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BETWEEN REDUCTIONISM AND HOLISM:
COMMENTS ON LEINFELLNER

by

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Discussion Paper on

Werner Leinfellner's

REDUCTIONISM IN BIOLOGY AND IN THE
SOCIAL SCIENCES

Many scientists and philosophers of science see the programme of reductionism as the sole blueprint for scientific progress, and for a unified scientific understanding of nature. From this viewpoint, various versions of anti-reductionism (e.g., emergentism) are simply so much rubbish lying in the path of science; and one particularly smelly rubbish of this type is holism - the view that an organised whole is not only greater than the (additive) sum of its parts (which is rather a triviality), but also that the behaviour of the parts is causally determined by the 'transcendent' whole, which follows its own sui generis "laws of motion" (so to speak). Committed holists, on the other hand, have often viewed reductionism as a dogmatic (or a priori) **eliminative** materialism which is unable to see the obvious facts of existence - namely, that there is much more between heaven and earth than is dreamt of in its philosophy. Reductionists, of course, would reply (with Nelson Goodman) that, unlike holists, their worry is that there should not be more things dreamt of in our philosophy than there are between heaven and earth; and that it is only by following a programme of reduction that we can clear the ontological slum which holists are apt to create.

If I understand Leinfellner's paper at all - and I must admit to having had considerable difficulties here - his aim is somehow to unite these two anti-thetical positions by modifying each of them. Thus, for him, the truth-content of reductionism lies in the claim that certain structural similarities can be found between higher level "wholes" (like markets, group utility, or living cells) and their lower level "parts" (like psychological preferences, individual utilities, or thermodynamic systems); while reductionism's falsity-content lies in its claims that the higher levels can be eliminated in favour of the lower levels, and that the laws or behaviour holding at the higher level can be fully explained in terms of, or reduced to, those applicable at the lower levels. On the other hand, the truth-content of holism seems to reside in its realisation that organised wholes are not merely the (additive) sum of their isolatable parts and that the whole can indeed causally influence the behavioural patterns of its parts; while its falsity-content consists in its assumption that the theories (or models) required for understanding holistic systems are totally sui generis,

and that the laws relating to wholes may even conflict with, or contradict, or override the laws governing its parts.

Thus, if I have got him right, Leinfellner hopes to develop a methodological approach to explanation which is between those of traditional reductionism and traditional holism, and which incorporates the virtues of both while avoiding their pitfalls. As Leinfellner himself puts it (p.7), "instead of reducing levels we bridge the gap between upper level holistic systems and its lower level parts (subsystems) by searching for similarities" and building up "a network of theories or models". Rather than attempting to achieve a uni-directional reduction of wholes to parts (traditional reductionism) or of parts to wholes (traditional holism), our aim changes to one of achieving "a 'mutual' reduction, i.e., to a structural comparison between different levels, with the goal being to bridge the gap between them by showing how lower level structures are integrated in, and correlated with higher, more complex upper level structures" (p.5). In this way we avoid the eliminative consequences of reductionism without thereby embracing the ontological excesses of traditional holism.

What, then, is Leinfellner's new method (or model) of explanation?; his new way of understanding the relations between higher level systems and their lower level subsystems? Here I must admit to being rather clearer as to what his new method or model is not than I am as to exactly what it is (more on this later). One thing which his model of explanation is not, is that it is not deductive. In searching for structural similarities (or even structural identities) between higher and lower level systems we are not attempting to deductively explain the structural properties of one in terms of the structural properties of the other - for to do this would be to land us back in one of the rejected uni-directional forms of explanation (note that, in general, deductive explanations are not reversible - one will rarely be able to turn the explanation "upside-down" and deductively explain the explanans in terms of the explananda). But in what sense, then, is the search for, or discovery of, structural similarities or identities explanatory at all? What exactly does the discovery of structural similarities explain? To take a silly example: chess and checkers undoubtedly

share certain "structural similarities" in that both are two-person board games played by moving pieces in accordance with fixed rules. These shared properties are even, to use Leinfellner's oft used phrase, "salient and robust" aspects of each game - if, that is, it is raining outside and one seeks to pass a quiet afternoon with a less than close second cousin. Yet I haven't the foggiest idea what the discovery of such "structural similarities" by itself explains. Of course, statements of structural similarities may play a role as part of an explanation (for example, an explanation of why, in the circumstances described, chess and checkers may be inter-substitutible); but as part of an explanation such statements are, I maintain, part of a deductive explanation. So although Leinfellner claims that his "structural comparisons which replace fully what has been called reduction" retain "the explanatory function of the former reduction" (p.7) I am less than convinced of this.

However, much more intriguing than the question of what Leinfellner's new method or model of explanation is not, is the question of what it is. Here, again, I have had considerable difficulties in understanding Leinfellner's paper, but it seems to me that his proposed method might be dubbed 'the method of structural modification and comparison'. The basic idea appears to be something like this: take two theories (models, sets of assertions) describing what are taken to be two "levels" of objects, processes, etc. where the domains of application of the theories at least partially overlap (i.e., they treat of some shared referents). Then the method of structural modification and comparison consists in attempting to "match" (or, perhaps, "map") partial aspects of each theory, or model, with (or into) the other. This will usually involve first modifying either, or both, of the initial theories so as to produce the potential for "matching". This modification process seems to be symbolised by '$\approx$' (i.e., $M_1 \approx M_1^*$ seems to mean that M_1 has been modified to M_1^* by either restricting M_1 by adding certain conditions to it, or expanding M_1 by adding some new element not previously found in it, or both). Next, having modified M_1 to M_1^*, or M_2 to M_2^*, or both, we compare them for partial structural similarity or identity (this similarity or matching relation being symbolised by '$\rightleftharpoons$'). Then, if $M_1^* \rightleftharpoons M_2$, say, we have somehow

bridged the gap, or at least partially done so, between the two "levels" described in our original theories, and shown that some "salient and robust" structural property of M_2 can be matched (mimicked?; reproduced?) at the "level" of M_1. Thus we have brought the two "levels" into some kind of "partial structural harmony" without reducing one to the other, or vice versa; and we have thus achieved a kind of unifying integration of the two "levels" without reduction.

Now, following Agassi, I do not mind admitting that I do not understand what I say when I say something which I do not understand. My problem here is not so much understanding the process of modification symbolised by '$\approx$' (although I do not see what constraints Leinfellner puts on it) as it is understanding what the matching or structural similarity relation '$\rightleftharpoons$' consists in. To see my problem, consider two systems S_1 and S_2 which completely lack any structural similarities, so that presumably $S_1 \not\rightleftharpoons S_2$. It immediately follows that S_1 and S_2 share at least one rather "salient and robust" (from Leinfellner's viewpoint) structural similarity: namely, the structural similarity of bearing $\not\rightleftharpoons$ to each other! To put this point another way, any two things must be similar in some respect. For assume that they are dissimilar in all respects; then they are similar to each other in the respect of being dissimilar to each other in all respects. Thus complete dissimilarity is impossible, and so the general notion of similarity is empty. For similarity to bite we must be supplied with some point of view, or problem, or interest from which to judge the question. For example, is ⊡ similar to ⊙? If our point of view or interest is that of looking for figures with dots inside them, then they are similar; but if we are looking for circles, then they are not similar. There is no point in seeking "structural similarities" between theories or models describing different "levels" unless we are supplied with some problem, or point of view, or interest - for in their absence we will always be able to find some "structural similarities" between them and thus will be able, by Leinfellner's method, to integrate anything with anything. It follows that 'success' in employing Leinfellner's method is in itself of little interest. Much better, it seems to me, to investigate the question of

the relation between different theories, or between different "levels", by trying to see if a rather hard-headed, and constraining, reductive explanation can be achieved with respect to them. This attempt, whether it leads to success or failure, can at least lead to failure (unlike Leinfellner's method), and so offers us the opportunity to learn something (since, following Popper, we learn from failure).

These last points relate back to my problem with Leinfellner's process of modification '$\approx$' - that is, the apparent lack of any constraints which he imposes on it. Some of his examples of the successful use of his method have rather the "feel" of a kind of hocus-pocus, in which one simply looks at (say) M_1; picks some "salient and robust" property P_1 out of it; maps some plausible M_2 analogue to P_1 into M_2 (thus getting $M_2 \approx M_2^*$); and then - hey presto - $M_1 \rightleftharpoons M_2^*$! But <u>of course</u> M_2^* will "integrate" with, or bear a striking structural similarity to, M_1 - since it was generated just to do so. So unless some constraints are imposed on the allowable means of modification, the achievement of "Leinfellnerian integration" looks not only once again much too easy but moreover bears a striking structural similarity to arbitrariness (i.e., it looks singularly arbitrary).

Compare this, however, with the situation which pertains when we seek a reductive explanation of one theory or "level" in terms of another. Consider, for example, the attempt to reduce phenomenological thermo-dynamics to statistical mechanics. Because of the 'tightness' of reductive explanation, this attempt quickly ran into severe difficulties - for instance, the fact that thermo-dynamics included irreversible phenomena whereas the known laws of mechanics were completely reversible. How could irreversible thermo-dynamic phenomena be explained (deductively) from reversible mechanical laws? The answer was: they couldn't be. Rather, a completely new physical quantity - entropy - had to be introduced into the "mechanical" premisses in order to get the explanation, and so the reduction, to go through. Because of the constraining nature of what was being attempted, the introduction of entropy (and so, in effect, the modification of mechanics) was not arbitrary in the least: it seemed to be required if the problem was to be solved.

Obviously there are many points in Leinfellner's paper which I cannot discuss in a brief comment such as this - for instance, some of the particular examples which he puts forward are worthy of considerable discussion in themselves. So let me end by saying that although I have here championed the search for reductive explanation, in preference to "Leinfellnerian integration", this should not be taken to mean that I am a reductionist, or that I think the programme of reduction has any sort of privileged methodological status in science as compared to what I have elsewhere called the programme of emergence (cf. my "Three Views Concerning the Unity of Science" in the Proceedings of the XIIth ICUS). Rather I champion the search for reductive explanations because (a) such searches can fail; (b) I hope that they will fail, and thus allow emergentism to pass some severe tests; and (c) even when they succeed they will generally do so only by forcing us to non-arbitrarity modify, and so improve on, that theoretical knowledge which was available to us prior to the attempt.