

Structural Realism, Again

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I fear I did not express myself as simply as I might have. My objection to Cao is that *something* must be ceded to Kuhn. One can of course try to oppose Kuhn's thesis root and branch, but to do so one had better counter his concrete examples, or one had better present an equally persuasive and wide-ranging history, but to a different effect. Perhaps Cao thinks his book provides just such an alternative, but alas, here a history of 20th century field theories simply doesn't cut any ice: it is a history of *normal* science, in Kuhn's terms, at least the way Cao tells it, devoting no time at all to the development of quantum physics, and hardly any to the discovery of special relativity, the two really revolutionary steps in physics in the last century. True, there remains one other plausible example of revolutionary science, and on this Cao does have something to say: the quantization of gravity. But this revolution is still in the making; one simply doesn't know whether gravity will be accommodated along the lines Cao suggests; one just doesn't know if his "gauge field program", "geometric program", and "quantum field program" will find a happy synthesis.¹

My own suggestion is similar to Worrall's, namely that at the microscopic level we give up on the more traditional, picturesque, common-sense idea of ontology - the sort of story philosophers and historians find so easy to tell - and embrace a more abstract, structural account of thing-hood and object-hood. But I add that we should also accept that the history of dynamics shows a very special character, very different from other branches of physics and other empirical disciplines. We should accept that dynamics is concerned with real structures to physical phenomena, but ones which recur in many different applications, embracing very different "ontologies", in the traditional sense of the term. And since these dynamical phenomena are spread across quantum and classical, relativistic and non-relativistic regimes - from particle physics to thermodynamics and condensed matter physics - here we have a kind of continuity (structural continuity) across domains treated by theories that differ in radical, revolutionary ways. This is a kind of continuity wholly neglected by Kuhnians.

Insofar as Cao considers this positive claim - not much - he objects that "it is not the question as to whether there is progress (...in an instrumental sense), or whether there is something in science that can be retained (....empirical data...) in the development of science; but rather the question of whether there is cognitive progress....in the sense that we can have more and more true knowledge of what exists and happens in the world." (1, p5-6). It is no objection as it

¹Cao is right to upbraid me for suggesting, in two places, that he looked to a *final* synthesis. I spoke in error, but the point is of no consequence to my criticisms. (Replace "complete and final" (p.xx) and "final" (p.xx) by "unifying"; all else stands unchanged.)

stands; the issue is whether or not the sort of structural knowledge of dynamics that I am concerned with is merely instrumental, or merely a summary of empirical data - or whether, as I maintain, it is true knowledge of what happens in the world, and of what exists, in the sense of dynamical structures.

This sort of claim needs to be backed up with concrete examples; “structure” is such a weasel-word. And I don’t think one can do this without presenting the equations, which Cao found so mystifying. Hence my exposition of the Landau-Ginzburg theory of critical phenomena, and of the identity of certain formal expressions for the scaling (anomalous scaling) of correlation lengths in the vicinity of critical points in statistical mechanics with those for running coupling constants for solutions to the renormalization group equation. It was Wilson’s perspective on the renormalization group that really put these correspondences in evidence, and helped to explain them. As he himself has made clear, they depended crucially on an understanding of quantum field theory, in d spacetime dimensions, in Euclidean terms, and the equivalence of this with a statistical mechanical system in d spatial dimensions. Wilson was not the first to remark on this correspondence; it was stressed by Symanzik (1966); without it, not.²

This is one of the most beautiful examples of the mutual influence of disciplines in dynamics, and it is a pity that I had to condense it. For a lengthy presentation, see Peskin and Schroeder (1995, Ch.10, 12, 13). For records of historical note, that present a very similar view of the renormalization group to the one that I have given, see Wilson’s Nobel prize lecture (Wilson 1983), and Fisher’s recent review (Fisher 1998).

Let me turn now to the other philosophical worry that I had with Cao’s book. What has all this to do with the problem of measurement? Here Cao has misunderstood me. My point was that structuralism as I (and French and Ladyman) understand it involves metaphysical questions that are closely tied to a number of approaches to the problem of measurement (Mermin’s, Everett’s, others based on decoherence theory), so that the one cannot be properly addressed and not the other. I made no remark on the status of the problem of measurement on Cao’s more traditional view of ontology, but here I would have thought the problem is obvious: if one is committed to a thesis of continuity

²Concerning Cao’s criticisms of my exposition in (1, fn.5), (a) obviously the general theory of scaling due to Kadanoff and Widom and others was very different from Gell-Mann and Low’s rudimentary theory of running coupling constants, but Cao’s point is historical, whereas mine is conceptual: does Cao really want to deny Wilson’s seminal achievement? It was precisely to show the connections between renormalized perturbation theory in QFT and scaling in condensed matter physics. (b) I have commented on this in the text; see Wilson and Kogut (1974, §9, 10 for a detailed account, based on lattice theory. (c) The fixed points with which I was concerned were the Gaussian fixed point of free-field theory and the Wilson-Fisher fixed point. Of course the latter only exists in QFT in 3 spacetime dimensions, but that is exactly to say that it controls critical behavior in statistical mechanics in 3 spatial dimensions, the realistic case. As for the derivation of asymptotic freedom, it is true that this follows most simply from the Callen-Symanzik equation in renormalized perturbation theory; but once one has available Wilson’s framework, and has identified the β -function with the rate of the renormalization group flow, one can determine its sign using the latter framework as well, notwithstanding that one integrates out the high-frequency components (it is only the sign of the β -function that one needs to deduce the existence of asymptotic-freedom).

of ontology in the traditional sense, robust enough to oppose Kuhn's view on the nature of revolutionary theory change, then one has to reconcile the ontologies of supposedly incommensurable theories - quantum and classical mechanics chief among them. So one has to deal with the problem of measurement on Cao's approach as well, much as he would like to ignore it.

Let me make a remark on Cao's elaboration of his account of "ontological synthesis" (this volume). I am sympathetic to it; I find it plausible; I have elaborated a view very similar to it myself (Saunders 1993), although there I placed great emphasis on the attempt to reconcile classical and quantum theory. What I there called the "heuristic plasticity" of objects, understood in structural terms, is similar to Cao's emphasis on the possibility of change at the level of "core statements" about objects, again when understood in structural terms, whilst yet talking of objects which are recognizably the same. It is a circle that we have both tried to square. The devil is in the detail. In his papers in this volume Cao has given many more details, and many of them I find congenial, but as I have now said repeatedly, he has only drawn support for them from a period of normal science.

Putting this to one side, where do Cao and I differ? I would certainly call myself an ontological structuralist; I believe that objects are structures; I see no reason to suppose that there are ultimate constituents of the world, which are not themselves to be understood in structural terms. So far as I am concerned, it is turtles all the way down. Here, I suppose, is a point of difference, for although Cao agrees that quantum field theories cannot be understood as dynamical particle systems, he also insists that "ingredients" - and here he repeatedly gives the example of electrons - "are ontologically prior to the structure"³. But I share French and Ladyman's puzzlement as to why Cao assumes that I, and they, are wedded to the belief that structures are *merely* mathematical. I have never said so, and nor to my knowledge have they. And my position is entirely neutral with respect to Platonism, as I believe is theirs. Presumably Cao is of the conviction that objects can only turn out to be structures if they are mathematical objects, but he does not give his reasons for believing this, and at times he allows that objects may be physical, and yet have an ontological status subordinate to the structure in which they figure (2, p.16; his example is spacetime points). The question of priority, he says, is to be settled by questions of causal efficacy, but I along with French and Ladyman see causal structure as structure; this is not a point against structuralism. Elsewhere he insists that mathematical structures "cannot deal with qualitative aspects of the world" (2, p.3), and perhaps he is right; but the pertinent question is whether qualitative aspects of the world can be captured in structural terms. There are surely arguments that they may not; the qualitative character of colors, it seems, cannot be captured by talk of wavelengths of light, or of relative spectral reflectancies of surfaces. But Cao does not remark on them.

The one clear difference outstanding between us appears to be this: that Cao

³2, p.16. Oddly, he says the same of nucleons, structured objects *par excellence* from the point of view of QCD.

maintains, whilst I and French and Ladyman deny, that there are objects which are not structures. What hangs on it? According to French and Ladyman, its importance is this: as realists, a metaphysical account of object-hood is owed, an account that is moreover underdetermined by physical theories *per se*. There is a structuralist account, and it is motivated, above all, by the problems that attend more traditional notions of object-hood (objects as “individuals”) that arise in quantum statistical mechanics. Presumably, just because it is metaphysics that is at issue, on their view, the account of object-hood should apply uniformly - so not only to quanta.

Now, I think Cao has some good arguments against this view of French and Ladyman. I share with him a suspicion of the thesis that metaphysics is underdetermined by physics, as championed in particular by French; it would certainly seem to be at odds with the view that metaphysics is continuous with physics, as I and I think Cao believe. Like Cao, I do not find French and Ladyman’s examples convincing. And on two counts I think they are just plain mistaken.

Why then suppose that objects - all physical objects - are structures? My reason is a pragmatic one: the notion of object is clearest in logic, in the structure of the proposition, but the language of physics is mathematics, not the predicate calculus. Physicists also speak about the world; as such knowledge of physics includes propositional knowledge; but not all, and perhaps not even the most important part of what physicists know, can accurately be put into words. We must do our best to say what there is, so there will always be a place for objects, understood as objects of predication; but I see no reason why objects in this sense should precisely line up with the constituents of reality, whatever they are, nor with what can be known of them, given that the primary vehicle for understanding reality is mathematics (interpreted mathematics). It is true that set theory can be formalized in *Begriffsschrift*; I grant that mathematics, or those parts of mathematics of use to physics, can be reduced to set theory; but I do not think that thereby one will learn what physical objects really are.

That said, one expects the traditional notion of object - but here I mean Frege’s notion of object, not the Scholastics’ - will come unstuck in quantum physics. Cao, and French and Ladyman, are all agreed that it is in trouble when it comes to quantum statistics (and by implication that there is no problem in classical statistical mechanics). Cao does not see this as a matter of empirical underdetermination, however, and insofar as particles in quantum mechanics are not individuals in the traditional sense, he sees no great consequence in that. French and Ladyman, keen on preserving the view that even in quantum mechanics particles may be viewed as possessing “transcendental individuality” - meaning that they can be viewed much as particles in classical statistics, save that certain states are dynamically inaccessible (thus accounting for the anomalous statistics) - insist on the underdetermination thesis; and insist that this is a lacuna in our understanding of quantum particles (whether or not they are taken as fundamental) which it is incumbent on the realist to fill.

Here I think are the two mistakes in their reasoning. The first is that they neglect an important sense in which particles in classical statistical mechanics

are just as indistinguishable as are particles (specifically fermions) in quantum statistical mechanics. An exact permutation symmetry applies to them both, with physical consequences: in the classical case, the consequence is that the entropy is an extensive quantity.

This point of view is well known in the literature (see e.g. Hestines 1970).⁴ It does not quite settle the matter as to whether particle indistinguishability is an empirical question, however; Huggett, for one, has insisted that the question of whether or not entropy is extensive is a matter of convention (Huggett 1999). But then I agree with Quine that not only is metaphysics continuous with physics, but that convention, in all but the most trivial cases, is continuous with fact.

Secondly, I think they are mistaken in their view that failing transcendental individuality, the very notion of object-hood is undermined by particle indistinguishability in quantum mechanics (and, if I am right in the foregoing, in classical mechanics). It is true that from exact permutation symmetry it follows that such particles (and, from the foregoing, classical particles) may in certain circumstances not be uniquely identifiable, in the sense that it may not be possible to refer to one member of a collective rather than another. But it does not follow, from logical principles, that such particles cannot be objects of predication. Indeed they can: objects, in Frege's sense, may share exactly the same properties and relations, with all other objects and with each other, and yet logically be counted as numerically distinct - *consistent* with Leibniz's principle of identity of indiscernibles (understood as the principle that questions of numerical identity are to be settled in purely predicative terms).⁵ What does turn out to be true is that elementary bosons may not be counted as objects. Logic, then, separates off gauge particles (and the Higgs boson, if it exists) from the stable constituents of matter, one and all fermions. One might take this to reflect a cleavage in nature, a fact about the fundamental ontology of the world; but that is to continue with the old ways, when logic was philosophy of science enough.

French, Ladyman, and I are agreed that the logical notion of object is inadequate to ontology, according to the structuralist, but they see the problem as more severe than do I, and unlike them, I see no reason to seek for an alternative notion of object-hood. The world is a structure, and it is thought of as such in exact physical, interpreted mathematical terms, but how it is to be broken down into parts, to be spoken of predicatively, can be a more rough and ready affair, sufficient only in the sense of FAPP, to use Bell's acronym; sufficient linguistically, but only for all practical purposes.

References

⁴It is known, in particular, to French, who holds rather that the failure of extensivity can be viewed as a failure of classical physics (French 1986). Against this I say that the identification of isomorphic models – in particular models related by exact symmetries, in this case the permutation symmetry – is a methodology common to quantum and classical physics.

⁵I refer to the Hilbert-Bernays definition of identity, subsequently championed by Quine. For its applications in physics, see my (2001).

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