Functionalism without multiple supervenience

Ausonio Marras

Department of Philosophy, University of Western Ontario, London, ON, N6H 1T4, Canada. **amarras@uwo.ca** **http://publish.uwo.ca/~amarras**

Abstract: Multiple supervenience is a problematic notion whose role can well be served by a contextualized or properly restricted standard notion of supervenience. It is furthermore not needed to defend functionalism against Kim's charge that cross-classifying taxonomies imply a serious form of dualism; nor does Ross & Spurrett's (R&S's) Kitcherian account of the metaphysics of causation crucially depend on multiple supervenience.

Because *multiple supervenience* is meant to play a large role in Ross & Spurrett's (R&S's) account of the metaphysics and epistemology of special science explanations, it is important to be clear as to what kind of relation it is and how it is supposed to help us resist Kim's reductionist stance. The notion makes its appearance in the context of the authors' response to Kim's (1998) charge that nonreductionists who appeal to the "cross-classification thesis" with respect to the mental and physical taxonomies are committed to abandoning psychophysical supervenience and to embracing "a serious form of dualism" (for supervenience is required for upholding the "causal closure of physics," a minimal requirement for physicalism). Here is what the authors say to this: "According to Kim, [holding the cross-classification thesis] amounts to a denial of supervenience as a one-way relation, permitting what Meyering (2000) calls 'multiple supervenience'" (sect. 3.1, last para.). They then go on to suggest that there are reasons for doubting that multiple supervenience implies any sort of dualism that denies the causal closure of physics. Because, as they later point out (sect. 3.3), Kim never confronts the idea of multiple supervenience ("it's off his radar in so far as it is more powerfully antireductionist than anything he seems willing to consider"; sect 3.3, last para.), their response to Kim suggests that even if he is right in claiming that cross-classification implies the denial of "one-way supervenience," he nonetheless fails to appreciate that this leaves open the possibility of another kind of supervenience, *multiple supervenience,* which (by their lights) is consistent with cross-classification, as well as with the causal closure of physics.

I think there are problems with this response. First, what sort of relation do R&S understand multiple supervenience to be? By contrasting it to "supervenience as a one-way relation," they seem to imply that multiple supervenience is *not* a one-way relation, and by supposing that the possibility of multiple supervenience enables one to "reject [Kim's] implicit premise that supervenience relations must all be 'downward,'" or that they all "point unidirectionally to physics" (sect. 3.2, para. 2), they seem to imply that *multiple* supervenience may point *upwards,* in the opposite direction than the standard sort of supervenience entailed by multiple realization. I think this is a confusion. All supervenience, multiple or otherwise, is a "one-way," unidirectional relation from the higher (functional) level to the lower (realization) level if conceptualized as a *dependence* relation, and from the lower to the higher level if conceptualized in terms of a relation of *determination.* The only difference is that the *mapping* effected by standard supervenience is a *one-many* mapping (at least if multiple realization is involved), whereas in the case of multiple supervenience, the mapping is *many-one:* multiple higher-level properties supervene on the same base property. No doubt R&S must have meant something of the sort; for surely the "direction of determination" (or, conversely, the "direction of dependence") remains the same in both cases.

Second, the idea of *multiple* supervenience so characterized is, strictly, incoherent. Consider two distinct, nonequivalent higher-level properties M1 and M2, and suppose that something x exemplifies M1 but not M2 at $t1$ and M2 but not M1 at $t2$ (i.e., suppose that x has undergone a change with respect to its M properties). Multiple supervenience would have us suppose that there might be a base property, P, on which *both* M1 and M2 supervene. However, that is impossible: by definition, supervenience requires that there cannot be a change with respect to the supervening properties without a corresponding change with respect to the subvening properties. One could fix this by imposing certain restrictions, for example, by requiring that the supervening properties be coextensive (where none can be exemplified without the others being simultaneously exemplified), by relativizing them to a given context (as would be natural in "Twin-Earthian" cases) or interpretation scheme (as when the same physical process in a computer implements different programs), or by broadening the supervenience base so as to include the appropriate contextual conditions. However, then it is not clear that the notion of *multiple* supervenience does any work that cannot be done by the standard notion of supervenience, locally or nonlocally construed.

Third, multiple supervenience is, in any case, not needed to answer Kim's challenge from cross-classifying taxonomies. We can have cross-classification *either* when we can make distinctions in terms of the higher-level properties that we cannot make in terms of the base properties, *or* when we can make distinctions in terms of the base properties that we cannot make in terms of the higher-level properties, *or* both. Now it is clear that when we are dealing with higher-level *functional,* and, in particular, *mental* properties, it is the *second* of the aforementioned options that is the relevant one, for it is of the essence of functional/mental properties that they be (at least in principle) *multiply realizable.* However, that implies that there are distinctions that can be made by the *base* (or *physical*) taxonomy that cannot be made by the functional/mental taxonomy, and that is just to say that the former supervenes on the latter. Therefore, cross-classification, in so far as it pertains to the functional/mental taxonomy *vis-à-vis* the physical taxonomy, does not violate supervenience and thus entails no "serious form of dualism." Conversely, the first and the third options above do entail the denial of standard supervenience: they represent precisely the sort of situation envisaged under *multiple supervenience* (hence, my earlier claim that unrestricted multiple supervenience is not supervenience at all). Far from providing a way to meet Kim's challenge from cross-classification, multiple supervenience falls prey to just that challenge.

Fortunately, then, functionalism does not have to depend on multiple supervenience to prove its metaphysical credentials, nor do R&S's valuable insights about the autonomy of functionalist explanation in the special sciences. Indeed, what does all the interesting work in their defense of functionalism against Kim's epiphenomenalist challenge is the unfolding of the Kitcherian idea that the metaphysics of the attribution of causal powers cannot be divorced from the epistemology and methodology of explanation, whose holistic, unificatory, and highly contextual character has no reflection in Kim's "conservatively metaphysical" conception of causation. Whether this idea is itself ultimately defensible is, of course, another matter.

Really taking metaphysics seriously

Barbara Montero

Department of Political Science, Economics and Philosophy, The College of Staten Island of the City University of New York, Staten Island, NY 10314. **barbara@antinomies.org** **http://barbara.antinomies.org**

Abstract: Ross & Spurrett (R&S) fail to take metaphysics seriously because they do not make a clear enough distinction between how we understand the world and what the world is really like. Although they show that the behavioral and cognitive sciences are genuinely explanatory, it is not clear that they have shown that these special sciences identify properties that are genuinely causal.

Ross & Spurrett (R&S) claim to be taking metaphysics seriously, but I doubt metaphysicians such as Kim would agree. Taking metaphysics seriously means in part making a distinction between

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how we understand the world and what the world is really like, that is, between explanation and ontology, and it seems that even if R&S have shown that the behavioral and cognitive sciences are genuinely explanatory, as I think they have, it is not clear that they have shown that these special sciences identify properties that are genuinely causal. As such, R&S's article fails to convince the serious metaphysician who is persuaded by Kim's causal exclusion argument that mental properties can perform real causal work.

Explanations in nonfundamental sciences, including much of physics, as R&S point out, are frequently not entirely bottom-up. Moreover, as they also argue, it is not at all clear how one could eliminate top-down explanations. For it does seem that when we substitute explanations in terms of neural states for explanations in terms of beliefs and desires, we lose the very phenomenon we are trying to explain. But is this a point about our cognitive abilities or a point about the way the world works? That is, is it an epistemological point or a point about the ontological nature of beliefs and desires?

R&S take the ineliminability of top-down explanations to show something about the way the world works because they take the connection between explanation and ontology to be tight. In fact, they claim that an explanation is not something that is merely psychologically satisfying, "but must cite explanans that are . . . true" (sect. 3.1, para. 1). If this were the case, the fact that the cognitive and behavioral sciences are not explanatorily irrelevant would also show that they could carry their ontological weight. However, although I agree that an explanation should be something more than merely psychologically satisfying, requiring that the explanans be true would rule out many, if not most, of our current scientific explanations from counting as explanations because many, if not most, of our current scientific explanations are probably false. For example, Newton's laws are taken to be explanatorily powerful yet are known to be false. And most likely, given the history of scientific theorizing in the hard and especially the soft sciences, it is likely that much of our currently accepted theories, which are employed to explain various phenomena, will turn out to be false. Thus, if we require explanations to cite explanans that are true, we have to admit that probably science is not explaining much of anything, which I would think R&S, being themselves good naturalists, would not want to do. Therefore, while requiring explanans to be true weds explanation to ontology, it does so at a high price. Once we give up the requirement that explanans must be true, however, we have a gap between scientific explanation and how the world really is, a gap that a savvy metaphysician such as Kim can attempt to pry open.

It is a distinct question whether Kim has pried open the gap between how science explains the world and how the world really works, showing in effect that we must be mistaken either in our belief that the special sciences traffic in causal properties or in our belief that explanations in the special sciences are in some significant sense irreducible. I happen to think that Kim has not done this. R&S claim that the causal exclusion problem turns upon there being a clear-cut notion of causation in fundamental physics. However, I do not think that it does. Kim can avoid talk of fundamental physics because the causal exclusion argument can be reformulated as a problem about the apparent overdetermination of the neural and the mental. Arguably, neurophysiology is causal (neurophysiologists, at least, do make causal claims), and it also seems likely that once we set the neurophysiological cause, one does not need to add anything mental to produce the desired effect. Therefore, it would seem that R&S's well-grounded skepticism about causal concepts in the domain of fundamental physics is beside the point.

Although this reinterpretation of the causal exclusion argument cannot be faulted for assuming that there is a clear-cut notion of causation in fundamental physics, it can be faulted for another reason. As I see it, while systematic causal overdetermination may be metaphysically profligate when the causes at issue are relevantly distinct, such as when a man is simultaneously shot and suffers a heart attack, and as such, his death is caused twice over, mental causes in as much as they are constituted by neural causes are not distinct in this way.[1] Is there any reason to say that the neurophysiological and not the mental does the real causal work? Certainly there is no more reason to say this than to say that aspirin does no real causal work and that only the ingredients of aspirin do. Since we need not reject aspirin's causal powers, we need not reject that the mental gives us real causal powers. Because of this, sciences trafficking in such causes are doing more than mere stamp collecting.

In responding to the causal exclusion argument in this way, am I trying to get a free lunch? I think not. The response does not reject the causal exclusion argument merely because it is general and thus, if successful, would not only render the mental causally profligate but also virtually all other phenomena save for those at the level of fundamental physics. Rather, the response provides a metaphysical distinction between properties that cause problematic overdetermination and properties that do not. As such, it seems to me to be a much more straightforward way to address Kim's causal exclusion argument and, at the same time, to take metaphysics seriously.

NOTES

1. Melnyk (2003) argues for this point.

The vessels and the glue: Space, time, and causation

Andrei Rodin

Department of Philosophy, Ecole Normale Superieure, 75230 Paris Cedex 05, France. **rodin@ens.fr**

Abstract: In addition to the "universal glue," which is the local mechanical causation, the standard explanatory scheme of classical science presumes two "universal vessels," which are global space and time. I call this outdated metaphysical setting "black-and-white" because it allows for only two principal scales. A prospective metaphysics able to bind existing sciences together needs to be "colored," that is, allow for scale relativity and diversification by domain.

If our world could be satisfactorily accounted for by a single science, then we would not need to distinguish a particular science of metaphysics or any other particular science. Because this is not the case and we have numerous sciences that cannot be reduced into one *trivially* (to say the least), we need metaphysics to work on gluing those sciences together, be the *glue* some kind of reduction to universal physical laws or something else. Aristotle invented metaphysics (which he called *first philosophy*) to bind physics (by which he meant broadly the study of all natural phenomena) with mathematics and logic (so afterwards the latter two disciplines could be considered as tools for the former). Because Aristotle's physics has branched into numerous disciplines, our need for the unifying science of metaphysics is even stronger than Aristotle's. A scientist calling for a *free lunch* has two options: either to take uncritically the nostalgic dogma of reductionism according to which in the distant future all sciences will collapse back into physics (to leave aside unification dogmas borrowed from outside of science), which is epistemologically irresponsible, or to give up the idea of unity of science, which turns science into a combination of mystery and stamp collecting. If no reasonable and testable reductionist hypothesis can be made *now,* then this is a job of metaphysicians to suggest tentative ways to glue sciences by means other than reduction. It goes without saying that working on binding sciences together a metaphysician must have a good understanding of what he or she is going to bind. Otherwise, the unifying efforts of a metaphysician will be simply ignored by the scientific community and for good reason. As Ross & Spurrett (R&S) show, this unpleasant situation is not uncommon even for the mainstream metaphysical discussion.

Now let me be more specific about the *glue.* R&S label as " localist metaphysics" and "localist paradigm" a generalised explana-