cases (identical results cannot be ruled out however), and should also indicate why T_2 was incorrect (e.g., crucial variable ignored), and why it worked as well as it did. (4) T_2 should be explicable by T_1 in the non-formal sense that T_1 yields a deductive consequence (when supplemented by reduction functions) T_2^* which bears a close similarity to T_2 and produces numerical predictions which are "very close" to T_2's. Finally (5) The relations between T_2 and T_2^* should be one of strong analogy—that is (in current jargon) they possess a large "positive analogy."

To clarify the role that the reduction functions play, as well as to put some flesh on this logical skeleton, it might be well to consider an example from the reduction of genetics by biochemistry. Here the reduction functions of the type discussed in condition (1) (a) above identify genes with DNA sequences: $\text{gene}_1 = f(\text{DRA segment}_1)$.[10] The functions of the (1) (b) type specify that a predicate from genetics—such as "dominant"—is effectively associated with an open sentence from bio-chemistry: "x is capable of directing the synthesis of an active enzyme," in such a way that "gene_1 is dominant" always and only when "DNA segment_1 is capable of directing the synthesis of an active enzyme."

The T_2 theory in this case is the genetics of the 1950's (as referred to earlier) and the T_2^* is the "corrected" genetics of today which employs the terms cistron, muton, and recon. There are certainly strong analogies between T_2 and T_2^*, and the exemplification of the other conditions in the context of this example should be obvious.

[10] I must emphasize that the term "gene" is here used in the sense of "cistron." The term "gene" is still employed in molecular biology but in this newer sense. Cf. [27] and also the discussion above. I am indebted to F. John Clendinnen for bringing this point to my attention.

When conditions (4) and (5) fail, then one might have to fall back on something like the Kemeny-Oppenheim paradigm, assuming that there is a clear distinction between theoretical and observational terms, and that the latter are common to T_1 and T_2 (or better, that T_2's O terms be a subset of T_1's). There is no theory relation in this case—only adequate explanation of the observational predictions of the previous theory.

When T_2 is identical with T_2^*, then we have a case of NWQ reduction, the only difference being that I am inclined to agree with some of the recent writers on reduction, namely Feigl [8] and Sklar [20], that the reduction functions are better understood as synthetic identifications than as physical hypotheses expressing correlations or as correspondence rules. (Cf. Nagel [13], pp. 354-58, and 366.)

As regards the Suppes paradigm, I think it can be shown that this is a weaker form of the NWQ approach—in fact so weak as it stands that it will not do as an adequate reduction paradigm. The relation to the NWQ account can be established by a theorem:

Theorem. If it is possible to construct an NWQ reduction for T_1 and T_2, then a Suppes reduction is also possible.

Proof. A Suppes reduction is established if for any model of T_2 we can construct an isomorphic model in T_1. The reduction functions cited in the NWQ type of reduction insure: (a) either a one-to-one correlation of the individuals of a theory's ontology with the reducing theory's ontology, or a one-to-one correlation of aggregates of a theory's ontology with individuals or aggregates of the other's ontology—and (b) also serve to maintain identical values of correlated predicates and open sentences when the argument places are filled with correlated individuals and/or aggregates. But to satisfy conditions (a) and (b) is tantamount to specifying that if T_2 has a model M_2, then T_1 must have a model M_1 which is isomorphic to M_2, since equicardinality is insured by (a), and identity of predicate value is assured by (b).

My second 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. I prefer to see it as elucidating the *methodology* of reduction, rather than the *logic* of reduction, in the sense that if an NWQ reduction is to be constructed, then a Suppes type of reduction must be shown to be the case. Revealing this isomorphism is in fact one of the things which a scientist does when he is attempting to demonstrate a reduction, as a perusal of the scientific literature on reductions will confirm.

Finally one might note that this general analysis is quite close to the PFK paradigm, but it differs from the beliefs of Popper, Feyerabend, and Kuhn to the extent that it asserts that reduction *is* possible. Their analysis of the relations of scientific theories attempts to reveal divergence, incompatability, and non-connectability; I have tried to utilize their notions to work in the other direction. My paradigm is again strengthened by the requirement that there be specifiable reduction functions interpretable as synthetic identities—this clarifies the problem of ontological reduction (at least in the empirical sciences). Recall also that I have introduced the notion that T_2 and T_2^* can have a strong positive analogy, and that T_2 can be considered as reduced by T_1 in this case. This last point is perhaps the most programmatic, for not much work of any import has been done on the logic of analogy.[11]

In conclusion, let me state that I think reduction is a scientific fact, and though it is not the simple thing that some of the earlier writers had taken it to be, it is not so recalcitrant that some general logic of reduction cannot be proposed.

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