Biological Species Are Natural Kinds

Abstract. This paper argues that typical biological species are natural kinds, on a familiar realist understanding of natural kinds—classes of individuals across which certain properties cluster together, in virtue of the causal workings of the world. But the clustering is far from exceptionless. Virtually no properties, or property-combinations, characterize every last member of a typical species—unless they can also appear outside the species. This motivates some to hold that what ties together the members of a species is ability to interbreed, others that it is common descent. Yet others hold that species are scattered individuals, of which organisms are parts rather than members. But not one of these views absolves us of the need to posit a typical phenotypic profile. Vagueness is here to stay. Some seek to explain the vagueness by saying species are united by “homeostatic property clusters”; but this view collapses into the more familiar realist picture.

Mill introduced the term “natural kind” in the course of explaining why it is that scientific induction can be counted on to yield knowledge concerning many of the world’s objects (Mill 1950, pp. 303-304). Since there are inductive sciences that discuss all manner of objects both great and small, and do so with a fair measure of success, one would expect natural kinds to be as thickly strewn across the world as leaves in autumn—if indeed they play the explanatory role that Mill assigned to them. But in fact contemporary proponents of natural kinds defensively huddle around a very small group of standard examples. Conspicuously absent from the standard examples are biological species. Aristotle can plausibly be said to be the first philosopher to make systematic use of the idea of natural kinds, even if the idea did not receive a name until Mill. Yet as a theorist of natural kinds, Aristotle clearly thinks that the central examples are, precisely, biological species. Contemporary philosophers of biology for the most part hold that biological species are not natural kinds at all, in anything like a traditional sense. This paper seeks to reclaim the main historical example of a natural kind. It argues that typical biological species are in fact natural kinds.
Just what would it take for biological species to be natural kinds? Here is a rough characterization of what a natural kind is: it is something that occurs in spatio-temporally separated instances, all of which non-accidentally resemble each other in a number of respects. For the purposes of this paper I will adopt a slightly more controversial characterization, one that speaks of properties. A natural kind obtains, I shall say, where a certain plurality of properties non-accidentally cluster together, in instance after instance, separated in space and time. (Below I shall add a qualification about the sort of clustering that must obtain.) A crucial feature of the concept of a natural kind—one that makes it controversial whether biological species are natural kinds—is that there is no limitation written in to the nature of any natural kind as to where in space, or when in time, instances of the kind can occur. That is, instances of any natural kind could occur at any place and time, so far as being an instance of the kind in question is concerned. Any natural kind could, in this sense, have more instances than in fact it has, or fewer. For some philosophers, there can even be natural kinds that in fact have only one instance (Wilkerson 1995, pp. 133-39).

A more standard characterization of a natural kind would be that it is a class of objects or individuals, across which the same properties non-accidentally cluster together. But this characterization appears to conflict with certain standard examples of natural kinds. Some standard examples are pluralities of objects: for example, electrons and H₂O molecules and stars. But particular matters and stuffs are also standardly said to be natural kinds: for example, gold and water and wax. The difference between the two categories of natural kinds emerges when we note that while it does make sense to ask how many electrons or H₂O molecules there are—either unrestrictedly, or in a particular region—it does not make sense to ask, in the same way, how many golds or waxes there are; one can only ask how much gold or wax there is (whether unrestrictedly or in a region). That is, “electrons” and “H₂O molecules” are count nouns, but “gold” and “wax” are non-count nouns. Henry Laycock has recently argued that some non-count nouns—and “gold”, “water” and “wax” are all examples—are also “non-atomic”: it is a
feature of their semantics that they do not divide their reference over objectively discrete individuals (Laycock 2006, pp. 135-39)\(^1\). E.J. Lowe makes much the same point when he argues that portions of such matters are not individuals but “dividuals” (Lowe 1998, p. 161; cf. pp. 72-74). So unless we are to drop some of the putative natural kinds that figure on the already abbreviated standard list, we should not say that natural kinds are pluralities of objects. We can say that there are instances of such natural kinds as gold and wax: there really are portions of gold and of wax. But we must not take this to mean that there is a particular number of such instances. For any portion of gold, indeterminately many portions of that portion are likewise portions of gold. We can count spatially maximal continuous portions of the natural kind, gold, but not portions simpliciter. All natural kinds have instances, then, but not all have countable pluralities of discrete instances.

Yet any natural kind—whether one like electrons or one like gold—must be capable of having instances unrestrictedly scattered across space and time, instances that non-accidentally possess the same properties as one another. Why might the clustering together of these properties, in instance after instance, be no accident? This question marks the divide between a language-centric or conventionalist understanding of natural kinds, and a realist understanding. On the conventionalist understanding, what it is for two objects (or two spatially maximal portions of matter) to be the same in kind is for them to fall under the extension of a common sortal. If one then supposes that the extension of our sortals is fixed by the satisfaction of a plurality of predicates that we associate with that sortal, one has supplied an answer to why it is no accident that instance after instance, of any natural kind, possesses properties belonging to a recurrent cluster. On this picture, one locates the “glue” that ties together the properties characteristic of any natural kind in us and our linguistic (or conceptual) practices. On the realist picture, in contrast, one locates the “glue” in the workings of the world itself. One supposes that quite apart from what sortals we may fashion, and what conditions we may lay down on the satisfaction of those sortals, certain properties just do cluster together, in instance after instance, as a function of the way the world works. On the most modest version of this position, the properties in question are bound together by mind-independent causal necessity, if not by “metaphysical” necessity.
The idea that the extension of our sortals is fixed by predicates (or “descriptions”) that we associate with those sortals has come under extensive scepticism ever since Kripke’s Naming and Necessity (Kripke 1972). Many have come to find it more plausible that our sortals gain their extensions through some sort of causal contact we have with members (or portions) of some kind—a kind that manages to be a kind prior to, and independently of, our linguistic practices, a kind the distinctive features of which are glued together independently of us. Given this picture of how our sortals work, it becomes entirely plausible that there are some recurrent clusterings of properties—and hence some natural kinds—for which we have no corresponding sortal. Sameness in natural kind then may be a wholly mind-independent phenomenon.

Elsewhere I have argued that the conventionalist account of sameness in natural kind is ultimately self-defeating (Elder 2004, 2007a, 2006). The present paper argues for a less aggressive thesis, namely that a realist account of natural kinds is defensible in the particular case of typical biological species. The heart of such an account is that what it is for certain properties jointly to mark out a natural kind is for them to be tied together causally, so that their recurrent clustering is no accident. But I should add—in order to ward off scepticism about the realist account—that one needs to require the right kind of causal clustering. That is so, at least, if one wants to retain the traditional idea that instances of a natural kind cannot depart from their kind without ceasing to exist—or, in other words, that the membership-conditions for any natural kind double as extinction conditions for instances of that kind (Elder 2004, 2003). For consider: it seems overwhelmingly plausible that a human adolescent can depart from adolescence without ceasing to exist, or that a portion of frozen H2O can melt without ceasing to exist, or that a human suffering from diabetes could (in principle) be cured of the disease without ceasing to exist. And yet there is a causal connection between the age of adolescents and a number of distinctive properties concerning adolescent behavior and brain organization; a causal connection between the temperature of frozen H2O and a handful of properties distinctive of ice; a causal connection between having diabetes and having a plurality of associated debilities. If adolescents, ice, and diabetes sufferers are not to qualify as natural kinds in their own right, a more finely-delimited causal connection must be
required of the properties that characterize a natural kind, the properties that are essential to its instances. Elsewhere I have argued that what keeps age, temperature, and endocrine health from being essential properties of these would-be natural kinds is that those properties do not hang together with others in either of two particular ways (Elder 2007b). Sometimes we judge properties to be essential to a particular natural kind because they are causally shaped by other properties that are characteristic of that kind: for example, the characteristic melting point and malleability and ductility of gold are shaped by others of gold’s properties, and ultimately trace back to gold’s atomic number. Sometimes we judge properties to be essential because they occupy the same role as gold’s atomic number itself: that property of gold is not shaped by other properties characteristic of gold—the causal shaping goes rather in the opposite direction—but gold’s atomic number does, all by itself, determine many other properties that are characteristic of gold. But an adolescent’s age is not shaped by behavioral properties of an adolescent, and is not all by itself determinative of those properties—for in a fifteen-year-old dog, it enjoins no such behavioral properties.

But this question—the question of what sort of causal connection among properties renders them characteristic of a natural kind, and essential to its instances—need not be settled for the purposes of this paper. For the purposes of this paper, all that matters is that the properties characteristic of a natural kind, on a realist understanding of what a natural kind is, must be glued together with a fixity at least great enough that the properties count as causally connected. This will matter in section III.

II

Prima facie, biological species do seem to be natural kinds. For any biological species, there is a plurality of rough phenotypic traits that is roughly diagnostic of membership in the kind. And it can be argued that the properties in any such plurality non-accidentally cluster together across the members of the species.
To get clear on what a non-accidental clustering of properties involves, it is best to begin with two standard examples in which certain properties do cluster together recurrently, but perhaps only accidentally. The first example is the putative natural kind jade, which turns out to be encompass two distinct minerals, jadeite and nephrite. Jadeite and nephrite share many properties that are of interest to jewelers and artisans, and so it is no accident that we have a single sortal, “jade”, that both minerals satisfy. But jadeite and nephrite differ in their chemical composition. In consequence, they differ also in some of their surface-level properties: for example, the color of nephrite is either creamy white or some flat shade of green, while jadeite can be blue, lavender, pink, or emerald green. The second example comes from philosophical fiction. On Twin Earth, Putnam hypothesized, there is a liquid which is “indistinguishable from water at normal pressures and temperatures” (Putnam 1975, p. 140): it is clear, odorless, potable, has an index of refraction of 1.414, etc. But it does not have the chemical composition H₂O; its complex chemical composition is abbreviated as “XYZ”. Under normal conditions, then, XYZ and H₂O share all their surface-level properties. One could suppose that XYZ and H₂O populate a single natural kind, one that we could call “surface-level water” or “SLW”.

Certain properties cluster together across portion after portion of jade; even more properties cluster together across sample after sample of SLW. Is it no accident that these same properties appear across all the different portions of jade, or all the different samples of SLW? Certainly it is no accident that all the same properties in a particular collection crop up in sample after sample of H₂O: the properties are governed by molecular structure, and all samples of H₂O have the same molecular structure. It is no accident that the same properties are found in sample after sample of XYZ, for a parallel reason. But it does seem to be mere accident that the very same properties crop up across samples both of H₂O and of XYZ—across samples of SLW in general (Elder 1993). For the reason why those properties crop up across the H₂O samples is different from the reason why they crop up across the XYZ samples: the underlying microstructures differ, and they are represented, in Putnam’s discussion, as having no significant chemical commonalities. It is bare coincidence, then, that all these properties can be generated by two entirely different microstructures, and hence bare coincidence that they are all found
across samples of H₂O and XYZ alike. Similarly, the reason why certain aesthetically salient properties crop up across samples of jadeite is different from the reason why those same properties crop up across samples of nephrite. The recurrence of these properties across samples of jadeite and nephrite alike is merely an accident.

If a plurality of organisms is to populate a genuine natural kind, then, more is needed than just that the same phenotypic traits crop up in member after member of the plurality. The same traits must recur across all the organisms for a common reason. The most plausible way of spelling out what this reason might be is to say that all the organisms in the plurality are shaped as they are by a common history of evolution, and in particular of natural selection. The pheneticists supposed that a sound taxonomy of species could sort organisms into species strictly on the basis of contemporaneously occurring phenotypic traits. But such a taxonomy cannot be relied on to carve out true natural kinds. For that, we must look to history—to common evolutionary descent. The recurrence across a population of merely homoplastic traits, even if the traits are numerous, is not enough to mark out a natural kind.

The prima facie reason for thinking that biological species are natural kinds is now starting to emerge: they are pluralities of organisms across which roughly-delimited phenotypic³ traits cluster together, and do so because of a common evolutionary history. This position need not deny that a typical species can alter in response to ecological changes, or changes in population structure, for one can think that the rough phenotypic profile that characterizes a species by nature occupies a place within a possibility-space, a “morphological field”, in which certain adaptations and not others are available (Webster and Goodwin 1996). But it does face weighty objections. In the next section I will consider the objection that “roughly delimited” here papers over a gaping hole: perhaps crisply defined traits are virtually never found across all members of a biological species, unless they also occur outside that species. In the fourth section I shall consider the position that a biological species is not a plurality of organisms at all, but a scattered individual. In the fifth I return to the idea that species are pluralities after all, pluralities united by the possession of one majority or another out of a common roster of crisply-defined properties.
But before leaving the present section, there is one loose end to be tied up. The prima facie case for viewing species as natural kinds is that their members have in common certain phenotypic traits, and have them because of a common evolutionary descent. But does “common evolutionary descent” here mean that the members of a biological species must—if the species is in fact to qualify as a natural kind—all come from a common sort of evolutionary history; or does it mean that the members must all come from numerically that very history, from which in fact they are descendant? To put the question differently, are species individuated by their actual evolutionary origins, if species are natural kinds? Is it a priori excluded (again, if species are natural kinds) that this very species could have arisen at a somewhat different place or time, or from numerically distinct members of ancestor species, from those that characterized its actual origin? If a species phenotypically just like this species had arisen from just the same sorts of ancestors, thanks to just the same sorts of mutations, and in response to just the same sorts of selectional pressures and the same sorts of intra-species competition as this actual species did, but had done so in numerically different historical settings, is it a priori guaranteed that the species in question would have been a distinct species from this one? Is it metaphysically possible that a given species should have arisen through different episodes of mutation, from those through which it actually arose?

The answer one gives to this question will depend on one’s general view on causation. That is because the connection between the properties essential to a given natural kind, on the realist understanding of natural kinds which this paper seeks to apply to biological species, must be strong enough that it qualifies as at least a causal connection. Thus coming-from-numerically-that-history-of-natural-selection can count as an essential property of a given species (quà natural kind) only if that “origin” property causally shapes other properties characteristic of that species (quà natural kind). But on most of the views on causation that are commonly defended, it is simply out of the question that there should be any such causal shaping. A classical view, still widely defended, is that any true causing must instance some general causal law, or some package of causal laws (Hempel and Oppenheim, 1948; Davidson 1970). A more recent alternative holds that any true causing must instance some
generalization, which need not be perfectly exceptionless, that expresses an “invariance”—a
generalization, that is, that expresses the way values of one variable track, and are shaped by, the values
taken by another variable, at least across a wide range of background circumstances (Woodward and
Hitchcock 2003a, 2003b; Woodward 2003; Hitchcock 2001a, 2001b). On any such view, causes produce
outcomes by virtue of the repeatables that they embody. That is, an event qualifies as a cause by virtue of
the properties that some object instantiates, or the properties that several objects instantiate, or the
relations that obtain between suitably propertied objects. True causing is always in principle replicable in
numerically distinct events involving numerically distinct objects. Causation shapes the way the world
goes by grabbing on to repeatable features.

I shall assume that this general sort of view on causation is correct. But then it follows that if a
particular species (quâ kind) can only have come from just that historical origin, this will be so in virtue
of something about that origin that is qualitative and/or relational, and hence in principle repeatable. The
origin will be essential to the species not because it is numerically the origin that it is, but because it is the
sort of origin that it is. This point will be of importance in the next section.

III

The thesis that biological species are natural kinds is commonly taken to say: for any biological
species, there is a plurality of phenotypic traits—or perhaps genotypic traits—that are individually
necessary, and jointly sufficient, for membership in that species. And it is almost universally agreed that
no such general claim is even remotely plausible (Dupré 1981; Rosenberg 1985, pp. 180-225; Hull 1992;
Sober 1982). One reason for finding the claim implausible is that in a fair number of species there is
regularized dimorphism, especially sexual dimorphism, or polymorphism: this threatens the idea that
some one package of phenotypic traits is necessary and sufficient for membership in a species. I will
discuss this point in section V. By far the more influential reason for finding the claim implausible is that
in any species, there occur members that are abnormal or aberrant, and members whose phenotypic traits
reflect localized genotypic variations. It is of little avail for those who think species are natural kinds to reply here that for many natural kinds, there are borderline instances. That does certainly seem to be true: stars seem to be a natural kind of which there are atypical members; it seems possible that there could be borderline instances of even the kind electrons (Collins 1988, pp. 210-13); “heavy water” seems to be a borderline case of water; for countless diseases and syndromes, there are borderline cases. But any such reply appears to miss what Darwin has taught us about biological species. The very processes that bring biological species into existence guarantee that there will be ceaseless departures, among the members of a species, from both the “typical” genotype and the “typical” phenotype. The idea that species are natural kinds seems to be associated with Aristotle for a discrediting reason: it appears to ignore that there is natural selection (Hull 1992). One way of responding, for philosophers inclined to think that species are natural kinds, is to say that while there is, for a typical species, no fixed collection of properties that are individually necessary and jointly sufficient for membership in the species, it nevertheless is true that the members of the typical species are bound to present some shifting subset—some shifting majority—out of the properties on a common list. I shall return to this suggestion in section V.

A more promising response, for those who wish to reclaim species as examples of natural kinds, is to ask: why think that the properties that mark out natural kinds in the domain of biology are limited to phenotypic and genotypic properties? Organisms possess the phenotypic and genotypic properties that they do simply in virtue of how matters stand during their own existences: these are non-historical properties. But historical properties may be crucial as well. In the previous section I argued that if any collection of phenotypic traits marks out a natural kind, in the realm of biology, it must be a collection of traits that cluster together for a common reason—and this common reason will consist, I suggested, in a common history of natural selection. It would follow that the members of a biological natural kind must share not only certain phenotypic properties, but the common historical property of being descendant from the same sorts of ancestors, under the influence of the same sorts of selectional pressures. Kind-membership cannot hinge solely on non-historical properties, whether phenotypic or genotypic.
Indeed the very objections canvassed above, to the claim that biological species are natural kinds, assume that historical properties—or, at any rate, properties involving descent—unite the members of any given species. This emerges as soon as we ask: *in virtue of what* do atypical or mutated members of a given species qualify *as* members of that species? There are two sorts of descent-involving properties that can be invoked to answer this question. One is forward-looking (and counterfactual), and the other is backward-looking. The forward-looking property is the capacity to interbreed, and to produce fertile offspring, with opposite-sex members of the species. This is the core of Ernst Mayr’s “panmixis” criterion of species membership: what unites the members of a biological species is not so much phenotype as the ability to interbreed (Mayr 1963, p. 19). The backward-looking property is descent from “the right” ancestor organisms. This is the criterion of species membership invoked by those who think that species are “historical kinds” (Millikan 1999; cf. Simpson 1961, p. 153).

What then really divides *opponents* of the idea that species are natural kinds from *proponents* of that idea—if, as I claim, the proponents must say that members of a species are united by a descent-involving property? Nothing, in the end. That is the point for which this section argues. Opponents often talk as if *all* that unites the members of a given species is possession of a common descent-involving property—as membership in a given species does not *also* involve satisfaction of a roughly-defined phenotypic profile. But while that position can take different forms, it is untenable in any form, I contend.

Consider, to begin with, the forward-looking form of this position: what the members of a species have in common is just the ability to produce fertile offspring upon mating with randomly-selected opposite-sex members of the species. Many of the challenges to this position (sometimes called “the Biological Species Concept”) can be met by adding a minor epicycle to the basic model; the real objection is that the epicycles end up being too numerous and randomly-assembled. One huge problem is that many biological species reproduce asexually. Even within sexually reproducing species, there are infertile or non-reproducing members. Among fertile members of a species, the claim that each could in principle interbreed with any fertile member of opposite sex seems to involve questionable idealization.
Historically separated populations of a given species sometimes develop different patterns of behavior, with the result that members of one population will not in fact interbreed with members of another: in what sense, then, is it true that members of the first could interbreed with members of the second? One way of accommodating this problem is to say that what is required is not that a member of a given species must be able to interbreed with just any other member, but only with some other members: if capacity to interbreed links population A to population B, and B to C, and C to D, we may then have a capacity that still links together the whole species, even if members of A cannot interbreed with members of D. The problem with this remedy comes from what are called “ring species”. What seem intuitively to be different species of arctic gulls, for example, display just this pattern of potential interbreeding (Tinbergen 1953).

The backward-looking form of the position that phenotypic features play no role in uniting the members of a species may at first seem more promising. Descent from common ancestors can indeed explain why the organisms in a certain collection all display the same (or roughly the same) non-historical traits as one another, and can usefully predict that we are likely to find yet other shared non-historical traits. That is why the cladist taxonomy of biological species (the so-called Phylogenetic Species Concept) is theoretically fruitful. But that the members of a given species all descend from the same ancestor organisms as one another cannot be the whole story on what unites those members as a species. For on a broad enough historical scale, members of what intuitively seem to be different species all descend from common ancestors; it may even be that all forms of life are ultimately descendant from the same unicellular organisms. To avoid the consequence that all organisms belong to just a single species, then, one must hold that what the members of a single species have in common is descent from ancestor organisms from which members of other species are not descendant.

But from which ancestor organisms: what qualifies certain long-departed organisms and not others as those, descent from which is required for membership in a given species? One might try to answer: “nothing qualitative—nothing that consists in the possession, by certain long-departed organisms and not others, of certain phenotypic (or genotypic) properties”. That is, one might say that what unites
the members of a given species is that all of them are descendant from just numerically *those* long-departed organisms. But with this position there are two problems, a lesser and a greater.

The lesser problem is that it seems entirely thinkable that more than one actual species arose not through some single episode of mutation, or some geographically isolated cluster of episodes, but arose instead from parallel but independent episodes of mutation. If that did happen, then there will have been members of that single species, at least in early generations, that did *not* all descend from numerically the same ancestor organisms as one another. And even if such parallel origination has never actually occurred, it seems hard to deny that it could have (cf. Kitcher 1992, p. 322). After all, there are countless examples of convergent evolution at taxa higher than species—for example, assassin spiders (*Arachaeidae*). These may not yield interbreeding populations; but, in view of such familiar phenomena as hybridization and “ring species”, it seems hard to deny that there *could* be convergent evolution for a single species.

The greater problem is that this way of understanding what “descent from common ancestors” amounts to makes it mysterious why descent from common ancestors might serve to explain why current members of the species share—or at least roughly share—the particular properties that in fact they do (at least roughly) share. For causation shapes events in the world by grabbing onto repeatables. If descent from common ancestors is to figure in causal explanations of why the current members of that species possess the particular traits that they do in common possess, this must be because of something *about* those common ancestors—some qualitative character that those ancestors had. That this or that current member of the species is descendant from numerically this or that long-dead organism says nothing about why this or that current member has the properties that it does. What does cast light on that question is what that individual long-dead organism was *like*, what non-historical properties it possessed.

If descent from certain ancestor organisms is part of what unites the members of a given species, then, the ancestor organisms in question must qualify for that crucial role by virtue of phenotypic (or perhaps genotypic) properties that they possessed—by virtue of their non-historical properties. Viewing biological species as “historical kinds” does not absolve us from the task of identifying non-historical
properties that are diagnostic of membership in that species. This point becomes doubly apparent when we ask just how many descendants of current members of the species will count as still belonging to that very species. A species can gradually be eliminated by natural selection, being replaced by a distinct successor species. The distinctness will be a distinctness in respect of non-historical properties.

I conclude that opponents of the idea that biological species are natural kinds are, for all we have seen so far, in much the same position as proponents of that idea. Each side must hold that some descent-involving property essentially characterizes any biological species. And each side must allow that there is some plurality of non-historical properties—I have suggested that these are phenotypic properties—that, one way or another, essentially characterize that species as well. One cannot indeed ignore the fact that within any species, there are aberrant and atypical members. Rather one must accommodate that fact in one’s account of what, in respect of non-historical properties, members of that species are bound to be like. The phenotypic (or perhaps genotypic) characterization that one offers must be sharply-defined enough that it applies, and can apply, to members of no other species—or at least, to no organisms that determinately belong to another species. Yet the more sharply one defines the required phenotype, the more cases of indeterminate (or borderline) species-membership one must allow. Biological species will inevitably be vague in their membership. For all that, they may really be natural kinds—just as stars and electrons and water and diabetes are natural kinds.

IV

I have so far ignored one widely-favored strategy for avoiding the problem that membership in any biological species is bound to be a vague affair, one that admits of countless borderline cases. This is to hold that biological species are not collections that have members at all. A biological species, on this view, is not a “many”.

There are in fact two ways that one might hold that a species is not a “many”, and one of them appears at first blush to be entirely consistent with the claim that biological species are natural kinds. For
there are, as we noted in section I, many natural kinds on the standard list of examples that themselves are not collections of discrete individuals—natural kinds that are not a “many”. There are in the world such natural kinds as gold and water, but there are not many golds or many waters. There is only much gold and much water.

This is not the widely-favored variant of the position that a species is not a “many”, and it is not one that can for long be pursued. The widely-favored variant says rather that a biological species is neither a “many” nor a “much”, but a “one”—a spatio-temporally scattered individual. But it is worth dwelling for a moment on the thought that species might be like matter-kinds, since the reasons why this thought cannot long be pursued will point to problems with the thought that species are scattered individuals.

Mill coined the term “natural kind”, as I said at the outset of this paper, in the course of explaining why inductive sciences can yield knowledge about many different domains in the world. But how can an inductive science yield knowledge about a natural kind that is not a “many” at all, but a “much”? One’s initial image of successful scientific induction is that it proceeds from finding that certain properties recur across a small number of individual items—or better, across many individual items—to a conclusion that those properties are found in all items in a certain class. Yet instances of a kind like gold or water are not objectively segmented into discrete individuals at all. How then can there be a successful inductive study of such kinds?

The answer is that the properties that characterize matter-kinds such as gold and water are (what are sometimes called) “border-insensitive properties” (Elder 2007a). That is, the presence or absence of these properties, in a given instance of a matter-kind, is not affected (within broad limits) by how far out the borders of that instance extend. If it turns out that some spatially maximal portion of gold has a certain ductility or melting point, it will be equally true that indefinitely many sub-portions within that portion have that same ductility or melting point. If it turns out that the water in the left-hand beaker freezes at 32°F and that the water in the right-hand beaker freezes at 32°F, it will be equally true that the water in the beakers—indeed the water in the room, if the only water is in the two beakers—freezes at...
32°F. There is no need to determine where one portion of a matter-kind leaves off and another begins, in
order to determine which border-insensitive properties characterize the matter-kind. And that is a good
thing, since there is no objective fact of the matter as to where one portion leaves off and another begins.
That is why inductive study of a matter-kind can succeed.

It is also possible to succeed in conducting an inductive study of what some particular biological
species is like. This is low-level science, to be sure, but it is what field biologists routinely do. (That
such study succeeds as well as it does serves, incidentally, to put in perspective one’s worries about
aberrant and atypical and borderline members; cf. Kitcher 1992, pp. 320-21.) But the properties which
such a study finds to characterize a given species are almost never “border-insensitive”. Such a study
might find, for example, that wolves have a characteristic method of hunting prey, and squirrels a
characteristic way of running for evading prey. It might find that either species has a weight, when
mature, that falls within a certain range. But only individual whole wolves run after prey in that way, or
weigh that much; only individual whole squirrels zigzag in that way away from predators. It is crucial to
be able to recognize how far out the borders of an individual instance of wolf or squirrel extend, in order
then to spot the properties that characterize that natural kind.

So we cannot save the thought that a biological species is a natural kind by holding that it is a
kind like gold or water—a “much”, rather than a “many”. Perhaps a biological species avoids being a
“many”—and thereby avoids having membership-conditions—by being a “one”, a scattered individual.
The biological kind Canis lupus, then, would be one spatio-temporally discontinuous thing, having parts
at just those places, and over just those stretches of time, where common sense thinks that individual
wolves are found. Perhaps we may and must relinquish altogether the thought that a species is a natural

But our reflections on matter-kinds leave us with the question: how is it that empirical study into
the nature of a given species can succeed? Which properties do we find, through empirical study, to
characterize such a scattered individual, and how do we learn of them? Let us postpone this question
momentarily to ask a prior one. Which properties does such a scattered individual have?
Consider, to stick with our example, the single scattered object that has parts just where and when, as common sense would say, the actual wolves that exist over history are found. It will have as a whole a certain extension across time, a certain temporal length. (For perdurantism in Hull, see Hull 1989, p. 187.) At any one time, it will have as a whole such properties as maximal extension across space, aggregate weight, and center of gravity. It will also have properties that are sensitive to the individual character of its individual parts. These will be so-called “structural properties”, properties expressed by conjunctive predicates of the form “has one part at such-and-such location bearing such-and-such properties, has another part at thus-and-so location bearing thus-and-so properties, [etc.]”. Each conjunct in such a predicate spotlights features possessed by one part taken individually.

Just which of the properties of such a scattered individual can empirical research hope to discover? It would be hard work to discover, at any one time, what aggregate weight the scattered object wolfkind had. But it would also be pointless: the property would be of no scientific interest. Neither would the maximal spatial extent possessed, at any one time, by this scattered object, nor the center of gravity. Thus not only would virtually none of the border-insensitive properties of this scattered object be of any scientific interest—not its combustion point or specific density or index of refraction, for example—but also virtually none of the border-sensitive properties of this scattered object would be of scientific interest either. Or, to speak more precisely, no border-sensitive of the unitary scattered object wolfkind, taken as a whole, would be of scientific interest. Interest would attach to properties sensitive to where the borders of certain individual parts of this scattered object lie. The properties of this scattered object that are of scientific interest would be—or would logically depend upon—its structural properties. Thus we might want to know at just which locations—more exactly, in just which climates or ecological niches—this object’s organism-sized parts were located. We might also want to know just which non-historical qualitative properties characterize the various organism-sized parts of this object.

The focus of scientific interest would lie in the last sort of property. If we did want to know of the structural property that expressed the locations—the climates, the niches—in which organism-sized parts of this object existed, that would be because we thought that this property might explain the
presence, in those organism-sized parts, of certain phenotypic (and, just possibly, genotypic) properties. We might also think that the structural location-property might help predict future alterations in the structural phenotype-property, alterations permitted by the “morphological field” in which that phenotype-property is located. Only if we thought we could know something fairly definite about the structural phenotype-property—the property expressed by the predicate “has one organism-sized part bearing such-and-such phenotypic features, another organism-sized part bearing thus-and-so phenotypic features”—would we have any interest or stake in learning of other structural properties.

How, then, might we hope to know something fairly definite about this structural phenotype-property? In general, we do not suppose, in confronting an individual object that is widely scattered across space, that study of a few of its parts will tell us much that is reliable about its remaining parts. We do not infer, upon observing the rail yards outside Chicago, that all of Chicago is covered by railroad tracks. We do not infer, upon seeing a mountain that rises at the Pacific shore, that all of North America is covered by mountains. Only where we can presuppose a certain qualitative homogeneity among the parts of a spatially sprawling object do we suppose that research into what some of these parts are like can tell us anything reliable about what the parts of that object are like in general.

Thus the proponent of the “species as individuals” view must say that we are warranted in expecting just such homogeneity, on pain of making it a mystery how any scientific study into the nature of a species might hope to succeed. She must say that while any species is a spatio-temporally sprawling individual, there is an objectively privileged segmentation of this individual into parts—to wit, into organism-shaped parts—such that roughly the same phenotypic (or possibly genotypic) traits can be counted on to crop up in part after part. But then really, this amounts to saying that the scientifically interesting properties of such a sprawling individual emerge only when we view it as a plurality of organisms. Operationally, the “species as individuals” view must treat species as natural kinds.

This point is sometimes overlooked. For example, Ereshefsky and Matthen (2005) argue that the typical species must be viewed as a population that by nature has a certain structure—and from this Matthen (2005, p. 216) concludes that typical species must be viewed as “causally active group-
individuals that have individual organisms as parts”. But does this really mean—as it seems to—that species must be viewed as individuals? As a scattered individual, a species has, as we noted above, almost no properties that are of scientific interest. The only properties that are of scientific interest are the very ones we notice when we treat the scattered individual as a plurality of objectively-segmented parts, of individual organisms. To be sure, one such interesting property may be membership in a population that bears a certain structure. But what bears this property are organisms, as objectively-privileged segmentations of the so-called scattered individual.

I concluded the last section by saying that opponents of the idea that species are natural kinds are in much the same position as the proponents: the opponents too must say that, for any given species, we can specify a rough phenotypic profile. We now have seen that the same is true for the latest group of opponents, the philosophers who think that a species is a single scattered individual.

V

Richard Boyd holds that what ties together the members of a given biological species, and sets them apart from members of other species, is the occurrence across these members of a “homeostatic property cluster” (Boyd 1999). This account sounds close to the characterization of biological species that, in section II, I represented as being necessary and sufficient for biological species to qualify as natural kinds. Boyd’s account faces the objections, mentioned above, from dimorphism and polymorphism, and so does the characterization of biological species that this paper endorses. My primary aim in this section is to address these objections.

But first the secondary aim, which is to differentiate the picture of species endorsed here from Boyd’s picture. This is mandatory for the purposes of this paper: for, if the account of natural kinds offered in section I is correct, Boyd’s picture of biological species is actually incompatible with the claim that biological species are natural kinds.
To see the incompatibility, one must start off by asking: just what is homeostatic about a “homeostatic property cluster”? Homeostasis occurs where some mechanism, or some group of mechanisms, ensures that the values assumed by some variable will hover within a confined range. In humans, the mechanisms for perspiring and for shivering between them ensure, at least across a wide range of circumstances, homeostasis with respect to human body temperature. Just what variable has its values confined to a narrow range, when organisms within a species are characterized by a “homeostatic property cluster”? It is a familiar idea that the members of a species are characterized by a cluster of properties—phenotypic ones, and perhaps genotypic ones as well. But where might homeostasis come in?

The clue to this question emerges when we note what the challenge is, to which Boyd’s account of the commonalities among members of a species is intended to respond. The challenge is that very often, we can specify no crisply-defined properties that can be counted on to occur across all members of a given species. Boyd’s response, I suggest, is to say that while there are no crisply-defined properties that can individually be counted on to occur across all the members, there is a plurality of crisply-defined properties that can collectively be counted on to be mainly present across all the members. There is homeostasis with respect to the variable number of these properties that occur across all members of the species. The values taken by that variable will hover at a number lower than, but close to, the total number of properties in the plurality.

Why might such homeostasis obtain? One answer—not Boyd’s (Boyd 1999, pp. 144-46)—would be the conjunction of two theses: first, the conventionalist thesis that what it is for two or more organisms to belong to the same species is for them all to fall within the extension of one of our sortal names for species; second, the thesis that the extension of such sortals is determined by satisfaction of at least a majority of the descriptions that we associate with the sortal. But this answer combines the “description” theory of reference criticized by Kripke thirty-eight years ago, with a conventionalist account of sameness-in-biological-species which Boyd does not endorse, an account to which this paper seeks to defend a realist alternative.
On the realist understanding, then, what would ensure the homeostasis that Boyd believes in would be mechanisms in the world that, between them, ensure that the properties in the list collectively characteristic of a species must largely hang together. Individual organisms may occur in which two or three of the properties on the list are absent; but the only organisms that could lack *many* of the properties on the list would be organisms that lack *any* of the properties on the list. To put it metaphorically: if you tried to strip away more than just two or three of the properties, but not all of the properties, you would find either that the remaining properties would cause the properties you were trying to strip away to be present after all, or that the causes *underlying* the co-occurrence of the remaining properties would themselves cause occurrence of the properties you were trying to strip away (Boyd 1999, p. 143).

It is important to note that on Boyd’s picture, what is bound to hold steady, in instance after instance of a homeostatic property cluster, is just that *some* majority or *other* of the properties in the cluster jointly are present. No individual property in such a cluster, nor even any small group of properties, is crucial for the occurrence of (at least most of) the rest. To put it metaphorically: the causal processes of nature say, to each individual property in a HPC, “you can be replaced”. This might seem to raise a question about the mechanism by which the homeostasis is maintained. Boyd holds (to repeat) that at any one time, for any one HPC, there is such a mechanism, out there in the causal workings of the world. But then one might ask: would not the presence of this mechanism, in instance after instance of the HPC, entail that some one property (or some small group of properties) must individually be present in each instance? If the members of a species, for example, have in common some HPC of phenotypic traits, and what underlies the homeostasis is some particular shared history of natural selection, then must not each member of the species have the historical property, *coming from just that sort of history of natural selection*? Or, to take another of Boyd’s examples, if all instances of inflation are characterized by a common HPC of economic properties, mustn’t there be something the same *about* every instance of inflation that causally ensures the homeostasis? But Boyd’s position is that, in different circumstances or at different times, *different* mechanisms may tie together the properties in any one recurring HPC (Boyd
1999, p. 144, p. 156). In different circumstances, we can have the same HPC, and the same HPC-kind, even though nature has inserted different homeostasis-underlying properties to sustain that HPC.

This is a marvelously egalitarian picture of the properties that collectively are characteristic of a given species. One might wonder whether it is too good to be true. Could it really happen that significantly different histories of natural selection causally sustain exactly the same HPC of phenotypic (or genotypic) traits? If we found that one and the same entire cluster of traits were accompanied by descent from two significantly different evolutionary histories, this might well make us question whether evolutionary history did after all causally govern the traits displayed by contemporary organisms—for the difference in evolutionary histories would be “a difference that makes no difference”. In just the same way, if we found that the entire packet of surface-level traits displayed by water could be accompanied both by the microstructure H2O and by the significantly different microstructure XYZ, we might well start to wonder just how much microstructure really has to do with a stuff’s surface-level traits. That is why Putnam said, in the story about Twin Earth, that XYZ is characterized by the same surface-level properties as H2O “under normal temperatures and pressures” (Putnam 1975, p. 140): room is left to suppose that difference in microstructure entails some difference in surface level features. In just the same way, jadeite differs from nephrite in some of its surface level features. This is what makes it plausible that microstructure matters in the taxonomy of minerals, and that jadeite and nephrite consequently are different mineral kinds.

The strongest claim that Boyd could plausibly make, then, would be that significantly different histories of natural selection can causally sustain distinct HPCs of phenotypic traits, but HPCs that largely overlap with one another. Might all organisms bearing either of these distinct HPCs belong to a common natural kind? The suggestion falls afoul of the requirement that across the members of a natural kind, certain properties must cluster together, and must do so for a common reason. In the case of biological species, I claim, the common reason will be that the members are all shaped by the same evolutionary history—meaning a history populated by the same sorts of ancestor organisms, the same sorts of ecological and intra-specific pressures, etc. To any biological species, one such historical property will be
essential. There cannot be the egalitarianism, among the properties jointly characteristic of a biological
natural kind, that Boyd’s picture calls for. We must endorse an inegalitarian picture of biological natural
kinds: descent from a particular sort of evolutionary history, we must rule, cannot be stripped off from
the cluster that characterizes such a kind.

But while the picture of biological species that this paper endorses differs from Boyd’s, it does
face some of the same objections. First, there is the objection from sexual dimorphism: it often happens
male and female members of a given species differ in respect of body size, coloration, and behavior, and
the differences are sometimes dramatic (Ereshefsky and Matthen 2005). How then could one claim that
there is a fixed list of traits that is even mainly characteristic of all members of such a species? The
problem gets even worse when we consider the various forms of polymorphism that characterize some
species (Ereshefsky and Matthen 2005). The members of one species of ants, *Atta cephalotes*, all live off
the mold that is produced when a morphologically distinctive sub-group of the species carries bits of
leaves to subterranean “mold farms”. The sub-group that tends the “mold farms” possesses another
distinctive morphology, and in all there are four morphologically distinct sub-groups to the species
(Hölldobler and Wilson, 1990). Or consider diachronic polymorphism, such as one finds in species
whose members are tadpoles when young, frogs when adults, or worms when young, butterflies when
adults. What one cluster of traits could be said to characterize (or even mainly characterize) every
member of any such species, across all of its existence?

Formally, of course, it is fairly easy to devise responses to these objections. One would only need
to speak of conditionalized phenotypic traits, or disjunctive phenotypic traits. Thus one might say that the
members of a sexually dimorphic species all have in common—thanks to a roughly-defined shared
genome—the disposition to assume such-and-such a phenotype when the chromosomes determinative of
the male sex are present, and thus-and-so phenotype when the chromosomes determinative of the female
sex are present instead. One could say that the members of a species of frogs all have in common—again,
thanks to a roughly-defined shared genome—the disposition to assume a tadpole shape at one stage in
their existence, and a frog-like shape at another. One could claim that the mold-farming ants all have in
common a disjunctive phenotypic profile, featuring four (and no more) crisply fixed disjuncts, to each of which a certain likelihood of occurrence is attached. Or, to put it differently, one could say that essential to every member of this species of ants is belonging to a population that has a certain structure (cf. Ereshefsky and Matthen 2005).

But would such replies be merely formalistic tricks? Would conditionalized or disjunctive phenotypic properties, such as those sketched above, be merely properties that we invented, to circumvent the objection that no particular properties are common to all members of the various species in question? The answer would seem to be No. The continued proliferation of a sexually dimorphic species might well depend causally on the capacity in its members to assume either of two specific forms, characterized by two distinctive patterns of behavior. The continued proliferation of mold-farming ants might depend causally on the disposition, found across ancestor members of the species, to assume any of four specific morphological forms—to figure in a population that has a certain structure. The conditionalized and disjunctive phenotypic properties might well be ones that nature installed in the members of certain species, in the course of natural selection. To suppose that these species-spanning properties are merely our inventions would be to suppose that the polymorphisms in question did not engage nature’s causal processes, in the course of natural selection. And that would be, in general, an implausible thing to suppose.

VI

The position at which we have arrived is that biological species are natural kinds, of a perfectly traditional sort. They have instances that are scattered in space and time, instances across which a common collection of properties clusters together, and for a common reason. To be sure, for any biological natural kind, there will be abundant borderline instances—perhaps moreso than for other types of natural kind. But what the instances are instances of will be a natural kind of the traditional sort. So
far as being an instance of a biological species is concerned, there will be no built-in restriction on where, in space and time, those instances can occur.

The remaining objection is that this simply is not how practicing biologists think of biological species. No biologist would suppose that if organisms arose on some distant planet that were morphologically and physiologically just like house cats here on earth, and that arose through natural selection from just the same sorts of ancestors as those from which our house cats arose, in response to just the same sorts of selectional pressures involving just the same sorts of environing organisms and physical environments, and just the same sorts of competition (for mates, for food, etc.) among members of that species, those organisms would then be members of the species *Felis domesticus*. Of course, the suggestion itself is fanciful. It is stupefyingly unlikely that such a thing could occur, and almost as unlikely to suppose that practicing biologists could be brought to believe that it had occurred. But setting that aside, the objection remains. Biologists would judge that bare numerical difference in origins entailed that the organisms on the distant planet were not members of the species *Felis domesticus*. Many biologists are willing to think that species are collections, collections that have members. But they speak and think as if species are, by nature, spatio-temporally restricted collections.

But the presupposition of this paper is that the reference of our sortal terms—for biological species as much as for the natural kinds on the standard list of examples—is not wholly fixed by descriptions that we associate with those terms. Rather it is fixed by some sort of causal contact with instances of a causal nexus that really obtains in the world, one that binds together certain properties and ensures that they cluster together across instance after instance. We could suppose that instances of gold are found only in the Western hemisphere—just as the alchemists supposed that instances of gold are all ripened phases of instances of silver—but it still would not follow that our term “gold” referred only to portions of matter found in the Western hemisphere. We would simply be wrong about the extension of “gold”. Just so, were my stupefyingly unlikely scenario to be realized, our term “house cats” would include, in its extension, those organisms on that distant planet. They too would be members of the natural kind *Felis domesticus*.4
Footnotes

1 To be sure, there are discrete individual atoms of gold, and discrete individual molecules of water. But when we say “there is gold in the vault” or “there is some water in the pitcher”, we are speaking of spatio-temporally continuous aggregations of atoms or of molecules. Indeed it is questionable whether a single molecule of water qualifies as “some water”, or whether a single atom of gold qualifies as “a portion of gold”—for a single molecule of water does not have a freezing point, and a single atom of gold does not have a melting point.

2 Existing versions of the so-called “causal theory of reference” generally focus on a causal contact that obtained when a given sortal was introduced, perhaps through a “baptismal utterance”: reference is fixed by the objective nature of the object (or perhaps property) that caused the relevant perceptual experiences of the baptizer and of her listeners. A more defensible version of an “externalist” account of reference—an account that opposes the “descriptivist” or “cluster” theory of reference criticized by Kripke—focuses instead on what causes proliferation of a sortal term, its replication in sentence after sentence. This alternative version comes from Ruth Millikan: see, for example, Millikan 1989 or 1984.

3 My motivation for thinking that particular phenotypic traits may more reliably cluster cross the members of a species than particular genotypic traits comes from (1) Dupré’s arguments to the effect that classical transmission genetics cannot be reduced to molecular genetics (Dupré 1993, Chapter 6), and (2) the contention by Webster and Goodwin and that the phenotypic traits that do roughly recur across a species are expressions of a common morphological field (Webster and Goodwin 1996).

4 I am grateful to Tom Bontly and Ruth Millikan for conversations on the topics of this paper.
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