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Why Metaphysical Abstinence Should Prevail in the Debate on Reductionism

Stéphanie Ruphy

My main aim in this paper is to show that influential antireductionist arguments such as Fodor's, Kitcher's, and Dupré's state stronger conclusions than they actually succeed in establishing. By putting to the fore the role of metaphysical presuppositions in these arguments, I argue that they are convincing only as 'temporally qualified argument', and not as 'generally valid ones'. I also challenge the validity of the strategy consisting in drawing metaphysical lessons from the failure of reductionist programmes. What most of these antireductionist standpoints have in common is a pretension to methodological imports. I conclude by explaining why, in order to remain relevant for scientific practice, antireductionist arguments should stay clear of metaphysics, for the latter, I argue, does not mix well with a taste for methodological prescriptions.

1. Introduction

In his influential discussion of the reducibility of classical genetics to molecular biology, Kitcher writes the following: 'Antireductionism construes the current division of biology not simply a temporary feature of our science stemming from our cognitive imperfections but as the *reflection of levels of organization in nature*' (Kitcher 1984, 371; my italics). Fodor makes a somewhat similar claim in his classical multiple realizability argument in favour of the irreducibility of the special sciences to physics: 'I am suggesting, roughly, that there are special sciences not because of the nature of our epistemic relation to the world, *but because of the way the world is put together*: not all kinds […] are, or correspond to, physical kinds' (Fodor 1974, 113; my italics). What these two quotations suggest is that for antireductionists such as Fodor and Kitcher, reductionism fails because of the way the world is and not (only) because of the way, we, investigators of the world, are.

Stéphanie Ruphy is Assistant Professor in Philosophy of Science at University of Provence (Aix-Marseille I), France.
Correspondence to: Stéphanie Ruphy, Départment de philosophie, Université of Provence (Aix-Marseille I), 29, avenue Robert Schuman, 13621 Aix-en-Provence Cedex 1, France. Email: stephanie.ruphy@wanadoo.fr

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My main aim in this paper is to show that antireductionist arguments such as Fodor's and Kitcher's state stronger conclusions than they actually succeed in establishing. The issue can be formulated in the following way: are these arguments convincing only as 'temporally qualified' arguments, that is, arguments whose validity depends on our cognitive capacities or on the present state of our knowledge?[1] Or are they convincing as 'generally valid' arguments, that is, arguments whose validity depends solely on the way the world is? This clarification of the status of antireductionist arguments is all the more necessary because most antireductionist standpoints go hand in hand with *methodological* prescriptions whose value for scientific practice depends, I will argue, on the kind of considerations—metaphysical or empirical—they are based on; hence, the necessity of clarifying the role and discussing the legitimacy of metaphysical considerations in arguing against reduction.

The paper will be organized as follows. After brief preliminary remarks specifying what I mean by 'metaphysical considerations' and what kind of reductionism is the main target of the antireductionist arguments discussed in this paper, I will divide my discussion of the status ('temporally qualified' vs. 'generally valid') of these arguments in two parts, corresponding to two distinct antireductionist strategies. Attacks against reduction may deny that reductions provide what classical reductionists such as Oppenheim and Putnam (1958) and Nagel (1961) claim they provide, namely, explanations, or they may challenge their feasibility.[2] Otherwise put, an antireductionist may defend the existence of autonomous levels of explanation in science, regardless of the existence or non-existence of actual successful reductions, or they may deny the very possibility of reduction.

In the last part of my paper, I will address an issue that is somehow the symmetrical of the previous issue: is the strategy consisting in *drawing* metaphysical lessons from the failure of reductionism valid? For the fact is that for antireductionists such as Dupré (1993, 1996, 2002) and Cartwright (1999, 2001), not only must the reductionist ideal of the unity of science be abandoned, but so should its underlying metaphysical picture of an 'ordered' world, to which one should substitute the alternative metaphysical picture of a 'disordered' world, allegedly derived from the failure of reductionist programmes.

In the closing section, I will show that appealing to metaphysical considerations in arguing against reduction undermines the possibility of drawing useful methodological moral for practising scientists.

2. Preliminary Remarks

By 'metaphysical considerations', I mean in this paper considerations of the type 'the world has feature A', for which no direct empirical support is available. My rejection of metaphysics in the debate on reductionism will thus be restricted to factual, but empirically unverifiable, assertions about nature. Note also that my argument leaves untouched the issue of the *general* interest of such metaphysical claims in other contexts: as I will explain in more detail later, it just states their illegitimacy in antireductionist arguments purporting to have methodological imports.

I will be concerned in this paper with influential antireductionist arguments aiming at a certain degree of generality and purporting to make a stronger claim than just showing that the thesis of reducibility *in practice* is false for a given scientific field. As noted, for instance, by Sober (1999, 554, n. 14), one can simply inspect present-day science to show this and general antireductionist arguments such as the multiple realizability argument are not needed. Authors of the antireductionist arguments discussed here make it clear that their target is reduction as an explanatory strategy, whose paradigm form is Nagel's (1961) concept of reduction (Garfinkel 1981, 51–52, Dupré 1993, 88 n. 1; Kitcher 1984, 337; Fodor 1974, 77–78).[3] To put it briefly, what is thus at stake is the availability of cross-sciences derivations and the explanatory virtue of these derivations.

3. Arguments for Autonomous Levels of Explanation

Let us start with the first kind of antireductionist strategy by focusing on two influential (and to some extent similar) arguments, one proposed by Garfinkel (1981, chap. 2), the other by Kitcher (1984, 2001).

Following Nagel's (1961) and Oppenheim and Putnam's (1958) classic accounts of reduction, Garfinkel emphasizes the explanatory purpose of reduction by giving the following general definition of the notion: 'theory A is reducible to theory B if theory B explains the phenomena previously the province of theory A' (Garfinkel 1981, 52). Assessing a claim of reduction thus boils down to assessing a claim about the dispensability of the explanations provided by the reduced theory. When the reductionist contends that the lower-level description is 'all there is', Garfinkel asks whether this reductionist claim implies that the explanations at the higher level are in any sense dispensable. Garfinkel answers negatively, and the bottom line of his argument is the contention that the microexplanation and the macroexplanation do not have the same object.

Take the example of an ecological system made up of foxes and rabbits, says Garfinkel. By building an algebraic model of the related variations of the two populations, macroexplanations of the fluctuations of the populations are easy to come up with. For instance, the macroexplanation of an event like the death of a rabbit would go something like that: the rabbit died because the level of the fox population was high (a high number of foxes placing high pressure on the rabbit population; one got eaten). Let us see now what a microexplanation of the same event—the death of a rabbit—would look like. A good candidate being, 'rabbit *r* was eaten because he passed through the capture space of fox *f*', Garfinkel notices that such a microexplanation does not have the same object as the macroexplanation. The macroexplanation says why a rabbit was eaten or not, whereas the microexplanation says why a rabbit *r* was eaten by a fox *f* at a certain time *t*. So, by including details irrelevant to the outcome (the death of a rabbit), the microexplanation fails to bring out the relevant feature of the situation to be explained at the macrolevel. Eliminating the macroexplanation would thus leave unanswered the question of why the rabbit was eaten at all. Moreover, argues Garfinkel, the microexplanation fails also to tell us what should be different for the rabbit not to get eaten at all (and not 'eaten by fox *f* at *t*'), namely a lower level of the fox population (for a sleeping fox *f* would not have saved the rabbit for long—fewer foxes would have).

In other words, microexplanations do not meet the requirement put by Garfinkel on explanation, to wit, that an explanation should tell us what could have been otherwise. And Garfinkel adds that this failure is inescapable for any system with redundant causality; that is to say, systems for which a situation can be caused not only by one situation but also by a bunch of other different situations. Hence, Garfinkel's conclusion: 'the explanation of the higher order state will not proceed *via* the microexplanation of the microstate which it happens to "be". Instead, *the explanation will seek its own level* [...]' (Garfinkel 1981, 59; my italics).

To sum up, Garfinkel is arguing not against the reductionist belief in the existence of a complete causal microstory of any microstate, but rather against the further claim that allegedly follows from this first claim, namely the *eliminability* of the macroexplanation.

A similar conclusion about the existence of autonomous levels of explanation is reached by Kitcher (1984), albeit from a slightly different argument. In his analysis of the actual relation between two theories of different levels in biology—molecular and classical genetics—Kitcher shows that appeals to molecular biology cannot explain for instance why Mendel's second law holds (for the sake of the argument, the law is considered as generally valid), whereas cytology provides an explanation based on the description of the process of meiosis. To assess the success or failure of an explanation at the molecular level, Kitcher embraces the following criterion: molecular biology should count in its natural kinds processes that can be identified with processes counting as natural kinds in cytology.[4] Since this criterion cannot be met (processes such as meiosis are realized in many different ways at the molecular level and consequently cannot be identified with a kind in molecular biology), the molecular account fails to provide an explanation. Here, too, as in Garfinkel's example of the fox–rabbit system, the lower-level explanation fails to bring out the relevant features of the situation at the higher level.

Kitcher (2001, 71) reinforces his point on autonomous levels of explanation with a very nice and simple example. In the early eighteenth century, it was observed that in the previous 82 years, more boys than girls had been born each year in London (note that the regularity does not reflect any peculiar English behaviour: it still holds today for all human populations). A microexplanation would focus on the physicochemical details of every fecundation and pregnancy leading to a birth in London in a given year, and that for the 82 years considered. And sure enough, by subtracting cases of female newborns from cases of male newborns, the analysis could in principle lead to the observed result. For all that, would the result be *explained*? Kitcher answers negatively, for the microexplanation fails to show that the observed regularity is not the outcome of a huge coincidence. Kitcher goes on to contrast the microexplanation with the available macroexplanation based on natural selection and conclude that to dispose with the macroexplanation leaves the regularity unexplained.[5]

At that point and in light of present knowledge, a reductionist might reply that a microexplanation does not exactly make the regularity 'anything more than a gigantic coincidence'. For we know now that male embryos are more prone to miscarriage than female embryos (the sex ratio at conception is 120:100). So, the microexplanation 'Y spermatozoids are more fertilizing than X spermatozoids' is now available (even if we still do not know where this advantage comes from), making the observed regularity highly *probable*, and not merely a coincidence. Nevertheless, the moral of Kitcher's story still holds to the extent that the microexplanation still fails to answer the question answered by the macroexplanation, to wit (in its new equivalent formulation), *why* Y spermatozoids are more fertilizing than X spermatozoids. Garfinkel nicely sums up the situation as follows: 'the microexplanation [can only] tell us the mechanism by which the macroexplanation operated' (1981, 58).

For all their convincing power, these arguments in favour of autonomous levels of explanation might appear somewhat ambiguous. Are they only arguments based on the epistemically reliable explanatory practice of scientists, or do they also have some metaphysical grounds? Otherwise put, does the explanatory purport of a reduction fail because of the way, we, investigators of the world, are, or because of the way the world is? It seems that Garfinkel would be content with the first option: his claim that explanations in science 'seek their own level' is typically an epistemic claim turning largely on what we happen to take as a satisfying explanation, and moreover, we have seen that Garfinkel is not challenging contentions about causal completeness at the microlevel. But the quotations given at the beginning of this paper suggest that antireductionists such as Fodor or Kitcher would endorse the second option. Let us see if this position is tenable. In Nagel's terminology introduced earlier, the issue is thus to know if arguments for the existence of autonomous levels of scientific explanation are 'temporally qualified' arguments or 'generally valid' ones.

A brief remark is first in order here. The distinction between 'generally valid' or 'temporally qualified' claims on reducibility is closely akin to the traditional distinction between reducibility in principle and reducibility in fact, minus perhaps some ambiguities. Reduction in principle usually refers to the theoretical possibility of reduction, whereas reduction in fact refers to its practical possibility. But as cogently noticed by Dupré (1993, 96), for instance, this theoretical/practical distinction is rather ambiguous. Consider a reduction requiring a calculus whose complexity is such that it could be achieved only by a computing device made up of more components than there are particles in the universe. Would that count as a practical or theoretical argument against reductionism? Well, I guess the answer depends on how much trust you put in your current theories about computing device and number of components in the universe. In any case, the possibility of a new theory of computing devices that would push back practical limits on calculus can hardly be excluded in principle. And indeed, quantum computing, albeit still very much in its infancy, renders null and void current estimations of computing capacities based on silicon electronics or even molecular electronics. One seems thus to be on clearer grounds with the distinction between 'temporally qualified' claims and 'generally valid' claims. Claims about the feasibility of a complex calculus are, without much ambiguity, temporally qualified claims (for they depend on the present stage of our knowledge on computing device). By contrast, antireductionist claims that would be based on the alleged existence of different levels of causality in the world are not temporally qualified claims but generally valid claims, for their validity does not depend on the epistemic context or on our cognitive capacities, but solely on how the world happens to be.

Let us go back now to the status of the arguments in favour of autonomous levels of explanation. Kitcher (1984, 348) considers explicitly the possibility that the failure of the reductive explanation of classical genetics by molecular biology is imputable to the limits of our cognitive capacities but immediately rejects this possibility on the following grounds: had we the cognitive capacities to grasp the full details of the molecular account of macroprocesses such as meiosis, claims Kitcher, we would still need the macroexplanation because the relevant explanatory features brought out by the cytological story would be missed or obscured by irrelevant details in the microreductive explanations (Kitcher 1984, 350). But how could that be so? How could the omission of macroexplanatory features be epistemically defective in some kind of absolute sense, that is, independently of what our cognitive capacities to handle the huge amount of details of the microstory happen to be? Kitcher leaves us on this crucial issue with no explicit answer. Instead, he points to a metaphysical way out by alluding at the end of his argument to the existence of 'levels of organization in nature' (see the quotation at the beginning of the paper). By refusing to link the inadequacy of the macroexplanation to the limitations of our cognitive capacities, Kitcher seems thus to commit himself to the thesis that the molecular explanation omits mention of some causally relevant macrofeature of the world.[6]

Kitcher's argument in favour of autonomous levels of explanation in science thus turns out to require, to *count as a generally valid argument*, a rather strong thesis about distinct levels of causality in nature, a thesis that Kitcher admittedly might not want to endorse (not surprisingly, he leaves this rather obscure notion of 'levels of organization in nature' unspecified), and in favour of which, in any case, he provides no argument. Given the notorious difficulties to establish claims about the causal structure of the world,[7] I am afraid that proponents of the existence of autonomous levels of explanation in science have to contend themselves with a more modest victory. In other words, arguments in favour of the existence of autonomous levels of explanations are undoubtedly very convincing as temporally qualified arguments about epistemically reliable practices. But they are much less convincing as generally valid arguments in favour of the explanatory failure of reduction, for such arguments, to count as *generally valid* arguments, need to appeal to claims about the causal structure of the world for which no argument is provided. (And I am even at a loss to see what an empirical argument in favour of the existence of different levels of causality could look like; inference to the best explanation could not do the trick here since it would be plainly circular, in order to establish the explanatory autonomy of the special sciences *as generally valid*, to infer from their current, temporally qualified autonomy, metaphysical claims about levels of causality.) Proponents of autonomy are certainly right to refuse microexplanations the status of 'the only genuine explanation', on the grounds that such a claim is loaded with unwarranted metaphysical beliefs about genuine levels of causality in nature. But such a sceptical stance should go both ways: a case in favour of autonomy should no more appeal to metaphysical beliefs about distinct levels of causality in nature.

4. Challenges to the Feasibility of Reduction

The second family of arguments against reductionism—namely attacks against the feasibility of reduction—will invite a similar diagnosis as regards their status ('temporally qualified' vs 'generally valid').

These antireductionist arguments are centred on the analysis of the form of the bridge principles required for reduction, in particular on the nature of the predicates linked by the bridge principles (see Nagel 1961, for a classical account of bridge principles in reduction). The common bottom line of these arguments is to say that the kinds of the theory to be reduced do not necessarily correspond, via a bridge principle, to a kind of the reducing theory and, moreover, that it is highly unlikely that it will ever be the case (following Fodor 1974, kind predicates of a science can be defined as the ones whose terms are the bound variables in its proper laws). Let us see in some detail why this is so. In his 'Special Sciences' article, Fodor (1974) considers for instance the issue of the reducibility of psychology to neurology. Fodor asks first what is required for such a reduction to take place. From considerations on the general form of reduction and the nature of bridge principles, Fodor draws the following answer: every psychological kind must be coextensive with a neurological kind, and the generalization that states this coextension must be a law. Is there any chance that this could be the case? For Fodor, we have every reason to doubt it. The grounds for such scepticism are the difficulty in pairing neurological structures with psychological functions, since a given psychological state may be brought about by different neurological states (and, vice versa, a given neurological structure may subserve different psychological functions).

An argument in the same vein is provided by Kitcher in his discussion of the reducibility of classical genetics to molecular genetics. Kitcher (1984) sets out to assess the reducibility of the two disciplines on three independent criteria. Kitcher's third criterion is the explanatory power of the reduction, whose failure has just been discussed in the preceding section. His first criterion is the existence of general laws about transmission of genes in classical genetics. His second criterion is the existence of bridge principles linking the vocabulary of the two branches. Failure to meet the first criterion follows from a careful examination of the actual form of the theory of classical genetics: one cannot find a few sentences that would encapsulate the content of the theory (Kitcher 1984, 340). Note that failure to meet this first criterion is a temporally qualified failure for it clearly depends on its epistemic context. As regards now the demonstration of the failure to meet the second criterion, Kitcher proceeds in a way roughly similar to Fodor's by diagnosing that the failure follows from the heterogeneous nature, from the molecular point of view, of key kinds of classical genetics such as 'genes'. Genes are indeed essentially characterized by their function and not by some structural properties. To put it simply, definitions of genes are about what they do, and not about what they are made of. And it turns out to be the case that the predicate 'gene' applies to many heterogeneous segments of DNA (and even segments of RNA), so that the 'gene kind' does not correspond to any molecular kind. In fact, the structural predicate from the molecular language linked to the predicate 'gene' by a bridge principle will be highly disjunctive. Now, why does that lead to a problem according to Kitcher? It is because since laws must succeed with counterfactuals, these 'disjunctive' bridge principles must cover not only the 'finitely many *actual* genes' but also 'the indefinitely many genes which *might* have arisen' (Kitcher 1984, 346); hence, the impossibility of coming up with adequate bridge principles.

In Fodor's article, this kind of multiple realizability argument applies not only to microreduction (that is, reduction between sciences whose domains of discourse are adjacent levels of organization, such as psychology/neurology or classical genetics/molecular biology) but also to the more general case of reduction of any special science to physical science. In this general case, reductionism requires that every kind (of every special science) be coextensive with a physical kind. And the general reasons to believe that it is highly unlikely are stated by Fodor as follows:

> (a) interesting generalizations [...] can often be made about events whose physical descriptions have nothing in common; (b) it is often the case that whether the physical descriptions of the events subsumed by such generalizations have anything in common is, in an obvious sense, entirely irrelevant to the truth of the generalizations, or to their interestingness, or to their degree of confirmation, or, indeed, to any of their epistemologically important properties; and (c) the special sciences are very much in the business of formulating generalizations of this kind. (Fodor 1974, 89–90)

This radically sceptical argument against the feasibility of reduction is indeed quite compelling, but here again, the issue is whether the argument is compelling as a generally valid argument or only as a temporally qualified argument. In spite of Fodor's contentions about 'the way the world is put together' (see the quotation at the beginning of this paper), it seems on the contrary that the validity of multiple realizability arguments clearly depends on the epistemic context: the physical descriptions Fodor is talking about may evolve, as well as the kind of generalizations sought by special sciences.[8] It may admittedly be difficult to imagine, for instance, what a classical genetics with a revised notion of gene much less heterogeneous from the molecular point of view (that is, a new version of classical genetics incorporating natural kinds much less recalcitrant to reduction) would look like, but the history of science provides us with *actual* cases where a reduction held as impossible between two sciences became possible when new theories were available in one or the other.

McLaughlin, for instance, explains in his detailed historical analysis of the 'Rise and Fall of British Emergentism' (1992) how claims of irreducibility of one science to another made at the beginning of last century became untenable in light of new theories. A case at hand is the contention that the bonding of chemical elements could not be given microexplanations in terms of subatomic particles. For the British emergentists, this claim was grounded in some peculiar metaphysical views about the causal structure of reality, summed up by McLaughlin as follows: 'some types of structures of material particles endowed the kinds they compose with fundamental causal powers that emerge from the types of structures' (McLaughlin 1992, 51). But this emergentist claim did not resist the scientific achievements of quantum mechanics, which was able to give a reductive explanation of the bonding of chemical elements. A similar conclusion is reached by McLaughlin as regards British emergentists' claims of irreducibility in biology: those claims did not resist either various advances in molecular biology or genetics (McLaughlin 1992, 54, 73). Moreover, McLaughlin explicitly attributes the vogue for talk of emergence during the first third of the last century to the epistemic situation at that time (McLaughlin 1992, 57) and then concludes that 'advances in science, not philosophical criticism, led to the fall of British Emergentism' (McLaughlin 1992, 90).

The type of antireductionist arguments discussed in this paper can admittedly not be put on a par with the heavily metaphysically loaded claims of British emergentists. When discussing the irreducibility of one science to another, one should nevertheless at least keep in mind the moral that can be drawn from this historical episode: the validity of claims of irreducibility depends on the epistemic context; such claims should not be grounded on some alleged metaphysical picture of the causal structure of the world or, in Fodor's words, 'on the way the world is put together', for how can assertions about 'the way the world is put together' be justified, independently of our evolving ways of describing it? It cannot. Here is Nagel's illuminating reply to emergentist arguments, worth quoting at length, for one can, I think, hardly be clearer on the irrelevance of metaphysical considerations to sustain or dismiss claims about the reducibility of one science to another:

> The [emergentist] conception is misleading because it suggests that the question of whether one science is reducible to another is to be settled by inspecting the 'properties' or alleged 'natures' of things rather than by investigating the logical consequences of certain explicitly formulated *theories* (that is, systems of statements). For the conception ignores the crucial point that the 'natures' of things, and in particular of the 'elementary constituents' of things, are not accessible to direct inspection [...]. Such 'natures' must be stated as a theory and are not the objects of observation; and the range of the possible 'natures' which chemical elements may possess is as varied as the different theories about atomic structure that we can devise. [...] Accordingly, the supposition that, in order to reduce one science to another, some properties must be deduced from certain other properties or 'natures' converts what is eminently a logical and empirical question into a hopelessly irresolvable speculative one.' [...] whether a given set of 'properties' or 'behavioral traits' of macroscopic objects can be explained by, or reduced to, the 'properties' or 'behavioral traits' of atoms and molecules is *a function of whatever theory is adopted for specifying the 'natures' of these elements*. (Nagel, [1961] 1979, 364–65; my italics)

Nagel pressed the same point when discussing his canonical example of the reduction of thermodynamics to statistical mechanics:

> If the 'nature' of molecules is stipulated in terms of the theoretical primitives of classical statistical mechanics, the reduction of thermodynamics is possible only if an additional postulate is introduced that connects temperature and kinetic energy. However, the impossibility of the reduction without such special hypothesis follows from purely formal considerations, and *not from some alleged ontological hiatus between the mechanical and the thermodynamical.* (Nagel [1961] 1979, 365; my italics)

More generally, Nagel emphasizes on several occasions that claims concerning the reducibility (or irreducibility) of a science 'must be temporally qualified', that is to say, questions on reduction are questions about 'the logical relations between sciences *as* *constituted at a certain time*' (my italics) and thus should not be discussed as if 'they were about *some ultimate and immutable structure of the universe*. […] What was impossible relative to one theory need not be impossible relative to another physical theory' (Nagel [1961] 1979, 364–65; my italics). A temporally qualified victory is thus definitely the best one can hope for; antireductionists may nevertheless find comfort in the fact that on this field, their camp have, in our present epistemic context, a crushing advantage over the reductionist camp.

5. Beyond Temporally Qualified Arguments: Dupré's Antireductionist Charge

But some members of the antireductionist camp are *explicitly* unhappy with a 'mere' temporally qualified victory, however crushing and lasting it may be: they want to make a case for disunity as 'an inescapable attribute of science' (Dupré 1996, 101). Not surprisingly then, what is needed is a metaphysics 'at least compatible with, and perhaps *even justificatory* of, that position […]' (Dupré 1996, 101; my italics). And indeed, Dupré's antireductionist charge against bridge principles explicitly purports to be grounded in 'a robust metaphysical basis' (Dupré 1996, 105). But at least, in Dupré's case, precise arguments in favour of this metaphysical basis are supplied. Let us see if they succeed in obtaining a generally valid victory over reductionism. The 'robust metaphysical basis' is provided by Dupré's rejection of natural kinds, leading to a metaphysical standpoint described as 'a combination of pluralism and realism' and coined 'promiscuous realism'.

Promiscuous realism boils down to the claim that there are no such things as natural kinds, but there are real kinds. This runs counter to the old Platonic metaphor of nature conceived as an animal at whose joints one should carve to classify things. The way we divide up things, argues Dupré, always depends on our theories and purpose. But this is not to say that there is no objective way of classifying objects. Rather, Dupré contends that there is more than one unique way to do so. His view is explicitly anti-nominalist: there are such things as 'real kinds', but individual things can be members of different real kinds, so that real kinds in science cannot be conceived as natural kinds (in particular, they cannot be conceived as natural kinds in any essentialist way) (Dupré 1996, 105). Given the abundant and compelling literature on the subject, I will take for granted the dismissal of the notion of natural kind in science (notwithstanding the revival of the notion by Putnam 1975 and Kripke 1972).[9] The question is now to see if promiscuous realism leads to generally valid arguments against reductionism.

Dupré (1993, 104–105) argues that a bridge principle provides a necessary and sufficient condition for membership of a kind in the science to be reduced, and moreover, this condition must be specified in structural terms. Dupré claims that this is tantamount to requiring the existence of natural kinds. If this point were valid, lack of natural kinds would indeed suffice to thwart reduction. But one might wonder whether Dupré's argument hinges on a peculiar conception of bridge principle, as suggested by the fact that in other antireductionist arguments, the possibility of bridge principle appears to depend not on the existence of natural kinds (in Dupré's strong sense), but rather on the possibility that the kinds ('real kinds' in Dupré's terminology) of the reduced science are linked via bridge laws to the kinds of the reducing science. To sum up Dupré's argument: if you buy promiscuous realism (first premise: there are no such things as natural kinds, but there are real kinds) and the claim that reductionism requires natural kinds (second premise) then, indeed, you would have a generally valid argument against reductionism. The problem is that the second premise is very disputable: it is not at all clear why bridge principles require natural kinds. This is certainly not a claim Nagel would have endorsed, for instance, and, indeed, a detailed discussion of Dupré's second premise recently developed by Witmer (2003) shows how problematic it is. I am thus afraid that the validity of the first premise does not help much to make a case for disunity as an 'inescapable' feature of science. A temporally qualified victory is still the best we can hope for.

6. Metaphysical Lessons Drawn from the Failure of Reductionism

I have critically discussed so far how metaphysical considerations are used as *grounds* for antireductionist arguments. Metaphysical considerations may also enter debates on reductionism as claims *derived* from the success or failure of reductions, and the fact is that today, antireductionist standpoints often go hand in hand with positive metaphysical claims about an alleged disorder of the world. To mention the two most prominent advocates of metaphysical disorder: Dupré (1993, 1996, 2002) talks about 'the disorder of things', by which he means the rejection of the following set of assumptions: 'a deterministic, fully law-governed, and potentially fully intelligible structure that pervades the material universe' (Dupré 1993, 2); Cartwright (1999, 2001) talks about a 'dappled world', namely a world that displays some features which are precisely ordered, whereas other features are unruly (Cartwright 1999, 10). I will focus on the main feature common to the metaphysical pictures of a disordered world proposed by Dupré and Cartwright, to wit, the thesis that not everything that happens is nomologically governed.

In Cartwright's scheme, the metaphysical picture of a dappled world is drawn from an analysis of the way science, and in particular physics, actually works. The punchline in her analysis is the contention that 'physics cannot account for everything that is in its domain' (Cartwright 2001, 210). There are 'messy' systems around us (Cartwright's favourite example is a dollar bill swept away by the wind) that are not nomologically governed (whatever the laws of physics may be). These unruly systems stand in contrast with what Cartwright calls 'nomological machines', that is, nomologically governed systems (such systems can be supplied by nature—like the solar system—or set up in the highly contrived environments of our laboratories) (Cartwright 1999, 49). Cartwright's strategy is thus to derive from the dismissal of the universality of laws—what she calls the failure of 'horizontal reductionism'—a metaphysical conclusion about parts of the world being unruly.

Dupré's name for metaphysical disorder is 'radical ontological pluralism' (Dupré 1993, 94). Radical ontological pluralism adds to the taxonomic thesis of 'promiscuous realism' discussed previously a thesis of causal egalitarianism stating the existence of 'genuinely causal entities at many different levels of organization' (Dupré 1993, 101).

A disorderly universe is then a universe in which one needs to appeal to all sorts of different, independent kinds of entities—each having equal causal status—to explain what is going on in this universe. Radical ontological pluralism goes hand in hand with an attack against the thesis of causal completeness: 'And [the existence of genuinely causal entities at many different levels of organization] is enough to show that causal completeness at one particular level is wholly incredible' (Dupré 1993, 101). Since a law is supposed to provide a causally complete account of a phenomenon (Dupré 1996, 111), Dupré's ontologically disordered universe is thus also a nomologically unruly universe.

As in Cartwright's scheme, the thesis of ontological disorder is inferred by Dupré from features of scientific practice: his strategy is to make a case for it on the grounds that classical reductionism fails. Note first that the very project of drawing ontological lessons from the failure of reductionism presupposes a Quinean take on ontology: if 'what there is' not determined in any sense by what scientific theories tell us, then obviously antireductionist views on science will not have any ontological import. Here is how Dupré presents his strategy:

> if reductive materialism [this is Dupré's terminology for classical reductionism *à la* Oppenheim and Putnam] were true—that is, if we could explain everything by referring only to physical entities—this [Quinean] conception of ontology would provide a clear sense in which only physical entities need be admitted to exist. An important corollary is that if this conception of ontology is accepted, and reductionism is shown to fail, we are immediately committed to a radical ontological pluralism. (Dupré 1993, 94)

One can immediately object that this is too hasty a conclusion, for Dupré seems to consider as an equivalence what is actually just an implication. If one adopts a Quinean take on ontology, the success of reductionism does entail ontological order, but this certainly does not imply that the failure of reductionism entails ontological disorder. This is a very simple logical point, but it admittedly leaves open that the failure of reductionism may still support metaphysical disorder via inference to the best explanation. A more interesting line of attack against Dupré's strategy is thus to examine what can actually be established by inference to the best explanation (IBE). Leaving aside general qualms about the very idea of using IBE to reach metaphysical conclusions, a compelling objection to Dupré's strategy is simply to note that the use of IBE to establish metaphysical claims of disorder on the basis of features of scientific practice is *inconclusive*. The point has been for instance cogently made by Davies: to the extent that we allow certain features of scientific practice (i.e. failures of reductionist approaches) to ground claims of metaphysical disorder, why not give equal credit to the (numerous) successful reductionist practices found in science as grounds for the metaphysical picture of an ordered world (Davies 1996, 9)?

Cartwright's strategy to infer from the failure of 'horizontal reductionism' the metaphysical picture of a dappled world lays itself to a very similar objection. As noted for instance by Lipton (2002), why not put modelling successes counting as evidence for nomological order on a par with modelling failures counting as evidence for nomological disorder? Here again, inferences to metaphysical claims based on features of scientific practice turn out to be inconclusive.

I will not sustain any further here this line of objection (not to mention other convincing lines of objection offered by Davies 1996, against Dupré's causal egalitarianism), for I would like to offer a different, perhaps more radical, reason to refrain from drawing metaphysical conclusions from the failure of reductionism. My objection is the following: the very notion of nomological disorder allegedly derived from features of scientific practice is *meaningless* as a *metaphysical* claim, that is, a claim about how the world is. I have developed this point at length elsewhere, specifically in response to Cartwright's picture of a 'dappled' world (Ruphy 2003). Let me briefly sum it up here, for it also bears on Dupré's strategy. By proposing a simple thought experiment, I have shown that Cartwright's division of the world into 'nomological machines' and 'messy' systems for which no law applies is meaningless as a metaphysical division, since being a nomological machine actually depends on what kind of questions one asks about it. The general moral of my story is that the very notion of orderliness must be relativized to the capacities and interests of knowers: predicates such as 'ordered' or 'unruly' *cannot* be applied to real-world systems independently of cognitive and practical expectations. This question relativity thus undermines *any* attempt to derive *metaphysical* lessons about nomological disorder from the failure of reductionism.

'One can extract only so much metaphysics from a physical theory as one puts in'. This is how the philosopher of physics, Lawrence Sklar, at the end of a paper entitled 'Time, Reality and Relativity', sums up, somewhat abruptly, his view on the possibility for a physical theory to refute or ground philosophical views (Sklar 1985). I am tempted to paraphrase this caveat to sum up my views on the relationship between antireductionist standpoints and metaphysics: 'one can extract only so much metaphysical disorder from antireductionist arguments as one puts in'; hence, my reiterated call for keeping metaphysical considerations at bay in the debate on reductionism. Let me try now, in the closing section, to clarify the scope of this ban on metaphysical activities I recommend.

7. Concluding Remarks

With the arguments developed in this paper I did not intend to reject metaphysical activities per se. What I am mainly concerned with is the compatibility of appeals to metaphysical considerations in antireductionist arguments with the pretension of these arguments to methodological imports. For the fact is that today, most antireductionist standpoints purport to bear on how scientific research should be conducted. Just to mention a few examples, Cartwright denounces quite vehemently the existence of 'imperialistic' methodologies grounded in reductionist views, which may have harmful social and epistemic consequences. Consider the takeover of superstring theory as the new candidate for the theory of everything: by monopolizing a significant part of the resources allocated to physics, claims Cartwright, string theory deprives us of breakthroughs that may have been achieved in other domains of physics, if sufficiently funded (Cartwright 1999, 16). Another example is the takeover of genetics as the dominant approach to try to cure disease like cancer (Cartwright 1999, 18). Hence, the importance for Cartwright to argue for a dappled world in order to get rid of these harmful methodological inclinations for generality and reductionist approaches. The title of one of Kitcher's articles (1999)—'The Hegemony of Molecular Biology'—speaks for itself. In a recent article on the drawbacks of a reductionist approach in evolutionary psychology, Dupré (2002) is also concerned by the takeover of reductionist scientific methodologies. But on which grounds may such methodological critiques be relevant for scientific practice?

Today, very few scientists defend classical reductionism in practice: a physicist studying how glue sticks (not to mention a biologist or an economist) usually does not expect string theory to solve their problems. Could it be so because they have heard of multiple realizability arguments so dear to philosophers or because they are taken by the metaphysical picture of a dappled world? It takes, I think, a very optimistic and idealistic philosopher to believe so. My hunch is that today, if a scientist chooses a reductionist approach to solve a specific problem or, on the contrary, develops concepts, experiments, and explanations without any reference to lower levels of organization, it is mainly because the favoured approach proves to be fruitful and empirically successful. In short, (creative) scientists are *opportunists*: they will be reductionist (or, for that matter, antireductionist) whenever it pays off. And philosophers should keep in mind that the fruitfulness of 'local' reductionist methodologies is an empirical, temporally qualified issue: reductionist methodologies might turn out to be epistemically defective in the present epistemic context of evolutionary psychology, as claimed by Dupré (2002), but they might turn out to be fruitful for certain problems in neuroscience or, say, in physics.[10]

Antireductionists such as Dupré or Cartwright urge scientists to give up reductionist methodologies. But, again, on which grounds should scientists do so? Because antireductionists claim that the metaphysical picture of an ordered world underlying these approaches is wrong (thereby eventually showing the same metaphysical hubris as reductionists believing in some sort of Book of Nature)? Or simply because giving up reductionist approaches might sometimes lead to better science. Given scientists' self-proclaimed disinterest for philosophy nowadays, the latter is clearly the more plausible.[11] Consequently, philosophical questionings of local scientific methodologies, when based on empirical considerations, are undoubtedly valuable for scientists, but they are much less valuable when based on metaphysical contentions. Antireductionists should be wary of indulging in imperialistic views on what is a good approach to solve scientific problems. General pleas for non-reductionist approaches based on the alleged disorder of the world or on metaphysical claims about its causal structure would just be the obverse of general calls for reduction, suffering from the same unfortunate lack of consideration of what actually works in science. If philosophers want their arguments to remain relevant for scientific practice, I am afraid that they must be more modest about the status of these arguments: only temporally qualified arguments that stay clear of metaphysical contentions can usefully bear on discussions of the merits and limits of reductionist approaches in science. Metaphysics might be alluring in many respects, but it does not mix well with a taste for methodological prescriptions.

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Notes

[1] I take the terminology 'temporally qualified' from Nagel (1961). I will return later to Nagel's use of the terminology.

[2] I leave aside 'local' antireductionist arguments, that is, arguments concerned with specific issues of reducibility in light of our current knowledge in a given field, for such arguments are clearly temporally qualified arguments. An example is Kincaid's (1990) discussion of a number of recent central results from molecular biology, showing their irreducibility to biochemistry, and Robinson's (1992) critique of it, based on a close look at what biochemistry actually offers today. In both cases, the arguments are grounded (or purport to be) in what happens to be our current state of knowledge in these disciplines.

[3] For a recent review of the differences between Nagel's account of reduction and other accounts of reduction as an explanatory strategy (such as Putnam and Oppenheim's), see Steel (2004). These differences do not matter here, since antireductionist arguments aim mainly at the minimal common core of reductionist claims just mentioned. Note also that the antireductionist standpoints discussed here do not respond to subsequent refinements of the classical concept of reduction, such as that proposed by Hooker (1981), or to the 'structuralist' approach of Nagelian reduction (see, for instance, Balzer, Moulines, and Sneed 1987, chap. 4, or Bickle 1998, chap. 3); nor do they deal specifically with the functionalist conception of reduction developed by Kim (1998, chap. 4).

[4] 'Natural kinds' simply refers here to objects or processes discriminated by a science. It need not be understood in an essentialist way or in the platonic sense of 'carving nature at its joints'.

[5] In a nutshell, the macroexplanation goes like this: imagine in a population at sexual maturity a departure from a sex ratio of 1:1—females, for instance, outnumber males. With males having more chance to mate, they will have more offspring than females. Parents genetically predisposed to produce more males will thus be favoured by natural selection, hence a return to equilibrium (i.e. to a sex ratio of 1:1).

[6] And indeed, Kitcher writes the following: 'Explanatory patterns that deploy the concepts of cytology will endure in our science because we would foreswear significant unification (or fail to employ relevant laws, or *fail to identify causally relevant properties*) by attempting to derive the conclusions to which they are applied using the vocabulary and reasoning patterns of molecular biology' (Kitcher 1984, 371; my italics).

[7] See, for instance, the ongoing debate between Fodor (1974, 1997) and Kim (1992, 1993, 1998) on the issue of genuine levels of causally relevant properties, and Davies (1996) on how Dupré's ontological thesis of 'equal causal status' (discussed later in this paper) is also undermined by Kim's argument on causal powers.

[8] For distinct, more radical lines of attack against the multiple realizability argument, see Sober (1999).

[9] The thesis is for instance defended by Kitcher (2001, chap. 4), with special attention paid to biology. Hacking (1999) also provides a nice illustration of the thesis in his chapter 'Kind-making: the case of child abuse'.

[10] See Bickle (2003) for case studies of successful reductionist approaches in neuroscience, such as the explanation of memory consolidation in terms of molecular mechanisms of long-term potentiation or sensory experiences induced by cortical microstimulation. Cases of successful reductions in physics are analysed, for instance, by Morrison (2000).

[11] '*The activities of the philosophers are simply irrelevant to my scientific life.* This view, or one very similar to it, is most certainly held by a vast majority of practising scientists in the English-speaking world' (Gale, 1984; italics in the original). This is how a paper published in 1984 in *Nature*, about the relationship between science and philosophy, begins. It is indeed usually admitted that the time when philosophical considerations were commonly invoked in scientific debate is gone. As interestingly noticed by Nickles (1987 536, n. 33), the drastic drop-off in scientists' interest in philosophy is, for instance, indicated by the change in the editorial board of *Philosophy of Science*. The original board of 1934 was composed almost entirely of leading scientists. Fifty years later, every member of the board was a card-carrying philosopher of science.

References

Balzer, W., C. U. Moulines, and J. D. Sneed. 1987. *An architectonic for science: The structuralist program*. Dordrecht: Reidel.
Bickle, J. 1998. *Psychoneural reduction*. Cambridge, MA: MIT Press.
Bickle, J. 2003. *Philosophy and neuroscience: A ruthlessly reductive account*. Dordrecht: Kluwer.
Cartwright, N. 1999. *The dappled world*. Cambridge: Cambridge University Press.
Cartwright, N. 2001. Against the completability of science. In *The proper ambition of science*, edited by M. W. F. Stone and J. Wolff. London: Routledge.
Davies, D. 1996. Explanatory disunities and the unity of science. *International Studies in the Philosophy of Science* 10: 5–21.
Dupré, J. 1993. *The disorder of things*. Cambridge, MA: Harvard University Press.
Dupré, J. 1996. Metaphysical disorder and scientific disunity. In *The disunity of science: Boundaries, contexts and power*, edited by P. Galison and D. J. Stump. Stanford, CA: Stanford Univeristy Press.
Dupré, J. 2002. The lure of the simplistic. *Philosophy of Science*, 69: S284–93.
Fodor, J. 1974. Special sciences. *Synthese* 28: 77–115.
Fodor, J. 1997. Special sciences: Still autonomous after all these years (a reply to Jaegwon Kim's 'Mutiple realization and the metaphysics of reduction'). *Philosophical Perspectives* 11: 149–63.
Gale, G. 1984. Science and the philosophers. *Nature* 312: 491–95.
Garfinkel, A. 1981. *Forms of explanation*. New Haven, CT: Yale University Press.
Hacking, I. 1999. *The social construction of what?* Cambridge, MA: Harvard University Press.
Hooker, C. A. 1981. Towards a general theory of reduction I, II, III. *Dialogue* 20: 38–59, 201–36, 496–529.
Kim, J. 1992. Multiple realization and the metaphysics of reduction. *Philosophy and Phenomenological Research* 52: 1–26.
Kim, J. 1993. *Supervenience and mind*. Cambridge: Cambridge University Press.
Kim, J. 1998. *Mind in a physical world*. Cambridge, MA: MIT Press.
Kincaid, H. 1990. Molecular biology and the unity of science. *Philosophy of Science* 57: 575–93.
Kitcher, P. 1984. 1953 and all that: A tale of two sciences. *Philosophical Review* 93: 335–73.
Kitcher, P. 1999. The hegemony of molecular biology. *Biology and Philosophy* 14: 195–210.
Kitcher, P. 2001. *Science, truth, and democracy*. Oxford: Oxford University Press.
Kripke, S. 1972. Naming and necessity. In *Semantics of natural language*, edited by D. Davidson and G. Harman. Dordrecht: Reidel.
Lipton, P. 2002. The reach of the law. *Philosophical Books* 43: 254–60.
McLaughlin, B. P. 1992. The rise and fall of British emergentism. In *Emergence or reduction*, edited by A. Berckerman, J. Kim and H. Flohr. New York: De Gruyter.
Morrison, M. 2000. *Unifying scientific theories*. Cambridge: Cambridge University Press.

Nagel, E. [1961] (1979. *The structure of science: Problems in the logic of scientific explanation.* Indianapolis, IN: Hackett.

Nickles, T. 1987. From natural philosophy to metaphilosophy of science. In *Kelvin's Baltimore lectures and modern theoretical physics*, edited by R. Kargon and P. Achinstein. Cambridge, MA: MIT Press.

Oppenheim, P. and H. Putnam. 1958. The unity of science as a working hypothesis. In *Minnesota studies in the philosophy of science*, vol. 2, edited by H. Feigl, M. Scriven and G. Maxwell. Minneapolis: University of Minnesota Press, pp. 3–36.

Putnam, H. 1975. *Mind, language, and reality. Philosophical papers, vol. 2.* Cambridge: Cambridge University Press.

Robinson, J. D. 1992. Discussion: Aims and achievements of the reductionist approach in biochemistry/molecular biology/cell biology: A response to Kincaid. *Philosophy of Science* 59: 465–70.

Ruphy, S. 2003. Is the world really 'dappled'? A response to Cartwright's charge against 'cross-wise reduction'. *Philosophy of Science* 70: 57–67.

Sklar, L. 1985. *Philosophy and spacetime physics.* Berkeley: University of California Press.

Sober, E. 1999. The multiple realizability argument against reductionism. *Philosophy of Science* 66: 542–64.

Steel, D. 2004. Can a reductionist be a pluralist? *Biology and Philosophy* 19: 55–73.

Witmer, D. G. 2003. Dupré's anti-essentialist objection to reductionism. *Philosophical Quaterly* 53: 181–200.