DISCUSSION

REDUCTION IN GENETICS*

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In a recent paper, William K. Goosens (1978) objects to the arguments I set out some time ago attacking the logical empiricist analysis of reduction as applied to genetics (Hull 1972, 1973, 1974, 1976a). In these works I did not argue against the claim that Mendelian genetics was being reduced to molecular biology. Nor did I conclude, as Goosens asserts, that in the case of genetics, "reduction is insignificant" (p. 93). To the contrary, I repeatedly stated that, "given our pre-analytic intuitions about reduction," the reduction of Mendelian to molecular genetics "*is* a case of reduction, a paradigm case" (Hull 1974 p. 44). And in agreement with Kenneth Schaffner (1967) I argued that reduction "in some very important, pre-analytic sense has taken place and is taking place in genetics" (Hull 1973, p. 634). The target of my objections to reduction in genetics was not "reduction" in some pre-analytic sense. Nor was it the various explications which have appeared since. Rather it was the notion of theory reduction set out a generation ago by such logical empiricists as Ernest Nagel (1961). The particular version of this analysis which I chose to attack was that presented by Schaffner (1967, 1969); for Schaffner's later views, see his (1974, 1976, 1977). I chose Schaffner's explication to attack because I thought it was the best of its kind and because it was the only defense of the logical empiricist analysis at the time which used genetics as one of its chief examples. Contrary to Goosens' assumption, in criticizing Schaffner's explication, I did not thereby "endorse" it (p. 82).

According to the logical empiricists, theories are axiomatic systems and reduction is the deduction of one theory from another. For example, Nagel (1961, pp. 361–2) states that "whether a given science is reducible to another cannot in the abstract be usefully raised without reference to some particular stage of development of the two disciplines. Questions about reducibility can be profitably discussed only if they

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are made definite by specifying the established content at a given date of the sciences under consideration." The chief modification introduced by Schaffner is the official recognition in his model of the need to "correct" the relevant theories if the derivation is to be carried through. The contrast between reduction and replacement thereby ceases to be a dichotomy and becomes a continuum. According to Schaffner, the degree to which the relation is one of reduction rather than replacement depends on how similar the corrected and uncorrected theories are to each other. Goosens (p. 83) notes the "strain" which results "as long as reduction and replacement are seen as alternatives rather than ends of a scale." But throughout our dispute, Schaffner, Ruse (1971a, 1971b, 1973, 1976), and I viewed reduction and replacement as opposite ends of a continuum; e.g., a "second fault with this simple dichotomous classification is that it implies that the distinction between replacement and reduction is reasonably sharp, when it is not. Rather it more closely resembles a continuum" (Hull 1973, p. 633; see also Hull 1974, pp. 9–10). Schaffner (1977) has since modified his position, choosing to recognize a generic sense of "reduction" which includes within it reduction in its earlier, specific sense, and replacement.

Goosens sees himself as defending "reduction in its modern classical, logical-empirical form" (p. 75) against the objections raised by early Ruse (1971a, 1971b) and me. He thinks that Schaffner (1974, 1976) and later Ruse (1973, 1976) give too much ground. In particular, he responds to several examples of differences between Mendelian and molecular genetics which Ruse and I discussed in our early writings. Several of the objections which he raises I think are well-taken. For example, early on, Ruse made much of the differences between Mendelian and molecular genes. At one stage in the development of genetics, geneticists briefly thought that the units of mutation, recombination and function might be coextensive. However, they turned out not to be. Goosens (p. 78–9) argues that the identity of mutons, recons and cistrons was never very central to Mendelian genetics. From the beginning I never thought that it was. The problems I raised concerned the existence of operators, intra-cistronic complementation, etc. Even so, I concluded that the differences between Mendelian and molecular genes were not all that great. "Although the reduction function connecting 'gene' with the appropriate terms in molecular genetics is not so simple as one might at first expect, at least it does not seem as if it will be prohibitively complex" (Hull 1973, p. 629; see also 1974, p. 37).

My objections to both the practicality and possibility of reducing (*sensu* Nagel) Mendelian genetics to molecular biology concerned the

predicate terms of Mendelian genetics. Here, once again, some of Goosens' criticisms are well-taken, especially his complaints about the relevance of my "reclassification" arguments. I have only one objection to raise in this connection. Goosens (p. 84) makes it sound as if I was unaware that Mendelian genetics applies as readily to molecularly-characterized traits as to gross phenotypic traits, when I myself repeatedly made this point (Hull 1972, p. 493; 1973, p. 625; 1974, pp. 23, 33, 38). My chief objection to the applicability of the logical empiricist analysis of reduction to genetics was, as Goosens recognizes, that numerous different patterns of Mendelian inheritance can result from the same molecular mechanism. Goosens' criticisms of this argument, however, depend crucially on discrepancies between what he and I think constitutes the logical empiricist analysis of reduction.

According to Goosens (p. 76), the logical empiricist analysis of reduction does not, contra Nagel, concern "fixed" and "historically dated" theories but research programs. For example, numerous formulations can be found in the biological literature which have been termed "Mendelian" genetics. Mendelian genetics is "not these theories singularly, collectively, or serially, but rather a research program within which these theories developed" (p. 77). Theories within a research program can change without the research program itself being abandoned. "Although all parts (of a theory) are subject to both modification and replacement, alterations differ in the degree to which they represent an abandonment of the program as opposed to an articulation of it" (p. 77). Furthermore, these versions are related to each other, not by similarity, but by the part-whole relation. Just as two organisms do not have to be similar to each other to be part of the same species, two versions of a theory need not be similar to each other to be part of the same research program. The part-whole relation is what matters (p. 92).

I find Goosens' views on both species and theories highly attractive. I should. I have been arguing for them for some time. As bizarre as it is likely to sound to most philosophers, I have argued that species are not classes of similar organisms but spatiotemporally localized individuals (historical entities). Organisms are not members of their species but part of them (Hull 1974, 1975, 1976b, 1978). I have also attempted to extend this same analysis to include all entities which can be legitimately said to "evolve," including scientific theories (Hull 1975, 1978). As attractive as this view may or may not be, by no stretch of linguistic conventions can Goosens claim that this is the logical empiricist view of scientific theories. To repeat, my objections to theory reduction were directed *explicitly* against the

views of Hempel, Nagel and Schaffner, *not* Lakatos (1970), Toulmin (1972) and Laudan (1977). Goosens has succeeded in defending "reduction in its modern classical, logical-empirical form" (p. 75) only by abandoning it. Goosens might well reply that philosophical development, like scientific development, is best analyzed in terms of research programs which have built into them permissible avenues of change. "The crucial question in all cases is whether the alteration was pre-formulated as a possible development of the theory" (p. 78). Lakatos, Toulmin and Laudan are merely latter-day logical empiricists. Toulmin and Laudan are free to respond to this implication of Goosens' view of philosophical development as they see fit. One only wishes that Lakatos were still around to enjoy this final irony.

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