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 dapplers," 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-

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nate because Kitcher has long recognised that the presumption that the world must be ontologically unified is a weakness of his unificationist approach to explanation. In his words: "it looks as though the approach must defend the *prima facie* implausible thesis that the world is necessarily unified" (Kitcher 1989, p. 496). Kitcher's initial response to this problem was to "recommend rejecting the idea that there are causal truths that are independent of our search for order in the phenomena. Taking a cue from Kant and Peirce, we adopt a different view of truth and correctness" (1989, p. 487). This is *a* solution to the problem created by the possibility of ontological disunity, but it is not a solution that genuine realists, which R&S purport to be, should be happy to endorse. In effect Kitcher is proposing that we compromise realist ambitions by adopting a Kantian position in which order is, at least in part, projected onto the world. Kitcher (1989, 1994) is quite explicit about the Kantian flavor of his views.

Kitcher has recently undergone a change of heart. He now tells us that his "grand project of articulating the most unified vision of nature that we could achieve . . . is mistaken" (Kitcher 1999, p. 347). In 1989 Kitcher was a grand unifier, but the 1999 Kitcher is an advocate of "Modest unificationism." Modest unificationism involves accepting that "the world may be a disorderly place, that the understanding of its diverse phenomena may require us to employ concepts that cannot be neatly integrated" (1999, p. 339). Modest unificationism involves looking for unity where we can find it, while accepting that there may be limits to the amount of unity that is there to be found. It is a position that should be congenial to genuine realists because it does not involve presuppositions about the ontological structure of the world.

R&S begin by observing that "Philosophy progresses with a tide-like dynamic." The low tide of logical positivism was more than half a century ago, but it seems that the high watermark of realism has not been reached, if their article is any guide. Their conviction that science should aim to describe the world as a single working machine appears to be an unwarranted remnant of the strong unificationism characteristic of the heyday of logical positivism. Kitcher has abandoned a similar conviction, and I can only urge R&S to follow his lead. Mainstream realism is compatible with the weak unificationism that Kitcher (1999) now advocates but not with the form of unificationism that R&S currently favor.

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Reduction, supervenience, and physical emergence

John Collier

Philosophy Programme, University of KwaZulu-Natal, Durban 4041, South Africa. **collierj@nu.ac.za** **http://www.nu.ac.za/philund/collier**

Abstract: After distinguishing reductive explanability in principle from ontological deflation, I give a case of an obviously physical property that is reductively inexplicable in principle. I argue that biological systems often have this character, and that, if we make certain assumptions about the cohesion and dynamics of the mind and its physical substrate, then it is emergent according to Broad's criteria.

Reduction is ambiguous in three ways. It may mean inter-theoretic reduction, the reduction of fundamental kinds of things (substance, traditionally), or that certain particular entities (objects, processes, or properties) can be eliminated without any loss of explanatory power in principle. I will ignore inter-theoretic reduction. The reduction of the number of fundamental kinds of things is best called *ontological deflation.* I will assume the closure of the physical (physicalism), and I will assume that all scientific explanation is in some sense causal and that explanatory power is lost only if the causal nature of a higher level entity is not in principle completely reductively explicable.

Despite supervenience, if explanatory reducibility fails in principle for some entity, then it is emergent. If there is no possible argument (deductive or inductive) from the parts, their intrinsic properties, and their relations to the full causal powers of the entity itself, then reductive explanation fails in principle. I will show that this holds for certain obviously physical properties of some systems under certain specific conditions. I will further argue that this helps to identify a class of systems for which reductive explanability fails. In these cases, even if physicalism is true, they are emergent. This idea of emergence fits C. D. Broad's criteria (Collier & Muller 1998).

The planet Mercury was found in the 1960s to rotate on its axis three times for each two times it revolves around the sun. This was extremely surprising because it had been thought that it would be in the same 1:1 harmonic as our moon-earth system. There are several more complex harmonic relations in the solar system. It is well known that the three-body gravitational problem is not solvable analytically, but it can be solved numerically, in principle, to any degree of accuracy we may require for any finite time (this is true for any Hamiltonian system). However, these cases involve the dissipation of energy through tidal torques unless the system is in some harmonic ratio. We would like, ideally, a complete explanation (possibly probabilistic) of why Mercury is in a 3:2 harmonic. Because of the high mass of the sun and the proximity of Mercury to the sun, the high tidal torque dissipates energy reasonably quickly in astronomical time; therefore, Mercury is likely to end up in some harmonic ratio in a finite amount of time. The central explanatory problem then becomes: why a 3:2 ratio rather than a 1:1 ratio, like our moon, or some other harmonic ratio?

We cannot apply Hamiltonian methods, because the rate of dissipation is roughly the same as the characteristic rate of the phenomenon to be explained. If the dissipation rate were small, then we could use an approximate Hamiltonian system; if it were large, we could use a step function. We are left with the Lagrangian. It is well known that these are not always solvable even by numerical approximation. I will give an intuitive argument that the Mercury's harmonic is such a case. Each of the possible harmonics is an *attractor.* Why one attractor rather than another? If the system were Hamiltonian, then the system would be in one attractor or another. In principle we could take into account the effects of all other bodies on Mercury and the sun (assuming the universe is finite, or at least that the effects are finite) and decide with an arbitrarily high degree of accuracy which attractor the system is in. However, given the dissipative nature of the system, it ends up in one attractor or another in finite time. If we examine the boundaries between the attractors, they are fractal, meaning that every two points in one attractor have a point between them in another attractor, at least in the boundary region. This is as if the three-body gravitational problem had to be decided in finite time, which is impossible by numerical approximation (the problem is noncomputable, even by convergent approximation). Therefore, there can in principle be no complete explanation of why the Mercury-sun system is in a 3:2 harmonic. There is approximately a one-third chance of 3:2 capture, one-half chance of a 1:1 capture, and the rest of the harmonics take up the rest of the chances. The chances of a 3:2 capture are good but not that good. The system is obviously physical, but it has a nonreducible property. This property fits Broad's notion of emergence.

How does this apply to the mind? It is highly likely that there are nonlinear dissipative processes in the brain in which the rates of the processes are of the same order as the rate of dissipation. There are also likely to be huge numbers of attractors. The larger the number of attractors, the lower the probabilities of capture in any particular one generally; therefore, a complete reductive explanation seems highly unlikely. This case is certainly true for many biological processes (as in development and in evolution; see Brooks & Wiley 1988; Kauffman 1990). The brain is, after all, biological. We must explain backwards from the attractors that are

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