

tit (1993) provides the most systematic, though very cautious, investigation of these ideas.

14. According to Menzies (1988) this line of argument was suggested by Lewis.

15. That is, a property possessed by an object (such as dormativity) in virtue of its having some more basic (e.g., chemical) properties.

16. We are especially indebted to Ponce's treatment here.

17. Clapp (2001) successfully argues that some leading defenses of the autonomy of special sciences, such as Fodor's (1974), are guilty of this lapse of metaphysical seriousness. We should therefore note explicitly that none of our arguments in this paper depend on the idea that the kinds of any special science must be preserved as kinds just *because* people find it useful to think with the concepts they represent. Indeed, on one interpretation this is what "taking metaphysics seriously" in our sense here *means*.

18. Ross finds it necessary to make Dennett's idea more systematic because Dennett's own account, as Ross explains, leaves too many doors open to emergentist and, in other places, instrumentalistic, readings. On the other hand, Dennett's paper surpasses Ross' in anticipation of a theme to which we will shortly be devoting much attention: the relationship between reductionism and a scientifically unsophisticated understanding of causation. See, especially, Dennett's footnote 11, and compare this with sections 4 and 5 of the present paper.

19. Stich (1983) devoted a book to arguing that this sort of picture, intended as a way of reconciling a plausible cognitive scientific typology of states with folk psychology, could not work. Kim must believe that Stich is wrong about this.

20. Cartwright (1983; 1999) has famously argued that the world is *not* a single, working machine, but is instead "dappled," by which she means ontologically disunified. Dupré (1993) has urged a similar thesis. For reasons given in Spurrett (1999; 2001a) we reject this conclusion. The fact that science is never finished, and therefore never completely unified, may mean that its current description of the world at any given time will always be of a world that is "dappled"; but to derive *as a metaphysical conclusion* the claim that the world *is* dappled is to simply abandon the regulative ideal that informs Salmon's project, and, for that matter, Kim's. Answering Kim *this way would* simply amount to shrugging off the significance of realist metaphysics, another way of trying to have lunch for free.

21. The "something," we would say, is indeed fundamental structure; Ross (2000) takes it to be the network of Schrödinger-style negentropic relations. That network is *our* favorite candidate for universal glue.

22. Kitcher develops, at length, additional criticisms based on counterexamples to Salmon's technical criteria for distinguishing genuine causal processes from pseudo-processes. We will not incorporate these into our summary here, because they contribute little to the issues relevant to our discussion, and because even if Salmon's apparatus is repaired so as to block the counterexamples, Kitcher's main critique is unaffected.

23. It is common outside philosophy for Dennett to be called a "reductionist" because he analyzes intentionality and consciousness without recourse to any entities or processes incompatible with the causal closure of physics. However, Dennett in fact denies, like us, that there is any *general* relation between physics and special sciences stronger than global supervenience; and in the context of most debates in philosophy of science, this makes him as anti-reductionist as the recent tradition allows. Thus, for example, when Kincaid (1997) defends anti-reductionism, he feels he needs to spend a few pages showing that he need not go as *far* in that direction as the "radical" Dennett. Ross (2000) explains in detail the sense in which Dennett's anti-reductionism is radical.

24. Philosophers typically grant that our current physical theories are open to revision, so the point here is *slightly* more complicated. Still, for philosophers of mind an ideal physicist is generally *assumed* to be making unproblematically causal claims, whereas an ideal economist, say, would need to do additional

philosophical work over and above her economics to justify thinking of her claims as causal.

25. We owe this insight to Andrei Rodin.

26. The logic of this, and comparison of the causation concept's role in different branches of science, is made formally explicit in a recent paper by Thalos (2002).

27. We allude to the earlier references to the work of Nottale (1993; 2000).

28. This point has also been vividly argued by Dennett (1991a).

## Open Peer Commentary

### Metaphysics, mind, and the unity of science

David Boersema

Department of Philosophy, Pacific University, Forest Grove, OR 97116.

boersema@pacificu.edu

**Abstract:** Ross & Spurrett's (R&S's) rebuttal of recent reductionistic work in the philosophy of mind relies on claims about the unity of science and explanation. I call those claims into question.

Ross & Spurrett (R&S) have written a spirited, and I believe fundamentally correct, rebuttal of recent work in metaphysics that seeks to undermine the anti-reductionist, functionalist consensus of the past few decades in cognitive science and philosophy of mind. Their rebuttal focuses on challenging metaphysicians' treatment of causality and their conception of physics, including the relationship between physics and metaphysics. Calling such recent work in metaphysics "the new scholasticism," R&S decry its "unhealthy disregard for the actual practice of science" and its tendency to "drift away from relevance to and coherence with scientific activity" (target article, sect. 1, para. 5). Although much of R&S's article focuses on the recent work of Jaegwon Kim in particular (Kim 1998), in this short commentary I will address the more global concerns of explanation and the unity of science. Also, although I agree with much of what I take R&S's project to be here, I will emphasize in my comments those aspects that I find suspect or at least in need of clarification.

R&S claim that: "The goal of science is to discover the structures in nature. We can discover such structures because, as fairly sophisticated information-transducing and processing systems, we can detect, record, and systematically measure mark-transmitting processes" (sect. 4.2, para. 5). This brief claim points to and points out several issues that beg for elaboration. While bemoaning the strident reductionism found in the work of recent metaphysicians (or what I take as the metaphysicians' commitment to a unity of science), R&S apparently do not deny a unity of goals of science (to discover nature's structures). The unity of science that is questionable, then, I assume is: (1) unity of methods, (2) unity of values, or (3) unity of content (i.e., epistemic unity, axiological unity, or ontological unity). Unity of methods I take as a commitment that the various sciences, in the final analysis, do or ought to investigate the world following the same (or similar enough) procedures, processes, and methods in order to provide accurate, reliable, and perhaps replicable information. I infer that R&S reject the notion that all of the various sciences do or even ought to do this. We hope clinical trials on the effectiveness of placebos will involve double-blind studies, but we don't expect cosmologists trying to determine more accurate parameters of black hole event horizons to involve such studies. But what reductionist would think otherwise?

Unity of values I take as a commitment that the various sciences, in the final analysis, do or ought to demand the same (or similar enough) standards, aims, and so on for (1) the worthiness of scientific information (i.e., unity of epistemic values such as quantifiability of data, levels of accuracy or precision, etc.) or (2) the social significance of scientific information (i.e., unity of social/ethical values such as providing predictable control of applications, emancipatory value of information, etc.). The epistemic values and the social/ethical values that we attach to and demand of scientific investigations are not uniform. Given the consequences of a mistake in the interpretation of data, we vary our expected level of confidence in experimental results. But again, what reductionist would think otherwise? Therefore, if R&S's concerns about reductionism are concerns about the unity of science, it is not clear that such concerns are ones about epistemic or axiological unity of science because it is not clear that reductionists are committed to any such unities. Is the concern then really just ontological unity? If it is, I confess that I share some of their reticence, although it is not obvious to me that most recent metaphysicians are guilty of being committed to such unity. We do demand that the content of the "special sciences" (i.e., not physics) not violate the content of physics because we take physics to tell us about the basic components and constituents of the world. However, it is not clear that the demand that the content of, say, cognitive science not contradict the content of physics is the same as the demand that the content of cognitive science be formally derivable from or eliminable into the content of physics. The difference between the two, I take it, actually lies not in any ontological commitments (to unity or otherwise) but rather in what counts as being explanatory of the phenomena being investigated. It is just this notion of explanation where I find another point that begs for elaboration. Quickly, before turning to that, however, I will repeat the present concern, which is just what sense of unity of science that R&S find so questionable (at best) or reprehensible (at worst). I find an underlying commitment to unity in their rejection (target article, Note 20) of Cartwright's and Dupré's criticisms of such unity (Cartwright 1983; 1999; Dupré 1993).

In their account of explanation, R&S draw heavily and directly from the work of Kitcher's (1981) unification model of explanation and Salmon's (1984) causal model. In combating a commitment to a reductionist unity of science view, however, they take not causality but information transmission (in the mathematical sense of Shannon & Weaver 1949) as the primitive notion for explanation. I find such a move to be fecund and philosophically more fruitful than a reliance on a causal model, but its very virtue is one I would take a reductionist to find as missing the point. The information-transmission model of explanation retains the virtue that R&S want, which is to provide an objective measure of explanation that does not make any ontological commitment to any reduction to physics. The old stand-by about the bank robber Willie Sutton demonstrates that what counts as a good explanation is relative to the aims and goals of the inquiry. ("Willie, why do you rob banks"? Willie: "Because that's where the money is.") However, I take it that for Kim and other metaphysicians, it is not a *good* explanation that is sought, but *the correct* explanation (with emphasis on *the* correct explanation, not *a* correct explanation). Reductionists, I suspect, would find R&S's discussion of explanatory accounts to be beside the point, because any explanation that is not finally cashed out in physical terms is not correct, regardless of how good it is toward salving any particular inquiry.

## Ontological disunity and a realism worth having

Steve Clarke

Centre for Applied Philosophy and Public Ethics, Charles Sturt University and Australian National University, Canberra ACT 2601, Australia.

stephen.clarke@anu.edu.au

<http://www.csu.edu.au/faculty/arts/cappe/people/clarst/clarst.htm>

**Abstract:** Ross & Spurrett (R&S) appear convinced that the world must have a unified ontological structure. This conviction is difficult to reconcile with a commitment to mainstream realism, which involves allowing that the world may be ontologically disunified. R&S should follow Kitcher by weakening their conception of unification so as to allow for the possibility of ontological disunity.

According to Ross & Spurrett (R&S): "Science aims to tell us how the world is structured, that is, how its various processes and classes of entities constitute a single working machine" (sect. 4.2, para. 5). They consider this claim to be "crucial to any sort of realism worth having" (sect. 4.2, para. 5). R&S's crucial claim sits very awkwardly with a consideration that is usually taken to be part and parcel of a serious commitment to realism. This is the requirement that the world be conceived of as existing independently of our thinking about it – the realist requirement of mind independence. The committed realist will be on the lookout for unwarranted presuppositions that we bring to our interpretation of the world and will attempt to get by without such presuppositions. The assertion that science aims to tell us how the various aspects of the world collectively constitute a "single working machine" looks like it is based on the presupposition that the world must be a single working machine. From the perspective of a mainstream realist who is committed to a conception of the world as mind independent, this is an unwarranted presupposition because it seems possible that the world is not a single working machine.

R&S do not do much to unpack the phrase "single working machine," and it may be thought that the above line of reasoning could be evaded if their commitment to a conception of the world as a single working machine was interpreted in a sufficiently nebulous way. However, R&S appear to disqualify themselves from adopting this line of defense by explicitly identifying Nancy Cartwright's (1999) "dappled world" thesis – the view that the world is ontologically disunified, lacking unifying laws, kinds, or other universal ontological categories – as a thesis that is incompatible with the claim that the world is a single working machine (target article, Note 20). Their conception of the world as a single working machine involves the assumption that the world must have a unified ontological structure.

R&S cite recent work, owing to Spurrett (1999; 2001a), that takes issue with Cartwright's claim that there is strong evidence that the world is ontologically disunified (Note 20). I agree with Spurrett that Cartwright (1999) has not done enough to warrant this conclusion. Nevertheless, it surely is possible that the world is ontologically disunified. We do not have to insist that the world is ontologically disunified to have grounds to doubt the claim that the world must have a unified ontological structure. We can be "agnostic dappers," to invoke Lipton's (2002) terminology, remaining open to the possibility that the world is ontologically disunified, as well as remaining open to the possibility that it is ontologically unified. If we adopt this sensible open-minded attitude, then an insistence that the world must be ontologically unified remains in tension with the realist ambition to depict the world as it is, independent of our presuppositions about it, because we remain open to the possibility that we are living in a disunified world.

R&S associate the claim that there is a unified ontological structure to the world with the work of Philip Kitcher, and they draw heavily on Kitcher's unificationist account of explanation in an effort to identify a form of explanatory unification that is suitable to their conception of science. Their reliance on Kitcher is unfortu-