Reply to Boyd on "Historical Kinds and the 'Special Sciences'"

Boyd

Concerning Boyd 2.0, I think that in one way there is less and in another way perhaps more disagreement between us than he suggests. I did not intend to endorse the notion, which I believe he rightly rejects, that psychological properties are defined by "analytic or otherwise a priori" "programmatic definitions," although I did wish to argue that if they were so defined, then a universal science of psychology could only be of an a priori kind. My own view is, first, that the notion of definition, whether nominal, real or "programmatic," does not rightly apply to natural kind terms at all for much the same reasons it does not apply to names of individuals and, second, that psychological properties are not functionalist properties characterized by "very abstractly formulated causal or explanatory roles" but instead are characterized by their "proper-functions" in the sense of (Millikan 1984). On the first point, my view is that natural kind terms refer directly, that is, in a manner psychologically unmediated by any definition at all (the best statement is probably in Millikan 1998a and 1998b, but see also 1994) even, surprisingly, when they are of the sort usually characterized as deeply theoretical (Millikan forthcoming). On the second point, proper-function categories are like functionalist categories in that they are multiply realizable, but they are very unlike functionalist categories in that they are not, in general, defined through a network of causal relations to items in other proper function categories. Thus proper function categories do

not, in general, pick out natural kinds. And the various natural kinds that possess psychological properties, over each of which a separate set of psychological generalizations must be drawn, obviously do not correspond to proper function categories. Beliefs that it is raining, like hearts, are as such members of proper function categories not members of natural kinds; dogs and people are members of natural kinds not members of proper function categories.

Boyd's 2.1 makes many good observations about species and also helps me, at least, to understand his interesting notion of homeostatic property cluster kinds better. Its bearing on my own views is somewhat problematic, largely because I reject the basic idea that either natural kinds or natural kind terms have "definitions" of any sort. I will make just a few scattered observations that may help to clarify what my position actually is.

Natural kind terms do not have definitions, but they do have "ontological grounds," that is, one or another kind of glue holds them together making it be the case that properties exhibited by one member of the kind are always or often exhibited also by other members so that induction is supported. A kind is a natural kind when there is a univocal principle, the very same principle throughout, that explains for each pair of members, why they are alike in a number of respects. That is, the principle explains the likeness between members, not, in the first instance, the properties themselves. (To explain why a photocopy is like the original is not to explain why either has the properties it has. I can know why the

photocopy is like the original without knowing what properties either has.) A given individual may be a member of a number of natural kinds at once. Some of these may be inclusive of others. Some of these may have one kind of ground and some another, or all may have the same kind of ground.

I made no claims about the possible number of different types of grounds there may be for natural kinds, but drew a distinction between historical grounds and eternal grounds. The former link one member of the kind to another by some sort of causal connections among them or by causal or historical relations to the same historical individuals or same historical setting. Members of different biological lineages derived on separate occasions by crossing members from the same pair of species are clearly examples of kinds having historical grounds. Eternal grounds, on the other hand, make no reference to historical relations between kind members but only to certain properties had in common from which other common eternal properties necessarily flow. I did not intend to exclude relational properties from the kinds of properties members of eternal kinds might share, but only to exclude relations to particulars (Boyd 2.2). ("Intrinsic" does mean non-relational, to be sure; my apologies!) But the categories "historical kind" and "eternal kind" were meant to be defined such that all and only kinds that have <u>no</u> <u>historical</u> <u>ground</u> <u>at</u> <u>all</u> are "eternal", thus making these categories mutually exclusive.

I did not intend any claims about which of the various sorts of natural kinds that an individual animal might fall

under should be recognized by biologists as "species." As Boyd articulates this question it seems to be a question not so much about what various natural kinds of animals there are as about whether there is a good way of making out the kind "species" as itself being a natural kind. I also did not intend any claims about the nature of "homeostatic property cluster kinds" in Boyd's sense. I meant to use this notion of his rather as he says he meant to use Mayr's notion of species. The particular kind of homeostasis I had in mind was homeostasis over time in the gene pool of a sexually reproducing species caused by the very stringent requirement that a new gene entering such a gene pool can persist only if compatible with any of a great variety of combinations of genes already there. The sort of kind produced by genetic reproduction plus this sort of homeostasis is clearly a historical kind. (That there are not exceptionless generalizations over the members of such natural kinds is not due to bad copying but to heterogeneity in the gene pool and heterogeneity of environmental influences for different members of the kind.) Boyd rightly points out that there may be many other examples of his homeostatic property cluster kinds that have only eternal grounds.

I am in full agreement with Boyd's remarks on the tenuous relations between possible worlds and science, and I much admire his section 3.2 on replacement stability and the variety of levels and kinds of natural kinds. It is relevant, however, that as he describes replacement stability it seems an essentially historical notion, one that will underwrite only kinds that ride on historical grounds.

The problem with the proposal of Papineau and Macdonald, as I see it, is that they propose a ground of induction for higher kinds that has no historical dimension, whereas in fact, the interesting higher biological kinds are, in general, historical kinds.

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