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Author(s): John Dupré

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DISCUSSION

Promiscuous Realism:
Reply to Wilson

John Dupré

ABSTRACT

This paper presents a brief response to Robert A. Wilson's critical discussion of Promiscuous Realism [1996]. I argue that, although convergence on a unique conception of species cannot be ruled out, the evidence against such an outcome is stronger than Wilson allows. In addition, given the failure of biological science to come up with a unique and privileged set of biological kinds, the relevance of the various overlapping kinds of ordinary language to the metaphysics of biological kinds is greater than Wilson admits.

I am sympathetic to much of what Wilson says in his comments on promiscuous realism. The alternatives he offers to promiscuous realism as an account of biological kinds make serious efforts to come to terms with the difficulties I and others have pointed out with standard monistic solutions. I agree particularly with Wilson's insistence that there are many scientific and philosophical problems still to be addressed concerning the nature of species, and the attractiveness of various general views of species may change fundamentally as further insight is obtained. Certainly I do not take myself to have arguments that can refute a priori the possibility that future scientific developments might make a monistic, even essentialist, view of species increasingly attractive. Although I acknowledge that it is hazardous to read a philosophical position off the current state of the science, I do, nevertheless, think that Wilson overestimates the likelihood of convergence on an integrated and monistic account of species. Here I shall make some brief remarks in support of this claim, and then conclude with a comment on the relation of this debate to the issues about common-sense taxonomy that Wilson also discusses.

One central point in Wilson's suggested monistic account of species is the proposal to integrate genealogical accounts of species with accounts in terms of reproductive isolation. To this end Wilson proposes as an explication of the account of species in terms of reproductive isolation that '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' (p. 12). He insists that this is not circular since it is merely an explication of the sense in which species are reproductively isolated. Clearly if it is to avoid circularity this explication cannot coexist with reproductive isolation as a criterion of species membership, so it must be seen as a subordination of this criterion to some genealogical criterion. Even then, serious problems remain. As an empirical matter, many lineages are not wholly reproductively isolated from one another. That is to say, members of different lineages interbreed. Offspring from such interbreedings cannot belong to both parental kinds unless an extremely radical version of pluralism is embraced. It might be possible to address this problem by making the lineage more and more inclusive until all interbreeding groups had been assimilated. But that would surely lead to groups so morphologically diverse as to be useless for many biological purposes. Thus reproductive isolation in the sense explicated by Wilson is not easily grafted on to a phylogenetic concept of species. (Wilson apparently has something more complex in mind in referring to reproductive isolation, since he sees it as compatible with genetic exchange between members of different groups (p. 12). I cannot see how, with genetic exchange, both sexual parents can be reproducing their distinct natural kinds.)

The fact of genetic exchange makes clear that an adequate elaboration of Wilson's account of reproductive isolation requires that more be said about what constitutes being a member of the same natural kind. That is to say, when we wish to recognize a taxonomic distinction between lineages despite genetic exchange across the boundary we are drawing, we need a criterion for allocating individual organisms that are the vehicles of this genetic exchange to one side or other of the distinction. And I cannot see how this can be anything but the stipulation of, broadly speaking, morphological features. It is, therefore, not surprising that among groups of organisms in which gene flow across species is common, most notably in botany and microbiology, morphological conceptions of species remain dominant. In these domains genealogy, with lineages distinguished by reproductive isolation, does not provide a sufficient number of sharp divisions for normal biological purposes and it must, at the least, be supplemented with something that is, from a genealogical point of view, arbitrary.

This leads naturally to a possibility Wilson introduces intermediate between fully essentialist integration and promiscuous realism, interdisciplinary pluralism. It might turn out that different areas of biology required different taxonomic principles, but this would not imply that the appropriate taxonomic principle was underdetermined for any particular group of organisms. I certainly agree with Wilson that there is nothing

divinely ordained about the scope of some homogeneous science of biology. Microbiology and ornithology, for instance, might be very different sciences. I do not think this proposal can block promiscuity, however. Very different kinds of organisms interact with one another, and the science of ecology studies some of those interactions. If different sub-disciplines of biology use different taxonomic principles then ecology must depend, promiscuously, on all of these. Whether or not there are good reasons for treating some biological subspecialties in isolation for taxonomic purposes, there are other parts of biology in which they must be brought together.

In summary so far, though I acknowledge the underdeveloped state of some central issues in biological systematics, and I certainly do not want to prejudice future developments of these issues, promiscuous realism seems to me more than just a snapshot of the current state of scientific development. Although there are most certainly connections between evolutionary, ecological, and morphological phenomena, there are no good reasons for expecting them to map on to identical taxa. Perhaps if genetic isolation had been more prevalent there would have been stronger reasons. Perhaps if biological organisms formed kinds defined by the simple structural real essences generally held to characterize chemical kinds, such convergence might have been anticipated. But there are strong grounds for denying both of these contingencies: in the first case there is much direct empirical evidence; in the second case there is no room for such essences in anything close to our current understanding of biology.

The most general point underlying all of this is that evolution, the source of biological diversity, is itself a diverse set of processes. There is no reason to expect that it will give rise to any unique and privileged set of categories suited to the varied sorts of enquiries and interests that we bring to the study of biological organisms. So although I readily concede that it is hazardous to derive an account of taxonomy from a particular stage in the development of biological science, I argue that taxonomic pluralism is grounded in fundamental aspects of our understanding of evolution. In the absence of some extra-scientific argument that there must nevertheless be some privileged monistic taxonomy to be discovered, the anticipation of such a discovery seems to me unmotivated and improbable.

Let me conclude with common sense. I agree with Wilson that science and common sense might have worked quite differently, and the absence of convergence between scientific and ordinary language kinds could, under different circumstances, even be taken as evidence for this divergence of aims or methods. This would, I suppose, be the right conclusion to have drawn if science had converged on a unique taxonomy defined by a hierarchy of nested real essences. But this did not happen. If promiscuous

realism is true for scientific biology, then there is no reason for privileging some set of biological categories over well-grounded common-sense categories. Common-sense categories are generally based on a perhaps rather narrow set of morphological criteria. The divergence between science and common sense suggests that focusing on a particular set of morphological criteria will produce an eccentric taxonomy. But since any morphological taxonomy must focus on a limited set of criteria this, in turn, suggests that there are a variety of possible, more or less eccentric, morphological taxonomies. In summary, then, the argument from common-sense biological categories would have no force by itself against an essentialist view of biological science. But to the extent that there are independent reasons for rejecting scientific essentialism, arguments for depreciating the categories of common-sense are undermined. When these categories are taken seriously they nicely illustrate the reasons for doubting the existence of a privileged set of scientific categories, and contribute significantly to the elaboration and illustration of promiscuous realism.

*Department of Philosophy
Stanford University
Stanford, CA 94305
USA*

Reference

Wilson, R.A. [1996]: 'Promiscuous Realism', *British Journal for the Philosophy of Science*, **47**, pp. 303–316.