

## SCIENTIFIC REDUCTION: THE DIALECTIC

The classical Positivists, such as Nagel, thought of reduction as a special case of D-N explanation: One strictly deduces the laws and other principles of the reduced theory T1 from the more general and fundamental laws and principles of the reducing theory T2, with the aid of well-confirmed “bridge laws,” “correspondence rules” or “coordinating definitions” connecting the special vocabulary of T1 to the vocabulary of T2. Thus in the D-N sense, T2 would explain (the truth of) T1, and the reduction would also show that T1 is in a way superfluous: Anything explained by T1 would in due course be explained in more general and fundamental terms by T2. Intellectual economy is achieved, and the unity of science is furthered. If every “special science” could eventually be reduced to physics, perhaps via a series of intervening reductions and the transitivity of D-N explanation, the unity of science would be complete. Nagel mentions (pp. 360-61) a few factors that, when present, make a reduction particularly worthwhile or desirable.

(Note Nagel’s indecision on pp. 356-57 regarding the logical status of the correspondence rules: Are they empirical generalizations, or are they “coordinating definitions” in the sense of stipulative redefinitions of the special terms of the reduced theory? Nagel is satisfied to conclude that that depends on what pair of axiomatizations one prefers in context. This relativity of logical status should embarrass him, and it is an instance of a more general drawback of the Positivist conception of theoretical terms, but let us pass it by for now.)

There are two basic sorts of objection to Nagel’s Positivist account. The first is that raised by Kemeny and Oppenheim and then by Schaffner: that few if any actual cases of scientific reduction have ever satisfied Nagel’s conditions of strict deducibility. The second, philosophically more interesting objection is actually suggested by the last four pages of our snippet from Nagel and is pursued by Fodor. In this handout I shall concentrate on the second after quickly summing up the first.

Schaffner’s general point is that reduced theories do not survive intact. In order for logical derivations to be carried out, various auxiliary idealizations must be made; certain theses of the reduced theory must be modified or abandoned; terms undergo visible meaning changes; etc. Thus the theory that actually gets reduced in Nagel’s sense is not the original theory but at best an “analogous” relative. This seems clearly true, so what are we to make of it? (i) Kemeny and

Oppenheim offer a very weak reduction paradigm: they require only that the reducing theory explain the observations explained by the reduced theory and that it be at least as “well systematized.” But this does nothing to distinguish reduction from mere theory *replacement*. On Kemeny and Oppenheim’s account, the “reduced” theory and its entities might simply be junked, not in any sense explicated in terms of the reducing theory. (ii) Feyerabend and Kuhn are skeptical of the whole notion of reduction, and doubt that anything interestingly worthy of the name ever occurs. (iii) Schaffner himself acknowledges the kinds of disparities that negatively impress Feyerabend and Kuhn, but tries to build them into a liberalized reduction paradigm and to make up for them by adding his condition of “strong positive analogy,” this last being inevitably left vague. Personally I think Schaffner’s approach is about the best one can hope for, if one is thinking of real science as opposed to “rational reconstruction.” Perhaps more can be said about the nature of “positive analogy” (see, for example, the book of Mary Hesse’s cited by Schaffner).

Our second sort of objection to the Positivist picture of reduction is suggested by Nagel’s surprising remark at the top of p. 363 and almost explicitly raised by him on pp. 364-66. It concerns the relation between reduction in the sense of derivation of laws and the reductive identification of the properties and theoretical objects posited by the reduced theory to the properties and objects of the reducing theory.

Notice first that Nagelian reduction requires a *type correlation* between the reduced theory, or its objects, and the reducing theory or its objects. Each coordinating definition or correspondence rule entails at least that reduced items of T1 occur when and only when the corresponding items of T2 do, and this correspondence is supposed to be lawlike. (The temperature of a volume of gas =  $N^\circ$  when and only when the mean molecular kinetic energy of the volume =  $M$  ergs or whatever.) Indeed, as Fodor points out, the correspondence had better be better than lawlike, or the “reduction” will be compatible with a Cartesian-style dualism of substances as between the reduced and the reducing science (see the top of p. 122). Two worries arise immediately:

(1) Since the correspondence rules are synthetic (unless the reduced terms have simply been redefined by fiat), it is hard to see how they *could* be better than merely lawlike, and it is hard to see how the reduced properties and objects could escape being seen as emergent with respect to the reducing science, i.e., as being distinct from any mere collection of the objects of that science even if nomicly correlated with some such collection. Moreover, since the correspondence rules are essential to the reductive derivation, it is not strictly correct to say that the reducing science is *itself* explaining events and other phenomena characterized in the vernacular of the reduced science. For both reasons, the reductive victory

seems hollow, even if some systematization of theory has been achieved.

(2) Once one leaves stock examples taken from physical chemistry, and looks at higher-level special sciences such as psychology, economics and even traditional biology, one finds that type correlations are *not forthcoming*. The natural-kind terms of the higher-level science simply do not match any particular kind terms of physics or even chemistry. (Fodor gives excellent examples on pp. 124-27.) Thus, no Nagelian reduction could take place. This is true of psychology, for example, and two different sorts of people react differently: tough-minded, scientistically oriented thinkers pretend it isn't true and claim against the evidence that type correlations will eventually be found, while tender-minded, humanistically oriented thinkers infer that the mind after all eludes the clutches of the crass science-worshippers, and perhaps that the mind is itself not physical.

All these questions can be sorted out once we attend to the distinctions very properly drawn by Fodor on pp. 122-23. The most important distinction is between "type"-identity and "token"-identity, and the key thing to grasp is that the latter may obtain in the absence of the former. That is, the types, properties or kinds characteristic of a higher-level science may be realized in multifarious and unpredictable ways by the entities of lower-level sciences on which the higher-level science supervenes. E.g. *money* (a kind that figures centrally in economics) can be made of virtually anything, yet is always made of something; every piece of money--every individual money token--is made of physical stuff, but we can't infer that there are any interesting or discoverable generalizations of the form "x is a piece of money iff..." E. another g., psychological mechanisms are realized even within the human species by a wide variety of different neurological structures; there is no particular neurological state one must be in in order to think that broccoli will kill you or to want a beer or to see a cathode ray tube. Yet again we can't infer that mental events are not every one of them neurophysiological events. (Perhaps it's true that they are not; my point here is only that this doesn't follow from the Nagel-irreducibility of psychology to neurophysiology.)

This shows something very interesting and, I find, spooky about nature itself. Nature is organized in a hierarchy of levels, from the microphysical through the chemical and biological up to the psychological and for that matter the sociological, and these levels are *differently gerrymandered*. In Fodor's terms, they *cross-classify* the same underlying basic stuff (presumably swarms of quarks, leptons and whatever). Indeed, it is this gerrymandering that defines a "level of nature"; a level of nature is determined by the characteristic nexus of kinds and laws appropriate to it.

Why *is* nature organized in this way, instead of more neatly? The world might have been such that all the kinds at all the levels were Nagel-reducible and type-identifiable all the way down. I think there are two answers. The first has to do with natural selection. The second appeals, once natural selection has done its thing, to the relations between the inanimate world and the minds of living creatures. But I leave it to you to speculate.

Let me quickly address the problems listed above, and then make a few closing remarks. Problem (1): The syntheticity of correspondence rules, where there are any correspondence rules, is not a problem, for in the early postPositivist, “scientific realist” period, a theory of synthetic a posteriori identification of properties was worked out that has been with us ever since (it is due originally to U. T. Place, J. J. C. Smart, and Hilary Putnam—see particularly Putnam’s article “On Properties”). On the basis of this view of property identification, the worry about explanation abates somewhat as well, for the identity of the properties is now thought of as being metaphysically necessary albeit synthetic and *a posteriori*. Thus even if we continue to concede that strictly speaking the reducing theory does not *alone* explain the phenomena as characterized in the vocabulary of the reduced theory, at least the additional correspondence rules do not themselves demand further explanation; there could be no further reason *why* lightning is electrical discharge or genes are segments of DNA molecules—they just are, and that’s that. (The Positivists, of course, could not be expected to buy the point when put in terms of “metaphysical necessity.”)

Problem (2): The fact seems to be that Nagelian reductions are simply not to be had once one passes chemistry in an upward journey through the hierarchy of levels of nature. But neither the tough-minded science-worshipper nor the tender-minded humanist should get ontologically upset about this. For once Fodor’s “type”/“token” distinction is in place, we are able to see that Nagel-irreducibility does not entail either a dualism of substances or an objectionable idea of “emergent” properties. There is a sense in which the teleological properties that figure in psychology are “emergent”: they are properties that objects of the respective reducing sciences do not and perhaps cannot have. But emergence of this sort is harmless so long as the entities of the reduced sciences that have the emergent properties have those properties only in virtue of consisting of collections of the reducing objects arranged in certain distinctive ways. The parts of a ladder are not (usually) ladders, but a ladder is a ladder in virtue of consisting of parts arranged in a certain way. The emergence of mental and teleological properties is merely a consequence of the gerrymandering phenomenon aforementioned.

In closing I would like to say a word or two about Fodor’s remarks on disjunctive laws (pp. 127-129). Basically, he is considering a possible objection to

his skepticism about reduction. The objection goes like this: “You use the fact of what’s called *multiple realizability* to impugn reduction, i.e., the fact that a single type or kind at one level of nature can be realized or instantiated by more than one kind of structure as characterized by the next level(s) down. But in theory we could, for any such single kind, produce a list of all possible lower-level structures that *could* realize that kind, and if so we could formulate a type-reduction after all, albeit a highly disjunctive one. To note that a type is disjunctive is not, or should not be, to deny that it is a type.” (Alex Rosenberg makes a move of this sort regarding biological predicates, in his recent book; he assumes that the number of possible states of the physical universe is finite, in order to keep his disjunctive properties only finitely disjunctive.) It seems, then, that we could have genuine laws, i.e., generalizations having that familiar if mysterious Modal Force, of the form (5) on p. 128. Fodor resists this conclusion because he is disinclined to grant that a wild disjunction of kind predicates at such-and-such a level of nature is itself a kind predicate at that level of nature, even if it corresponds nomicly to a real kind predicate at the next higher level of nature, and he thinks that *laws* and *kind predicates* are closely interdefinable (see the very bottom of p. 128). Thus he proposes to deny that “It’s a law that ----- ” defines a truth-functional context. — Indeed, more accurately, what he’s really denying is that that operator is closed under deduction, since he denies that “It’s a law that P brings about R” and “It’s a law that Q brings about S” together entail “It’s a law that  $P \vee Q$  brings about  $R \vee S$ .”

Something is wrong here, for on our standard possible-worlds picture of lawlikeness and nomic force, “It’s a law that ----- ” is closed under deduction. If in every nomicly possible world  $P \supset R$ , and in every nomicly possible world  $Q \supset S$ , then by ordinary quantificational and truth-functional reasoning, in every possible world  $(P \vee Q) \supset (R \vee S)$ , QED. Moreover, even if the range of multiple realizability is not finite in this world á la Rosenberg, it is limited by logical space: There will be some definite even though nonfinite number of worlds in which something or other realizes such-and-such a higher-level property, so there will be lawlike generalizations whose antecedents are infinite disjunctions but perfectly definite ones for all that.

Fodor’s point about laws and kinds is nonetheless worth noting. Just because a hideously disjunctive property figures as the antecedent of a lawlike generalization in the sense just described, we do not want to say that that property counts as a natural kind in our own world. So, as Fodor says, “[o]ne...inherits the need for an alternative construal of the notion of a kind” (p. 129). Fodor takes this to be an objection, since he says he doesn’t “know what that alternative would be like.” I am more optimistic. For example, we can distinguish between the vocabularies appropriate to the various levels of nature, and accordingly between lawlike generalizations stated in the vocabulary of one level from generalizations

stated in that of a lower level. Then a kind predicate relative to a level would be one that constituted the antecedent or consequent of a lawlike generalization stated in the vernacular of that level. There may be problems here, but the suggestion is neither mysterious nor unnatural.

I commend to your attention Fodor's two excellent points on pp. 129-31.