Committee I
Unity of the Sciences

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# REDUCTION(ISM) IS A RED HERRING IN THE PROBLEM OF INTER-THEORETIC RELATIONS

by

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**Discussion Paper**

on

**Noretta Koertge's**

**THE PROBLEM OF INTER-THEORETIC RELATIONS: A METHODOLOGICAL INTRODUCTION**

The Thirteenth International Conference on the Unity of the Sciences
Washington, D.C. September 2-5, 1984

Like Koertge (and, I suspect, most members of Committee I) I am bored by the topic of reduction. As she asserts, there has never been a single instance of the successful reduction of one theory and its domain to another--all candidates for the title turn, upon close examination, out to be cases of theoretical <u>explanation</u> (of one theory by another) and <u>enrichment</u> (of both the secondary or reduced domain and the primary one). Thus I propose that it is much more fruitful to ignore reductionism (which is, after all, a desideratum only from the standpoint of a metatheory of science which requires a unified ontology, methodology and epistemology) as a genuine problem for philosophy. Instead, we should focus upon genuine problems in the domain of "theoretical relations"--such as attempting to understand what is involved when a theory <u>explains</u> something (either a data domain or another theory), what constraints epistemology places upon ontological speculation, the problems posed by complex phenomena, what constraints are imposed by our methodology, and so on. I am well aware that the ghosts (or descendants) of logical positivism are very much alive (witness Committee II), but little is to be gained by rehashing the excellent "classic" arguments against reductionism provided by, e.g., Feyerabend (1962), Kuhn (1970), Radnitzky (1970), among others (after all, the positivists still have not read or understood them). Instead, it is time to redirect attention toward the real problems of how theories are related, and how they succeed one another. If we follow this latter course reduction(ism) will be seen correctly: As an empirical issue concerning the relation of particular theories and domains, not as a methodological or metatheoretical issue.

Let me list several points that will require much thought if we are to understand inter-theoretic relations in sufficient detail that we can put reduction(ism) in its place. In a restricted commentary such as this one must realize that my remarks are bald (and perhaps, bold) statements designed to provoke discussion rather than the full fledged defenses that my claims ultimately require.

Explanation isn't reduction, and neither one is deduction. The first part of this claim is familiar enough that it requires almost no comment: To explain something means to offer a theory as to why the phenomenon must be what it is, not to reduce it to something else (least of all to something that is "familiar"). But the second contention is less familiar, and is disturbing to Popperian sensibilities. The Positivistic equation was that explanation equals reduction which equals deduction. Against this tripartite identification Popperians (and others) argued that to explain is not to reduce. But Popper (who, with Hempel, is the father figure for the "covering law" model of explanation) identifies explanation with deduction. In this sense Popperians are unwitting allies of Positivists and other reductionists, because the reductionists argue (plausibly, I believe) that "If explanation is deduction, and if a reduction explains, then there is no difference between us except Popper's aversion to the word reduction."

Against both Popperians and positivists I wish to deny all three identifications: Reduction is not explanation (except in the illegitimate sense of "explaining away"), and explanation cannot be deduction (even though the only logic is deductive). Explanation is a matter of argumentative claiming: Theories assert that their models of reality are true. Explanations are argumentative models of the structural properties of a domain. It is to the domain of pragmatics and the province of rhetoric (rather than to syntactic structuring and logic) that we must turn in order to understand explanation. When we do so, when we look at the nature and function of explanatory reasoning in actual scientific activity, it is clear that no explanation is "nothing but" logical deduction.

This negative thesis has been argued persuasively by Fries (1828), Körner (1966) and Feyerabend (1962). All have pointed out that empirical reality is never the last step in the "deductive unification of experience" (Körner, Fries), and that all genuine explanations change the meanings of the terms involved, and thus commit the equivalent of the logical fallacy of four terms (Feyerabend). Perhaps this is why Popper has been forced to remove explanation from actual science and restrict it to a matter of after the fact reconstruction in World 3. But even if that retreat to a purely logical realm is made, one cannot retain the equation of explanation=deduction without saying that there are no explanations in domains of essential complexity (as I have used the term: See Weimer, in press). This is because such domains range over infinite particularity, and the theories thereof cannot "deduce" infinite empirical particulars (Körner shows that no theory deduces <u>any</u> empirical particular, a different but equally devastating argument). Such realms employ what Hayek (1967) called explanation of the principle (rather than explanation of the particular, which is all that the "covering law" account can acknowledge). Such principles are part of an explanatory structure that is rhetorical and argumentative, but they cannot be deduced from it (although a part of it) nor do they allow for the deduction of particular events.

(Another line of reasoning that should be added here is Bartley's (1982) thesis that criticism [and hence the property of criticizability for a theory] need not pass thru deducibility, that it need not employ what he called the [logical] transmissability assumption. I mention this only to shock Popperians, since it constitutes an existence proof that one indispensible property for scientific theories need not involve logic. It puts Them in a more receptive frame of mind when I ask them to entertain the far more heretical thesis that explanation is not and cannot be just deduction.)

If we admit that explanation is argumentative and rhetorical rather than logical we sweep one more prop out from under the diehard reductionist, and at the same time open up for critical scrutiny the previously closed problem of what constitutes an explanation in actual practice. I think both consequences are quite desirable.

Epistemology constrains ontology. Put another way, what we think we know about how we know what we know forces us to be ontological agnostics and epistemic dualists. Reductionism is incompatible with epistemology, and cannot be "known" in ontology. This thesis can be developed by straightforward examination of human knowledge and experience.

How we know is a matter for cognitive and neuropsychology: At issue is how knowledge results from the structuring and restructuring of patterns of neural activity. But what do we know? Here dualism is inescapable: On one hand we have phenomenal experience or acquaintance (in Russell's [1912] sense), on the other hand we have knowledge by description, discursive knowledge, of the nonmental realm with which we are not directly acquainted. Acquaintance is not and cannot be description, and vice versa (despite the fact that we can sometimes know by acquaintance the referential basis of a description, and can describe experience with which we are acquainted. Although knowledge by description and acquaintance may sometimes have the same reference, they can never have the same sense. As Russell (1948) and Maxwell (1972) have argued, our only knowledge of the nonmental realm, including our own bodies, is purely by description of its structural properties. The only intrinsic or non-structural properties of objects which we can know (if indeed we know any at all) are those of the events in our brains comprising our phenomenal experience. Knowledge by description, whether common sensical or scientific, always transcends experience. Thus arises the mind-body problem of sentience: We do not know the intrinsic properties of any nonmental entity, and therefore we cannot say whether the "mental" is or is not intrinsically identical to the "physical."

This puts the cluster of mind-body problems that traditionally relate to reductionism in a new, and inherently problematic, light. What we know about the human epistemic predicament, as disclosed by the inescapability of a sharp acquaintance-description distinction, forces us to be dualists (I presume no one today is foolish enough to deny that human acquaintance is real, and that solipsistic Bishop Berkeleys who deny the realm of description are known to be acting inconsistently when they tell us anything of the sort). This leads to ontological agnosticism: Any claim that the "mental" is or is not the same stuff as the "physical" must rest on conceptual arguments alone, for no contingent knowledge claim can address the issue. No scientific theory (contingent and empirical) can ever tell us whether the intrinsic properties of any object are the same as those with which we are acquainted. The mere existence of acquaintance, the "raw feels" of human experience, forever precludes the tenability of any physicalistic monism. Likewise, the mere existence of knowledge by description precludes idealistic monism. Reduction of one to the other can never be known to be tenable.

Problems posed by complex phenomena. The thesis that all science should be conducted upon the model of classical physics, and/or produce results of the same form, is widely recognized to be scientism. What we find when we examine domains of essential complexity is that there are compelling arguments for why complex phenomena can never be understood upon the simple science model. Indeed, the type of "progress" that is often lamented for its absence in the social sciences by the physics-is-the-genuine-science advocates can never be achieved. This is not because the moral sciences are "immature" or not yet scientific, but because science must take on a fundamentally different form when one deals with essential complexity. For a fuller presentation of the arguments involved I must refer to other sources (Hayek, 1967; Weimer, 1980, 1982, in press). But the import for reductionism of such arguments is obvious: The quest for reduction, to either the ontology or methodology of even an ideal physics, is chimerical.

Reductionism has become a "haunted universe" doctrine. While it may have had a testable and refutable status when initially introduced, reduction(ism) has become something quite different for later day positivists and their descendants. In the terminology of Watkins (1958) it has become a confirmable and influential metaphysical thesis that is immune to falsification and hence is not empirically criticizable. Consider the proposition that a castle on a hill is haunted. There is "evidence" that is compatible with this (e.g., hearing what one assumes to be moans or chains rattling at midnight) that an advocate will take to be confirmation of the thesis. Likewise, such "partial success" at reduction is evidence in its favor to the metaphysical Reductionist. But there is no event that can disconfirm or falsify the proposition that the castle is haunted. Even if we listen for 1000 years and hear no moans or chains it cannot falsify the hypothesis that the castle is indeed haunted. Such theories Watkins called confirmable and influential (influential because if you believe that the castle is haunted you will alter your behavior in its presence--especially at midnight during a storm) even though they can never be falsified. Reductionism has ceased to be testable and become a haunted universe doctrine. Its proponents are searching for ghosts.

How can one criticize a haunted universe metaphysical doctrine if it is not falsifiable? One way is by opposing it with another docrine and adducing arguments in its favor, preferably simultaneously showing that the doctrine to be discredited is inelegant, clashes with other well established (not: justified) knowledge, etc. (see Bartley, 1982; Popper, 1983). The next section provides one more opposing argument that makes reductionism pale (quite ghostly) by comparison.

Do not block the division of labor and knowledge. All useful methodological rules are negative constraints that tell us what mistakes to avoid rather than what particulars must be achieved (Weimer, in press). Reduction(ism) violates this, because it (scientistically) tells us what particular result we must achieve: Theoretical or ontological unity. Against the reductionist goal is the brute fact of the superior power of specialization in a catallactic order. The division of labor that creates disciplinary divisions can produce far more than a single discipline (or

9

individual). The argument for the superiority of the division of knowledge is a straightforward extension of the situation for labor, which was clear to Ricardo one hundred and fifty years ago in his law of comparative cost or association (see Mises, 1949).

As Mises (1949) put it,

> The increase in productivity brought about by the division of labor is obvious whenever the inequality of the participants is such that every individual or every piece of land is superior at least in one regard to the other individuals or pieces of land concerned. If $A$ is fit to produce in 1 unit of time 6 $p$ or 4 $q$ and $B$ only 2 $p$, but 8 $q$, they both, when working in isolation, will produce together 4 $p$ + 6 $q$; when working under the division of labor, each of them producing only that commodity in whose production he is more efficient than his partner, they will produce 6 $p$ + 8 $q$ (p. 158).

When we produce the commodity of scientific knowledge the result is the same: Superior productivity with individual (disciplinary) specialization. Thus over a period of time, in trial and error fashion the division of labor will displace isolated labor when the two compete. With this division we also achieve the division of knowledge that has produced science, the arts and humanities, and technology: Indeed, everything that transcends the capacity of a single individual.

The Ricardian law of association is a fail safe mechanism that insures that we can continue to evolve and grow, even though no single individual (or plan, or theory, or discipline) can equal the productive capacity of the spontaneous order or predict its course. Adherence to reductionism is equivalent to advocating central planning or a single locus of control (allocation) for the market order: Both programs attempt to restrict the output of a complex spontaneous order (either science or society) to a single plan thought out in advance, and both attempts fail to allow for novelty and the unforeseen--for the unanticipated growth of knowledge.

In contrast, the lesson to be learned from the division of labor is that specialization--in individuals, disciplines, and resultant knowledge--will be never ending so long as we do not restrict the power of the catallactic order. Thus if we attempt to adhere to C. S. Peirce's supreme maxim, Do not block the way of inquiry, we must not block specialization in the production of knowledge. Reductionism, far from advancing scientific understanding, is in fact an impediment to its achievement. (In this respect it is exactly analogous to the "scientific" socialism that positivistic advocates naively believed in--an attempt to force the abstract order of society [or science] to conform to the simple, superficially rational and desirable principles of operation that can work only in the face to face or tribal society that the civilized world abandoned thousands of years ago.)

In defense of the strategy of reduction. Since I have been quite critical of reductionism it is worth brief mention to note that the strategy of attempting to reduce (e.g., the program of materialism in psychology) is often a very useful strategy for research. There is no paradox here at all: Reductionist strategy can be used to sharpen the points of agreement and disagreement between competing theories and research programs. (As a dualist I do indeed welcome computer based A. I. research, as Koertge's example indicates, because it gives me a sharper target to refute.)
In this sense trying to reduce is an excellent way to make scientific progress, because it is likely to uncover errors very quickly. On this point I am an

unrepentant Popperian: Learning is a matter of weeding out error, and I am in favor of all strategies that permit us to learn from our mistakes.

But this strategy works only in periods of what Kuhn (1970) called normal science, when there is a shared framework that permits puzzle articulation and solution to occur. At some point a "revolution" will occur, and then the strategy is as likely to hinder as to help. Indeed it sometimes occurs that a new discipline will be created by following a reductive strategy (witness sociobiology, which attempted to reduce sociology to a "firm" biological basis). Then the revolutionary figures that created the new domain will retard progress if they push reductionism too hard (as Wilson appears to be doing).

In any event, however, one cannot blindly advocate the strategy. When normal science breaks down it becomes impossible to say when an anomaly will be "reduced" or instead become the basis for a revolutionary new theory. All strategies are fallible. Nonetheless reductionistic programs can indeed aid the growth of knowledge.

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