Putnam on reductionism

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It is Putnam's view that reductionism (i.e., "the doctrine that the laws of such 'higher level' sciences as psychology and sociology are reducible to the laws of lower-level sciences – biology, chemistry, ultimately to the laws of elementary particle physics"*) is wrong. In this note, I take issue with some of Putnam's arguments.

Putnam begins with the 'logical point' that "from the fact that the behavior of a system can be *deduced* from its description as a system of elementary particles it does not follow that it can be *explained* from that description" (131). That a square peg, a fraction less than 1″ across, goes through a 1″ square hole but not through a 1″ round hole can be deduced *and* explained by appealing to elementary geometry, the rigidity of the peg, and so on. But, even if deducible, it can't be explained by appeal to elementary particle physics, there being a pragmatic constraint on explanation: The relevant features of a situation must be brought out by an explanation and not buried in a mass of irrelevant information (132). So explanation is intransitive and reduction fails (provided, of course, reduction without explanation is an impossibility).

This argument is not as clear as Putnam seems to think. One might cling to the idea that deducibility guarantees explanation and deny that explanation must be *revealing*. Then, only *revealing explanation* would fail of transitivity, not explanation itself. (In this respect, note that Putnam himself has occasion to talk of revealing explanation (133).) A more telling complaint is that reduction need not make for explanation (in Putnam's sense) since derivability, not explainability, guarantees reducibility. (But see below.)

Putnam has a second important line of argument, however, which if correct, undercuts the criticisms just mentioned. As he puts it, the laws of higher-level science (e.g., economics, even biology) are *autonomous* vis-à-vis the laws of physics, in the sense that there is *no deduction* of the former from the latter to be had. The former can only be deduced from the latter in conjunction with " 'auxiliary hypotheses' which are *accidental from the point of view of the lower-level discipline*" (134, Putnam's italics). Whereas, says Putnam, "given the structure of the peg and board, one *can* deduce the rigidity... given the microstructure of the brain and the nervous system, one cannot deduce that *capitalist*

*Putnam, H. (1973) Reduction and the Nature of Psychology", *Cog., 2,* p. 131. All page references in the text are to this article.

production relations exist... [The laws of capitalist society] depend on 'boundary conditions' which are *accidental* from the point of view of physics but essential to the description of a situation as 'capitalism' " (*ibid.*).

To see that something has gone wrong here consider the relationship between Kepler's laws and Newton's laws. In this case, the 'higher-level' discipline (Kepler's laws) is no less autonomous vis-à-vis the 'lower-level' discipline (Newton's laws) than is biology vis-à-vis physics. For the former cannot be deduced from the latter; they can only be deduced (or rather an *approximation* to Kepler's laws can only be deduced – see below) from Newton's laws *plus* auxiliary hypotheses, accidental from the point of view of Newton's theory. (The central mass must be much larger than any of the other masses and all masses must be far apart.) So, provided we agree that Kepler's laws are reducible to Newton's, reduction does not entail lack of autonomy.

A similar point can be made about the peg and the board. Its behavior *cannot* be deduced from the laws of elementary particle physics alone. We also need its description as a certain (accidental) configuration of elementary particles.

But one might insist (cf. Putnam's remarks on p. 134) that, whereas *rigidity* can be deduced from *microstructure, capitalistic production relations* can't be deduced from the *microstructure of the brain and the nervous system.* In reply, note that even if the reported difference between pegs and boards and captialism is genuine, failure of reduction doesn't follow. To see why consider a container of gas. The pressure of the gas *cannot* be deduced from its microstructure alone. In addition, we need facts about the container – boundary conditions in the literal sense – which are accidental from the point of view of statistical mechanics. But if we allow that thermodynamics is reducible to statistical mechanics why should we make reduction of the social sciences to the physical sciences more difficult? In the case of economic systems, why should we bar appeal to boundary conditions (e.g. facts about profits and the like) which are accidental from the point of view of the physical sciences?

Next note that according to Putnam (134) reductionism gives rise to and reinforces the bad idea that human nature is unchanging. For, he contends, if reductionism is true, the laws of psychology are deducible (via the laws of biology) from the laws of physics, which are unchanging.* So the laws of psychology are unchanging, which is to say that human nature doesn't change. But it is not at all obvious – not to some scientists and philosophers, at least – that the laws of physics are unchanging. More important: Even if the laws of psychology do not change, it does not follow that human nature is unchanging. *The boundary conditions may change.* Certainly, the laws of physics have not changed noticeably in the last half century but the nature of, e.g., radios has: They don't distort so much, they are less noisy, frequency response has improved, and so on.

*Putnam also allows (unchanging) reductive definitions in these derivations (134). But this does not affect the point to be made.

Methodological individualism is another bad idea which Putnam believes is nurtured by reductionism. But this is far from evident. Even within physics, reduction of one theory to another frequently *fails* to bring about theory replacement. (A vast amount of contemporary work within physics makes use of Newtonian mechanics, a theory long ago reduced to something better.) Furthermore, reduction need not precipitate neglect of the subject matter or the methods of higher-level disciplines. Indeed, even the most thorough-going reductionists may consistently advocate that disproportionately large efforts be expended on higher-level disciplines. Reduction is not the only nor even a major aim of science; to effect a reduction one needs something reasonably well worked out to reduce.

Next, some issues of a more general nature. Whatever else it is, reduction surely involves a *connectability* requirement – the terms of the higher-level theory must be linked in some way with terms of the lower-level theory – and a *consequence* requirement – the laws of the higher-level theory must be linked in some way with laws of the lower-level theory.* But to say this is not to say very much. Can we say more?

First, connectability. This would seem to hold only if (i) there is a one-many (not necessarily a one-one) correspondence between higher-level and lower-level structures and (ii) for every open sentence, ϕx, of the higher-level discipline there is an open sentence, ψx, of the lower-level discipline *such that* ϕx is satisfied by a higher-level structure whenever ψx is satisfied by one of the lower-level structures that clause (i) associates with the higher-level structure.

Now, given this account of connectability, reduction can fail in a number of ways. For instance one might argue that there are no open sentences of physics which correspond appropriately to open sentences of psychology. Indeed, it has been held that the only appropriate correspondences are between psychology and even higher-level disciplines. (Consider the claim that psychological fact depends on institutional or sociological fact.) But such arguments are difficult to sustain, it always being open to the reductionist to claim that the brain can *internally represent* psychological and even sociological facts. (As far as I am aware, what evidence there is rules more in favor of internal representation of this sort than against it.)

More interesting is the possibility of there being a many-one correspondence between higher-level and lower-level structures. Putnam seems to hold that such correspondences exist. (Capitalistic and socialistic economic orders, he seems to hold, are both compatible with the same psychological order.) And Hull has suggested on the basis of a detailed study of reduction in genetics that "the same molecular mechanism can produce different phenotypic effects..., (that) the *same* molecular situation can result in phenomena which would have to be characterized by different Mendelian predicate terms."** But, if things

*In Mechanistic Explanation and Organismic Biology, *Phil. Phenomenol. Res., 1,* 1951, E. Nagel mentions two similar conditions: definability and derivability. My conditions are designed to allow for non-definability and non-derivability. (But note that Nagel's definability condition includes connection by empirical biconditionals.)

**Hull, D. L. (1972) Reduction in Genetics – Biology or Philosophy, *Phil. Science, 39,* 497–498.

like this can happen in biology it is not unreasonable to think that they can happen in psychology too, However, once again, matters are not so straightforward as they appear at first sight. The one-many relation may be eliminable by introducing additional factors. For instance, in the biological case this can be done by 'expanding' the notion of a molecular mechanism.* But, if this is so, the reductionist can retort: Why shouldn't we expect something similar to happen in the case of psychology? Surely, what sort of structures are appropriate for correlation is an empirical matter, not something to be decided *a priori.* In short, it is by no means obvious that connectability, *construed reasonably,* fails to hold between the social sciences and physics.

Earlier I spoke of the consequence requirement as one of *deducibility*. But, as is well-known, certain incompatible pairs of theories seem (intuitively) reducible one to the other. For example, some of the consequences of Newton's theory conflict with consequences of Einstein's theory. Whence, it follows that we must forgo deducibility or relinquish our intuitions.

But lack of deducibility is hardly a strike against reductionism. For there seem to be a number of plausible candidates for the consequence constraint other than deducibility. One is that the consequences of the higher-level theory agree *approximately* with those of the lower-level theory.** Another is that the lower-level theory correct the higher-level theory and explain why it works to the extent that it does.*** What needs to be shown (and as far as I am aware what has not been shown) is that the consequence relation, construed reasonably, – e.g., in the one of the ways just mentioned – fails to hold between the social sciences and physics.

One might object that there is an important disanalogy between intra-physics reductions and reductions between physics and higher-level disciplines. Putnam expresses it this way (138): Whereas it *is* the business of physics to account for inaccuracies in physics, accounting for inaccuracies in the social sciences *may* or *may not* be the business of the social sciences. But surely there are 'levels' within physics just as much as there are levels within science? It is (in part) the business of microphysics to account for inaccuracies in macrophysics. (Consider, e.g., the relation between statistical mechanics and thermodynamics.) It is (in part) the business of relativistic physics to account for inaccuracies in non-relativistic physics. And so on.

*Cf. *ibid.* p. 498. Another example is this. The pressure on a region depends not merely on the molecular motion of particles in or near it but on the entire molecular milieu. So, reduction would fail were we to connect pressure with molecular motion in the vicinity. However, all this need show is that our chosen connections are at fault, not that reduction is not to be had.

**Cf. H. Putnam (1965) How Not to Talk about Meaning. In Cohen and Wartofsky (eds.) *Boston Studies in the Philosophy of Science,* II, Reidel. In this paper, Putnam makes some suggestions concerning what counts as an *approximation.*

***W. Sellars (1961) in The language of theories, Feigl and Maxwell (eds.) *Current Issues in the Philosophy of Science.* Holt, Rinehart and Winston, discusses a view similar to this. Note that I don't intend to suggest that these two approaches are or need be distinct.

Putting all this together we get the following moral: One should not expect to convince a reductionist that he is wrong by showing that a reduction paradigm fails to apply vis-à-vis the social sciences and physics when it also fails to apply to clear cases of reduction within physics itself.

In this note, I have not commented on Putnam's claims concerning intelligence testing. These seem to be both cogent and important. However, I believe they can be and should be launched and developed independently of any considerations concerning reductionism. They can be because reductionism is compatible with human natures which change; they should be because an appeal to anti-reductionism results in a loss of generality.