

DISCUSSION

Promiscuous Realism

Robert A. Wilson

ABSTRACT

This paper is a critical discussion of John Dupré's recent defence of promiscuous realism in Part 1 of his *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*. It also discusses some more general issues in the philosophy of biology and science. Dupré's chief strategy of argumentation appeals to debates within the philosophy of biology, all of which concern the nature of species. While the strategy is well motivated, I argue that Dupré's challenge to essentialist and unificationist views about natural kinds is not successful. One conclusion is that an integrative conception of species is a real alternative to Dupré's pluralism.

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1 Introduction

As part of a broad-ranging attack on a series of received views in the philosophy of science, John Dupré ([1993], Chs.1–3) has criticized essentialist and unificationist positions about natural kinds. Further articulating and defending an alternative to such positions that he introduced in the early 1980s, Dupré concerns himself largely with the biological sciences, arguing that his own form of realism—promiscuous realism—provides a view of natural kinds that better fits science as it is practised than do traditional realist views.

My critical aim in this paper is circumscribed. After briefly summarizing the sort of traditional realist view that Dupré rejects, I shall examine the arguments that Dupré provides against essentialism and unificationism

and for promiscuous realism. Since Dupré's chief arguments appeal to debates within the philosophy of biology, their examination requires a discussion of issues, such as the nature of species, that occupy a central place in that field. While agreeing with Dupré's strategy of argument, I argue that his challenge to essentialist and unificationist views about natural kinds is not successful. I shall suggest that an integrative conception of species remains a plausible alternative to Dupré's pluralism.

2 Traditional scientific realism

Realists about science hold distinctive views about scientific theories, theoretical entities, and scientific knowledge: theories in at least mature sciences are typically and for the most part true; the theoretical terms those theories contain typically refer to entities that exist independent of us as observers and conceivers, even if those entities are unobservable; and scientific knowledge is to a large extent cumulative over time because the above views of theories and entities hold. There is, of course, more to realism—including views about explanation and laws of nature—but for my purposes it will suffice to augment the above sketch of scientific realism with two aspects of the realist's view of natural kinds.

First, the traditional scientific realist holds that natural kinds are individuated by *essences*, where the essence of a given kind is a set of intrinsic properties, each necessary and together sufficient for an entity's being a member of that kind. Essences might not themselves be observable, but for a realist observability does not erect any sort of metaphysical boundary. Science discovers essences; this explains its theoretical and practical successes.

Second, the traditional scientific realist holds that natural kinds are *unified* in some way. Reductionists have provided a cluster of views about this unity: it consists in the reducibility of theories, concepts, kinds, or laws in a science cast at the n -th level of description to those in a science pitched at the $(n - 1)$ th level of description, with all sciences ultimately being reducible, in the appropriate sense, to physics. This view of scientific taxonomy is *hierarchical* in its view of the structure of the various sciences, and *bottom-up* in its basic metaphysical commitments. Non-reductionist versions of traditional realism share a view of scientific taxonomy with these two properties, though they are stated in terms of notions such as composition and realization, rather than reduction and identity. It is less clear that non-reductionists are *committed* to a unificationist view of natural kinds, but such a position has, I think, been assumed by many realists. (If promiscuous realism is in fact a non-reductionist

version of realism, as it purports to be, then unificationism about natural kinds is a gratuitous association of realism.)

3 A statement of promiscuous realism

Dupré thinks that the traditional realist views of natural kinds and scientific taxonomy are mistaken. He distinguishes two types of pluralism about science, both of which he advocates. Of the first, Dupré says, 'in opposition to an essentialist doctrine of natural kinds, pluralism [is] the claim that there are many equally legitimate ways of dividing the world into kinds, a doctrine I refer to as "promiscuous realism" [PR]' (pp. 6–7). Dupré says '[m]y thesis is that there are countless legitimate, objectively grounded ways of classifying objects in the world. And these may often cross-classify one another in indefinitely complex ways' (p. 18), taking PR to involve a 'metaphysics of radical ontological pluralism' (p. 18).

PR thus makes at least two claims, the first about the criteria for membership in a given natural kind (entailing the rejection of essentialism about natural kinds), the second about how natural kinds are related to one another (entailing the rejection of a unificationist view of natural kinds). PR could be expressed negatively as the conjunction of the following claims:

- (a) There is no *one* criterion for membership in a given natural kind, i.e. that provided by the essence of the kind.
- (b) There is no *one* way of ordering the natural kinds that there are in the world so that they constitute a unity.

Traditional realists make existential claims that (a) and (b) deny. As the emphasis on 'one' in each of (a) and (b) is meant to highlight, (a) and (b) deny, respectively, the *uniqueness* of criteria for natural kind membership and the ordering of natural kinds. As such, they constitute part of a pluralistic version of realism about natural kinds.

Note that while (a) is the contradictory of essentialism about natural kind membership, and (b) the contradictory of unificationism about the overall organization of natural kinds, these do not exhaust the possible realist views that one might hold about natural kinds. Traditional essentialism and unificationism (at least in its reductionist guise), and promiscuous realism are contrary but not contradictory *realist* views of natural kinds.

We can see this more clearly by supplementing these negative characterizations with a positive formulation of PR that brings out its rationale more explicitly. In summarizing his rejection of essentialism about biological kinds, Dupré says:

There is no God-given, unique way to classify the innumerable and diverse products of the evolutionary process. There are many plausible and defensible ways of doing so, and the best way of doing so will depend on both the purposes of the classification and the peculiarities of the organisms in question, whether those purposes belong to what is traditionally considered part of science or part of ordinary life (p. 57).

This suggests that PR is the view that there are many objective, equally legitimate ways of dividing the world into kinds in part *because* each of these ways best satisfies a different legitimate interest or purpose. For this to count as a realist view, a legitimate interest or purpose must be one that allows us to discern kinds that exist in nature. Realists may accept (a) and (b) without endorsing this aspect of Dupré's view.

Dupré offers two sorts of arguments for PR. The first appeals to common sense categories and their relationship to taxonomies in biology; the second appeals more directly to biological taxonomy. I shall take them in turn.

4 Promiscuous realism and common sense

As a precursor to his first argument for PR, Dupré (p. 19) points to the ontological profligacy of common sense categories of biological entities. Dupré rightly points out that the concern of common sense is not to arrive at a unified picture of the world but, rather, to gather more information about it. Thus it should be no surprise that common sense categories for natural objects, such as organisms, are many and messy. The central conclusion of Dupré's argument (pp. 21–36) is that these common-sense categories are not refined by the corresponding taxonomies of natural science as Putnam [1975] claimed in his theory of natural kind terms. The precursor to the argument indicates that there is a plurality of natural kinds *within* common sense; the argument itself concludes that there is a plurality of natural kinds *between* common sense and science.

Pivotal in Dupré's argument is the claim that biological kinds often cross-classify objects as they are categorized by common sense. When we common folk classify things together as (say) trees, biological science classifies things very differently. To take one of Dupré's examples (p. 28), while we common folk use the term 'lily' to refer to certain kinds of flowers, these belong to many different genera, each of which also includes many things (such as garlics and onions) that we would not count as lilies. Common sense and biological science provide us with pluralistic ways of taxonomizing biological reality. Dupré makes a convincing case that such apparent disagreements between common sense and

science are many and varied: they include entities as different as cacti, butterflies, and hawks.

Just how does this claim support the negative and positive formulations of PR given in the previous section? It supports (a) only if ordinary language supplies a criterion for membership in a given natural kind that provides an alternative to that supplied by science; likewise, it supports (b) only if ordinary language gives us an alternative way of ordering natural kinds to that provided by science. Thus, in order to offer support for at least the negative characterizations of PR, Dupré's claim needs to be joined with the assumption that common sense and science have very much the same sort of roles to play in an account of natural kinds and so provide alternative and *prima facie* competing criteria for natural kind membership and orderings of natural kinds. This assumption is also manifest in the positive characterization we gave of PR. For common sense and science to provide *equally legitimate* ways of dividing the world into natural kinds that best satisfy different legitimate interests or purposes, they must at least each divide the world into natural kinds.

My claim is that common sense, and thus ordinary language, does not try to do this: they are not in the business of individuating natural kinds at all. This claim is, I think, implicit in the precursory claim to Dupré's argument mentioned above—the claim that common sense is profligate in its ontological commitments—for the reason that it can be profligate is that, unlike science, it is not primarily concerned to uncover or order natural kinds. (It is perhaps a mistake to think that common sense and the ordinary language that flows from it have well-defined purposes at all.) The fact that ordinary language lacks the purposes that drive scientific taxonomy, a fact that Dupré himself is keen to emphasize, supports rather than undermines essentialism and unificationism, for it is only the purposes of scientific taxonomy that could be achieved by finding the relevant types of essences.

This aside, recall that the chief target of Dupré's broader argument is Putnam's essentialist view of natural kinds. On Putnam's view, science specifies the essences of substances whose nominal essences are provided by (enlightened?) common sense. Dupré argues that this view is mistaken, as follows:

1. Sciences specify the extensions of natural kind terms by identifying the real essences for kinds whose nominal essence is supplied by common sense (Putnam's claim).
2. The nominal essence of 'lily' implies that A, B, C, and D fall in the extension of 'lily'.
3. A, B, C, and D do not fall in any one biological category; in fact, there

is not even a best fit biological category corresponding to the common sense term 'lily'.

4. Thus biology does not specify a real essence of lilies that corresponds to the nominal essence supplied by common sense.
5. What is true of lilies is true of a range of categories that, *prima facie*, are natural kinds.
6. Putnam's essentialist view of natural kind terms, which includes Premise 1 above, is mistaken.

Here I want simply to grant the soundness of the argument and ask what this implies about essentialist and pluralist views of natural kinds more generally.

Putnam's view includes both a semantic account of natural kind terms and a traditional realist view of the nature of natural kinds. Clearly, the above argument is directed primarily at the semantic aspect of Putnam's view; in particular, it is directed against the claim that natural kind terms have a structure to them that shows how their common sense and scientific uses are integrated. To show that this aspect of Putnam's view is mistaken may be significant for semantics, but the importance of this conclusion for essentialism and unificationism about natural kinds more generally is less clear.

Even an essentialist who wished to defend a Putnamesque view about natural kind terms could adopt this sort of view, taking Dupré's argument as showing only that common sense does not do a perfect job of specifying the nominal essences of natural kind terms. Natural kinds may still have essences in the traditional sense (intrinsic, physical properties shared by all members of the kind). It is just that ordinary language provides, at best, a rough and ready guide to which terms in a language *are* natural kind terms, one whose reliability varies from area to area. To put this point in terms of the notion of reference-fixing, whether a given ordinary language use of a term does fix the reference of a natural kind term is itself an a posteriori matter, one determined by how the corresponding science develops. Just as putative proper names can sometimes fail even to fix reference to an individual, so too can putative natural kind terms sometimes fail even to fix reference to a natural kind.

5 Pluralism and biological taxonomy

The second type of argument for PR that Dupré offers is, I think, stronger. Dupré himself (p. 36) sees this type of argument as an extension of the previous argument: pluralism about natural kinds is supported not only by reflection on differences between common sense and scientific taxonomies,

but also by an examination of the sciences, particularly the biological sciences, themselves. Dupré's argument here appeals directly to taxonomic practice in biology, and versions of it have been advocated and endorsed in broad outline by Philip Kitcher [1984, 1989] and Marc Ereshefsky [1995].

Dupré focuses on three debates in the philosophy of biology about the nature of species: whether species are individuals or natural kinds; what criteria should be used for species membership; and whether species have essences (p. 38). Dupré argues that each of these debates provides evidence for the truth of PR. Of the first of these issues, Dupré claims that there is no one answer to what species really are, only answers relative to different explanatory enterprises within the biological sciences; thus, this claim supports the positive formulation of PR specified above. Dupré draws a similar conclusion from his discussion of the second issue: there is no single criterion for species membership that serves all the purposes that the concept of species is used for within the biological sciences, and so pluralism about species membership is the most plausible view to adopt. On the third issue Dupré's position is somewhat less clear, since although at one point he concedes that 'a truly tolerant pluralism should surely allow the occasional appearance even of a plausible candidate for a real essence' (p. 55), the tenor and substance of his argument here (pp. 53–9) suggests a rejection of the view that species have essences. These second and third conclusions also support (a) and (b), the negative formulations of PR that traditional realists must reject.

I think that these arguments for PR are of mixed value. Specifically, I shall argue that because the first argument is actually irrelevant to pluralism about natural kinds, it is of limited importance for PR; and that the conclusions to be drawn from the second and third arguments (which I shall treat together) are less clear than Dupré thinks.

6 The irrelevance of the debate over the ontological status of species

Traditionally, species have been considered paradigm examples of natural kinds. This view was forcefully challenged by Ghiselin [1974] and Hull [1976, 1978], whose view of species as individuals has won a steady stream of converts over the last twenty years. Dupré is not persuaded that the view of species as individuals completely usurps the traditional view: we need both views. He says, 'to the extent that we take theoretical embedding as the correct way to consider the question of the ontological status of species, we are driven to a pluralistic answer: in some contexts species are treated as individuals, in others as kinds' (p. 43).

A natural way to express Dupré's pluralistic conclusion from what he

sees as a deadlock in this debate would be to say that species are both individuals and kinds. In summarizing his discussion, Dupré recognizes that there would be something strange in stating his conclusion in this way, since individuals and kinds belong to fundamentally different ontological categories—as fundamentally different as individual and abstract object, or substance and property. So Dupré states his conclusion as follows: '[t]he real question is whether the same set of individuals can provide both the extension of a kind and the constituent parts of a larger individual. And the answer to this is clearly yes . . . ' (p. 58).

This affirmative answer, however, does not state a view about the ontological status of species, but, rather, a view of the ontological status of *individual organisms*: they can both be members of a kind (a species) and parts of some larger individual (a species). This would express a view about the ontological status of species were we to make explicit that the 'kind' referred to above is a species, the very same species to which 'larger individual' refers. But that would bring us back to the puzzling and confused expression of pluralism from the last paragraph which Dupré himself rejects.

I think that the only resolution of this issue is ineliminably pragmatic. There are two ways in which the term 'species' is used in the biological sciences, ways that lead to confusion when they are run together. One way, the traditional way, is to treat 'species' as referring to natural kinds; species, then, are the sort of things that feature in biological generalizations and laws. The other way is to treat 'species' as referring to individuals themselves constituted by organisms; on this view, species are the sort of thing that can come into existence, change, grow, and disappear. We could say that species are individuals, and then explain the other use of 'species' which treats species as natural kinds; conversely, we could take species to be natural kinds, and explain the use of species terms as designating individuals. Note the confusion that arises when we attempt to combine the two views. If we say that species are individuals whose parts are individual organisms that are members of natural kinds (species)—or, alternatively, that species are natural kinds whose members, individual organisms, also collectively constitute an individual (species)—we lapse into the absurdity of saying that one and the same thing is both a natural kind and an individual. The pragmatic response I am suggesting does not make the pluralistic metaphysical commitments that would support PR.

It is, in any case, difficult to see how a metaphysical deadlock in this debate constitutes an argument for PR, which is, recall, the view that 'there are many equally legitimate ways of dividing the world into kinds' (p. 6). If indeed these two ontological views of species *are* 'equally legitimate', then we have one way of dividing a part of the biological world *into kinds*, and

another 'equally legitimate' way of dividing it *into individuals*. This is not a form of pluralism about natural kinds at all. Support for PR here, then, is at best indirect: it lies only in whatever similarities exist between the ontological pluralism that Dupré argues for about the ontological status of species and PR proper.

7 The species problem

Dupré sees irremediable weaknesses in the three major criteria for species membership—morphological, biological (or reproductive), and phylogenetic (or genealogical)—and argues that each is appropriate for individuating species for certain biological purposes but not others. Thus, there is no one criterion that can serve as a species essence; rather, there are different criteria, depending on just what areas of biology (palaeontology, microbiology, ecology) one is working in and what types of organisms one is concerned with.

Unlike the issue of the ontological status of species, the debate over the individuation of species ties directly to issues separating essentialists from pluralists about natural kinds. It appears to support both aspects of the negative characterization of PR. That there is no one criterion for species membership seems to be what the debate shows, and given this, there is no one way of ordering or organizing species. Furthermore, the resulting pluralism seems to derive from the different theoretical contexts in which the concept of species has been developed and employed. Despite appearances, I want to suggest that matters are not so straight forward.

One reason is that despite (or perhaps because of?) the attention that the species problem has received from both biologists and philosophers over the last twenty-five years, there are complexities to that problem that remain unwoven, and unclarity in the corresponding debate that need to be removed. These indicate that much conceptual work remains to be done on the species problem, and give reason for being cautious in the metaphysical conclusions we draw from reflection on it. Let me mention just one complexity and one unclarity to indicate the sort of problem I have in mind.

When biologists articulate their various criteria (morphological, reproductive, genealogical) in response to the species problem, they are doing at least two different things. They are both offering a criterion for species membership and a criterion for marking off species from other groups of organisms, whether they be mere varieties below, or genera or classes above the species level. These two dimensions to the species problem interact with other issues concerning biological taxonomy, such as that of the ontological status of species, the reality of taxa (especially higher-level taxa) other than species, and the role of convention in biological

taxonomy more generally. The different responses to the species problem that have been provided themselves reflect different emphases within this cluster of issues. For example, pheneticists, who focus exclusively on morphological criteria for species membership, rate empiricist virtues, such as observability and simplicity, highly and tend towards a nominalist view of biological kinds (including species). Cladists, by contrast, in opting for phylogenetic criteria, assume that any adequate response to the species problem must make patterns of descent explicit. Differences in responses to the species problem are very much parts of broader philosophical and scientific proclivities.

This might seem to be grist to Dupré's mill: PR is the version of realism that makes the most sense of these disagreements, since it is the position that, if you like, reads off its philosophical conclusions directly from scientific practice. By contrast, I see the complexity of the species problem itself as a reason for caution, especially given that, as Dupré's own discussion (pp. 44–9, 53–7) makes clear, there are inadequacies in each of the approaches to the species problem. Not all of these inadequacies can be removed simply by going pluralistic. Our epistemic situation now is most like that within late sixteenth-century astronomy where much integrative conceptual and empirical work lay in the future and the endorsement of a pluralistic realism would have been at best premature.

The unclarity concerns how the various responses to the species problem should be categorized. While Dupré's trichotomy of morphological, reproductive, and phylogenetic criteria for species membership is standard, the relationships between these and which positions they encompass have not been clearly articulated. For example, some phylogenetic proposals (e.g. Paterson [1985]; Templeton [1989]) are intended to incorporate or modify reproductive criteria, and their classification as proposals of either one or the other type is problematic. Each type of proposal appears to be exclusive of the other two. But morphology, reproductive behaviour, and genealogy are not completely independent aspects of biological reality, and the apparent mutual exclusivity of the criteria is likely to derive from the incompleteness of each criterion. Moreover, the extent to which various proposals *agree* about what groups of organisms count as species has, surprisingly, received little discussion.

There may be ways to classify responses to the species problem that make the prospects for integration more promising. The various 'evolutionary' criteria, for example, can be divided into *process-* and *product-*oriented criteria. The former (e.g. Mayr [1970]; Ehrlich and Raven [1969]; Paterson [1985]; Templeton [1989]; van Valen [1976]) offer criteria of specieshood that focus on the processes that maintain species as distinct biological groups (geographic isolation, cohesion, interbreeding, adaptive

zones, natural selection); the latter (e.g. Simpson [1961]; Wiley [1978]; Cracraft [1983]; Mishler and Donoghue [1982]) focus on species as the product of evolutionary processes, and so emphasize the relationships that hold between these historical entities. This way of carving up responses to the species problem makes perspicuous two points: first, that at least some 'reproductive' and some 'phylogenetic' responses share potential for integration, since they both focus on processes leading to reproductive isolation; second, that the concept of species may in fact be doing double-duty. This second point suggests a way of diffusing the appearance of disunity about species, for there may be two concepts in play here. The plausibility of this view of responses to the species problem, and whether it does provide beginnings for an integrated conception of species, are the sorts of issues that require further development and discussion.

This is not to say that reproductive and genealogical criteria do not disagree (they do), but to suggest that there is still further conceptual work to be done before giving up on an integrative approach to the species problem.

8 Integration vs pluralism

Reflection on the standard objections that Dupré recounts to each of these views of species also suggests an integrative rather than a pluralistic approach to the species problem. In particular, I think that the standard objections to the so-called biological species concept (BSC)—the reproductive view of species—are not conclusive grounds for simply rejecting the BSC. Rather, they suggest that the BSC needs to become more explicitly genealogical if it is to serve as a general conception of species.

One such objection is that the BSC, which defines species as 'groups of interbreeding natural populations that are reproductively isolated from other such groups' (Mayr [1970], p.12), does not apply to populations of asexually reproducing organisms because individuals in those populations do not interbreed. True enough. But reproductive isolation and interbreeding are different types of properties: reproductive isolation is a property of an intergenerational *population* of organisms, while interbreeding is a property of pairs (or more generally, n-tuples) of individual organisms.¹ If we view the BSC as focusing on reproductive isolation, with interbreeding being an important way of establishing and maintaining

¹ Dupré himself overlooks this point when he writes that 'it [the BSC] has no application to asexual organisms, every one of which is isolated from every other' (p.46). Since the BSC predicates reproductive isolation of the population, not of the individuals composing that population, this seems to me both to mischaracterize the BSC and to represent a misunderstanding of the objection to it, a misunderstanding that is perhaps widespread.

such isolation in sexually reproducing populations, then there appears to be a way of broadening the BSC so that it applies naturally to both sexually and asexually reproducing populations. Species are reproductively isolated populations in that individual organisms in that population reproduce (sexually or asexually) other organisms of the same natural kind, not other organisms of different kinds. (Note that there is no circularity here since I am not offering criteria for species membership but explaining the sense in which species are reproductively isolated populations.) This change in emphasis in the BSC makes it applicable to a cloneline, i.e. to generations of asexually reproducing organisms descended from one such organism; a further genealogical modification is required if the BSC is to apply to a series of clonelines whose members, intuitively at least, are members of the same asexual species.

Playing down the emphasis on interbreeding and making the BSC more explicitly genealogical would also provide a way of responding to the objection that the BSC is licentious in that it classifies so-called multispecies and other putatively distinct but interbreeding groups of organisms as single species. The species that comprise a multispecies remain reproductively isolated over time despite interbreeding and genetic exchange between their members. This description of the phenomena requires a genealogical rather than a 'biological' understanding of reproductive isolation.

Part of the suggestion here is that at least the reproductive and genealogical conceptions of species should be developed integrally rather than competitively; perhaps all three types of criteria should be integrated. It is difficult, however, to see how such an integrative conception of species could be developed within the confines of the standard, one-criterion answer to the species question. But this seems to me a limitation of that model rather than a problem with the suggestion, and indicates one way in which traditional essentialist views have been straight-jacketed by a narrow conception of scientific definition. An alternative to that model that I find promising and will simply mention here is Richard Boyd's [1988, 1991] homeostatic property cluster view of natural kinds.

9 Interdisciplinary and intradisciplinary pluralism

Finally, recall that PR is not simply the view that there are many natural kinds—from quarks to lions to stars to elements—each the subject of a particular science but no two of which give us different, competing ways of carving up the one reality. Were PR this view, then one could simply point to the myriad kinds that exist across the sciences to support it; PR would hardly be a view that challenges traditional scientific realism. PR

goes further than recognizing *interdisciplinary* cross-categorization: it recognizes *intradisciplinary* cross-categorization. Hence the appeal to the biological sciences in particular.

But here we face a question: are 'the biological sciences' a discipline? Are they any more of a discipline than, say, the social sciences or the physical sciences? If the answers to these questions are negative, then the lack of agreement about the criteria for species membership is not as radical as it appears, for it is more like the 'disagreement' one finds across disciplines—such as that between physics and economics about whether quarks or self-interested agents exist.

There is more than semantic fancy footwork here. If we look at the areas within the biological sciences for which each of the species criteria seems most appropriate, we find that the morphological criteria is championed in botany and in microbiology (especially where there is asexual reproduction), while reproductive and genealogical criteria have widespread application in zoology. Given this, what we have are, as Kim Sterelny ([1994], p.18; cf. Williams [1992]) has pointed out, *different* individuals taxonomized by different criteria, but both being called 'species', rather than the same individuals being differentially taxonomized. This does not imply a pluralism of species concepts that would challenge essentialism and unificationism about natural kinds; rather, it suggests the heterogeneity of the species category and a corresponding division of labour between those investigating the biological world.

As a response to the problem that asexual reproducing species poses for the traditional BSC, this option is somewhat more radical than the integrative alternative that I suggested earlier. But both provide us with some conceptual space between the denial of essentialism about biological kinds, at least of a traditional kind, and acceptance of promiscuous realism.

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*Department of Philosophy
313 Watson Hall
Queen's University
Kingston, Ontario
Canada K7L 3N6*

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