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Species of Biology:
The Cohesion and Individuality of Biological Species

by

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Abstract

At the intersection of philosophy and biology lies the *species ontology problem*. What sort of thing is a biological species? The traditional view is that species are natural kinds, but the *individuality thesis* has usurped the traditional view, arguing that species are *not* kinds and instead *are* individuals. Putatively, the individuality thesis has implications for debates ranging from biological classification and conservation, to moral philosophy. I challenge the individuality thesis. First, I argue that individualists have neglected promising revisions of natural kinds theory: species may be kinds. Second, I argue that there are different kinds of cohesion, that gene flow does not "hold species together," and that, therefore, species do not display the kind of cohesion that individuality requires: species are not individuals. Moreover, analyzing cohesion in terms of the philosophical notion of *realization* affords novel insights into the nature of cohesion, species, agency and individuality more generally.

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I hope this thesis is worth the efforts of all the people who helped bring it about.

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1

Species Ontology

Why has nature, and more precisely natural selection,
favored the discontinuities among the species?

Ernst Mayr 1999

1. WHAT SORT OF THING IS A BIOLOGICAL SPECIES?

One of the most obvious features of the living world is that its organisms do not form a seamless continuum of life but rather cluster into distinct species, such as red maple and bald eagle. Despite seeming so obvious though, species are remarkably puzzling. How to conceive of their evolution, how to classify them, and how to conserve them, are all familiar problems. Notoriously we struggle to even define the term "species." But the most fundamental species problem concerns the very nature of species. To get a glimpse of this, consider that species are indeed naturally distinct from each other, as Ernst Mayr indicates in the quote at the head of this chapter, but that this implies a species taxon is at once a multiplicity *and* a singularity. *Acer rubrum*, for example, is a multiplicity of red maples but is also a real entity that is singular in its distinction from, *Haliaeetus leucocephalus*, the bald eagle. What must explain this and other curious features of each species taxon is the fundamental nature of species taxa—what sort of fundamental thing we take species taxa to be. Yet deep controversy surrounds the fundamental nature of species taxa. What sort of thing is a biological species?

This is the *species ontology problem* and it is the focus of this work. Part of our job in this first chapter is to detail the two best-known and competing solutions to this problem so that we are prepared to examine them in the following chapters and draw our own conclusions about species ontology. To get just an initial sense of the conclusions for which we shall aim, we can begin with a sketch of the two competing solutions: the *species as kinds view* and the *species as individuals view.*

The species as kinds view is an ancient one going back to at least Aristotle and it claims that species are *natural kinds.* It attributes the discontinuities among species to each species being individuated by natural essential properties unique to the organisms it comprises. Moreover, individual organisms belong to successively more inclusive kinds, such as plant and animal, so that the living world layers out into a hierarchy of natural kinds grounded in individual organisms. The essential properties that individuals bear actually *cause* them to be the kinds of things they are at each respective level and these properties relate in lawful ways with those properties defining other kinds found both within and across levels. The hierarchy of biological natural kinds that organisms ground thus has a singular nature: the living world is integrated and within it species have primacy as the natural kinds to which organisms belong most fundamentally (Furth 1988, 28; Aristotle's *Categories* V; but see Pellegrin 1987, 313).

Advocates of the species as individuals view, or *individualists* as I shall call them, propose an altogether different view of species ontology, one motivated by reflection upon Darwinian evolutionary theory. On this view, a group of organisms forms a distinct species in virtue of forming "spatiotemporally localized cohesive and continuous entities" (Hull 1978, 294), rather than in virtue of essences that the organisms uniquely share. Moreover species are said to display the same kinds (though perhaps different degrees) of localization, continuity and cohesion as do organisms. For instance, a species is spatiotemporally localized, or *bounded,* in that it has temporal beginnings (speciation) and endings (extinction), and locations in space that (in part) determine its distinction from other species. A species is also *continuous,* at least temporally, as from its inception until its extinction it persists through changes as the same species. Finally, a species is *cohesive* in that conspecifics stand in certain relations with each other and these relations realize cohesive bonds between them. Furthermore, not only does a species seem to display the same kinds of boundedness, continuity and cohesion as does an organism, but these three characteristics also seem to be the very conditions of individuality that mark out organisms as paradigm individuals (Hull 1978, 294; J. Wilson 1999, 9). By satisfying these conditions then, species are not themselves organisms but they are bona fide individuals just like them (e.g. Mayr 1969; Ghiselin 1969, 1974, 1987, 1997, 2002; Hull 1976, 1978, 1980, 1999; Sober 1980, 1984; Holsinger 1984; Williams 1985, 1989; Splitter 1988; Horvath 1997; de Queiroz 1999; Crane 2004; Brogaard 2004; and for qualified versions of this thesis see Wiley 1980; Mayr 1987; Ereshefsky 1988, 1991, 2001).

The implications of the species as individuals view for the sort of integrated hierarchy of natural kinds to which the competing species as kinds view attaches are not immediately clear and authors have only just begun discussing them. But certainly on the species as individuals view, species no longer have primacy in the hierarchy of living kinds in virtue of being the kinds that organisms fundamentally are. Rather, organisms are the "parts" of "species individuals," just as your heart is a part of you.

Which is the better solution, the species as kinds view or the species as individuals view?

A majority of philosophers of biology, taxonomists and many biologists more generally now prefer some form of the species as individuals view. Indeed, for this majority (see references above) not only is the individual view best, but the kinds view is intractable and these authors take this intractability to lend considerable support to the individual view. It is, then, the conjunction of the individualists' negative thesis (species are *not* natural kinds) and positive thesis (species *are* individuals) that forms the more inclusive *individuality thesis*. First advanced in earnest in the 1970s by Michael Ghiselin (1974) and David Hull (1976, 1978; and see Stamos 1998 on how one can trace the individuality thesis back to Buffon), the individuality thesis has usurped the traditional conception of species ontology that stems from Aristotle.

Despite the impressive support that the individuality thesis has gathered, my view is that the thesis pledges a radical shift in our conception of the nature of species that individualists have not yet justified. Throughout this work, *Species of Biology*, I shall attempt to draw two main conclusions against the individuality thesis. The first of these conclusions confronts the negative arguments of the individuality thesis: it deflates the arguments against the species as kinds view. The second conclusion will occupy the majority of our time and confronts the positive arguments of the individuality thesis: it derives from a focus on the concept of "cohesion" within the contexts of species and biology more generally. Let me clarify these two conclusions and underscore their significance by taking each in turn.

One reason why the arguments for the species as individuals view have attracted so many supporters is that the individualists' arguments against the species as kinds view have seemed indubitable. However, we shall see that the case against the kinds view is unstable. A complete resurrection of the kinds view is beyond the scope of this work, but in a chapter we can show that individualists have neglected the resources that natural kinds theory bring to the species ontology problem. We shall work at drawing this conclusion prior to the second conclusion, since systematically turning back each of the arguments against the kinds view shall place added burden on the positive portion of the individuality thesis before we consider it in detail. This is a burden I do not think "species cohesion" can bear.

In considering species cohesion, the second and main aim of this work is to show that species do not demonstrate the cohesion of individuals. Specifically, we shall examine the notion of species cohesion in light of an analysis of cohesion more generally and then argue that individualists have failed to show that species cohesion corresponds to the *kind* of cohesion that is constitutive of individuality. This conclusion requires a little more elaboration before we set out to reach it.

As noted above, individualists presume that boundedness, continuity and cohesion are key marks of individuality exhibited by paradigm individuals, such as organisms. Rather than refer to an abstract analysis of "individual," individualists then exploit the "parity thesis" strategy of comparing the relevant features of species to those of organisms. Yet when individualists draw an equation between species cohesion and organismic cohesion they err in at least two ways: they help themselves to an unanalyzed notion of "cohesion," and they remain complacent about the causal status of gene flow interactions among conspecifics. Many authors claim that gene flow interactions between conspecifics (as enabled, for example, by interbreeding) are responsible for the evolutionary cohesion that species display, and many individualists think this ensures that species cohesion is just like organismic cohesion.

We can object to this argument from gene flow if, first, we attend more closely to the metaphysics of cohesion, and, second, we investigate the putative causal status of gene flow. Attending to the metaphysics of cohesion shows there are at least two kinds of cohesion at play in species debates and that only one of these kinds is (at least minimally) constitutive of individuality. By then investigating the idea that gene flow interactions are responsible for species cohesion, we shall see that gene flow does not hold species together in the way that individuals are held together. The causal powers of gene flow have been overstated. Moreover, no extant account of species cohesion besides the gene flow account suffices for individuality either. Therefore, current biology entails that species do not demonstrate the cohesion of individuals.

For at least two reasons, this conclusion will pose a serious challenge to the individuality thesis.

First, individualists tend to countenance the widely held idea, going back (again) to at least Aristotle, that "by far the most important definens of an individual is its internal cohesiveness." (Mayr 1987, 155; and see Ereshefsky 1988, 432; 2001, 112-119; Borjesson 1999; Stamos 1998; Lee and Wolsan 2002; Armstrong 1980; Shoemaker 1979; Ayers 1999, 229-253; van Inwagen 1990) Given this, the species as individuals view crucially relies on the claim that species demonstrate the kind of cohesion that paradigm individuals do (e.g. see Hull 1999, 32; de Queiroz 1999, 67; Brogaard 2004, 229, 236; Ereshefsky 2001, 114-119). My critique, then, will undercut one of the most powerful of the individualists' arguments, and one that their view requires if it is to go through.

Second, the claim that species and organisms share in a *kind* of cohesion has taken on new gravity in light of recently proposed revisions to the species as kinds view with which individualists compete. Early in the debate between the two views of species ontology, many authors mistakenly presumed that all natural kinds are spatiotemporally *un*restricted classes and that since species are clearly not unrestricted as such, they must be individuals (Grene 1989; Winsor 2003; Keller et al. 2003, 94). Merely arguing for what has been called the "historicity of species," then, was to argue *against* the kinds view and *for* the individuals view.

But authors have lately argued that species could be "historical natural kinds" (e.g. see R. A. Wilson 2005, ch.5; LaPorte 2004, 10ff.; Millikan 2000, 18-32; Boyd 1999; Griffiths 1999). Indeed, almost nobody now argues that species are *a*temporal kinds or classes; the historicity of species is beyond doubt; and a few authors even consider the historicity claim to be the individuality thesis' "key" insight (e.g. Sterelny 1994, 10; 1999, 123, f.n.4). But however key and widely accepted species historicity is, the emergence of revised species as kinds views, along with complications surrounding the simplistic distinction between spatiotemporally restricted and unrestricted entities more generally, shows that the historicity of species does not settle the ontological status of species. Individuals and historical natural kinds may not contrast as sharply as do individuals and atemporal natural kinds, but they nonetheless denote different ontological categories, where an entity falling under one category will have a fundamentally different nature than an entity falling under the other (R. A. Wilson 2005, 115-118; but see Boyd 1999, 162-163). We should thus follow the lead of Jack Wilson (1999, 62) and Joseph LaPorte (2004, 17), each of whom implies that for the individualists to establish that species are bona fide individuals distinct from historical kinds or "mere" particulars of some other sort, they must show species have the kind of cohesion constitutive of individuality. Many individualists seem to accept this burden and presume their arguments can bear it (e.g. see Holsinger 1984, 296; Williams 1989; Brogaard 2004, 228; Horvath 1997, 657; J. Wilson 1999, 84, 53; Hull 1976, 183).

Thus, on the one hand our critical focus upon species cohesion will gain salience in light of recent clarifications of the individualists' competition. On the other hand the generally accepted significance of cohesion for individuality will entail that our clarifications of species cohesion will block the species as individuals view from going through.

Given the import of the individualists' claims about species cohesion, it is remarkable that individualists have complacently helped themselves to an unanalyzed notion of cohesion. I shall offer textual evidence of such complacency below, but I want to note here that even critics of the species as individuals view have likewise not paid the attention to cohesion that they could have. Marc Ereshefsky (e.g. 2001, 112-119) and Mishler and Brandon (1987, 399-400) raise initial doubts about the individualists' appeals to cohesion, but do not have the space within their respective works to thoroughly pursue these doubts. Our focus upon the nature of species cohesion and the nature of cohesion more generally will help fill an intriguing lacuna in the literature. Additionally, the critical attention we pay to the gene flow account of species cohesion will allow us to contribute to recent criticisms and suggested revisions of the biological species concept (BSC), which enshrines the gene flow account and is the leading proposed solution to a species problem that is closely related to the species ontology problem. This additional species problem concerns the problem of defining the term "species," and we can recognize it as the *species definition problem*. As we proceed I shall further explicate this problem and its connection to the species ontology problem, while also further explaining how the BSC attempts to solve it but will fall short of doing so without undergoing revisions.

Summing up the issues upon which we will focus and the conclusions we shall draw, our first aim is to deflect the individualists' arguments against the species as kinds view through discussion of revised notions of natural kinds. Our second and main aim is to undermine a key argument upon which the species as individuals view depends, through focus upon largely unanalyzed notions of cohesion, and upon the relation between species cohesion and gene flow.

These aims are demanding. One unfortunate consequence of pursuing them carefully is that we shall not have the space to discuss all of the implications that successfully reaching our aims may have for the many ways in which authors think the species ontology problem connects with other issues in biology and philosophy. However, our more focused discussion will be a fertile one and will afford some extrapolation in the final chapter. I have already suggested that our discussion will gain traction within debate over the BSC, for instance, and to now get a broader appreciation of our discussion's potential to be fertile, consider the following ways in which authors have claimed that the individuality thesis connects with ancillary issues in biology and philosophy.

The individuality thesis has been a resource for authors holding multi-selectionist views within the "levels of selection debate." For example, when arguing that natural selection operates on numerous "agents" (e.g. genes, organisms, groups, species) across the biological hierarchy, Stephen J. Gould (2002, 595-741) appeals to the individuality thesis to show that species have the ontological status required to be agents of selection. This manoeuvre exploits a tight link that authors see between agency and individuality more generally: many authors think agents just are individuals. Moreover, given this tight link the individuality thesis' pronouncements on individuality have also shaped more general conceptions of biological agency and, in turn, the way we think of the causal structure of the world that agents help constitute. It is thus not surprising that the view that species (and even whole clades) are individuals plays an important role within Gould's multi-selectionist thesis *as well as* within his view of the structure of evolutionary theory, and the structure of the living world more generally (also see Ghiselin 1997).

It is not just in an indirect manner, via the levels of selection debate, that the individuality thesis has come to bear upon conceptions of agency and the living world's causal structure. Authors directly discuss agency and the living world's causal structure as fundamental issues in their own right and the individuality thesis has had a role to play within these debates as well. For example, two competing views of agency and the living world's causal structure are "pluralism" and "integrationism," respectively. Pluralists such as Brogaard (2004) have co-opted the individuality thesis whilst arguing that the species as individuals view *entails* that a plurality of species concepts are compatible with one another even though each concept posits a distinct definition of "species." On Brogaard's pluralism the species category is a hodgepodge of different kinds of entities, including species defined by gene flow, species defined by their niches, and so on.

Backed by the individuality thesis, pluralism challenges an integrated view of the living world's causal structure in more general ways as well. Consider that R. A. Wilson's (2005, 236-237) integrated view gains plausibility from the special role that he accords organisms within the evolutionary hierarchy, but that the individuality thesis casts doubt on the organism's ability to anchor an integrated view of the biological world. The individuality thesis implies there is nothing particularly unique about the ontological status of organisms. Species are just like organisms, for instance.

The individuality thesis has also featured in fundamental debates over the nature of individuality. Jack Wilson (1999), for example, discusses how the species as individuals view might shape our conception of individuality, and since the concept of individuality gains traction in numerous debates far beyond the disciplinary boundaries of biology (e.g. see R. A. Wilson 2004; 2005), so too does the individuality thesis have potential significance beyond these boundaries.

As we leave the bounds of biology and the philosophy of biology, we might be surprised to find that the individuality thesis has also had a role to play within philosophy proper, such as within recent arguments for how we should conceive of substance concepts and the natural grounds of induction. Millikan (2000, ch.2), for instance, appeals to the individuality thesis when arguing for a substance concept that softens traditional views of both kinds and individuals and which can support inductive inferences in a way that comports with current scientific practice.

One can even find the individuality thesis as far afield as moral philosophy. Authors have suggested that the individuality thesis and ontological status of species bear upon the conceptions of human nature that ground numerous ethical theories, for example. Such theories include Kantian duty ethics and Aristotelian virtue ethics. Sober (1980) mentions how these theories would suffer if the individuality thesis is correct to imply that people are not human beings in virtue of shared natures, but rather in virtue of their perhaps unique roles as parts in larger species individuals.

Even theorists studying ontogeny and psychological development have not escaped the putative scope of the individuality thesis. Morss (1992) argues that if species are something like individuals, there cannot be Piaget-type laws of child development.

Certainly, however, it is upon species debates that authors have thought the individuality thesis bears most visibly. Aside from connections between the individuality thesis and multi-selectionism that Gould and others draw, some individualists also claim that the individuality thesis helps to underwrite radical shifts in the way we classify living forms. In distinct ways, de Queiroz (e.g. 1992) and Ereshefsky (2001) have suggested that we abandon the Linnaean classification system altogether because it embodies a species as kinds view that the individuality thesis rejects; they then suggest replacement classification schemes that they think comport with something like the species as individuals view. Of course, the taxonomic revolutions that authors think the individuality thesis motivates would have widespread trickle-down effects, changing the way we calculate biodiversity, conserve species and draft species-at-risk legislation.

The individuality thesis and the species ontology problem more generally, then, connect to numerous issues in biology, the philosophy of biology, and beyond. Our narrower focus upon species cohesion and the options for revised species as kinds views will help those who wish to properly conceive of and investigate these connections.

Having now an idea of the aims and salience of the upcoming arguments, the rest of this first chapter sets about the spadework that those arguments require. Much of this will involve gaining a more detailed appreciation of the two views that compete to solve the species ontology problem. Thus, after clarifying basic terminology and presumptions surrounding the species ontology problem, I shall explicate the Aristotelian foundations for the species as kinds view (section 2) and then detail the Darwinian foundations for the species as individuals view (section 3). An appreciation of evolutionary theory will allow us to sharpen our characterization of individualists (section 4), and then focus upon the portions of their parity thesis argument that deal with boundaries and continuity (section 5) and cohesion (section 6). The chapter closes with an outline of the overall argument of *Species of Biology* (section 7).

2. ARISTOTELIAN BASES FOR SPECIES AS KINDS

Within the biological sciences, there are two general usages of the term "species" and unclarities can arise if we conflate them (Williams 1992). *Species taxa* are individual species, whereas the *species category* is the category to which (traditionally) all species taxa belong. So for example, *Acer rubrum* is a species taxon, whereas in the Linnaean classification hierarchy the species category marks a category distinct from "genus" above and "subspecies" below, and subsumes all species taxa including *Acer rubrum*, the red maple.

The species ontology problem is thus a problem about the *ontological status of species taxa*. My initial summary of the problem via the question "what sort of thing is a biological species?" is simply a colloquial way of putting the more technical question "to what ontological category does a species taxon belong?" David Hull (1987, 171) neatly summarizes the importance of controversy over such ontological division when he notes, first, that science is largely in the business of discovering the world's causal regularities, and, second, "if we insist on dividing up the living world inappropriately, we will not discover the operative causal regularities."

What ontological categories we recognize depends, of course, upon the metaphysical framework we employ, and (the realist hopes) upon the categories that actually exist in the world. Generally, all parties to the debate over the species ontology problem are realists in at least a broad sense, so throughout much of my discussion I take this starting point for granted, raising local anti-realist concerns only when relevant. Nevertheless, the ontological categories to which a realist subscribes can vary and might include natural kinds, nominal kinds, relations, aggregates, sums, substantial individuals, etc. The species as kinds view, of course, proffers that each species taxon falls into the first category in this list and the individuals view opts for the last. I make the perhaps controversial assumption that each species taxon has the same ontological status as every other, and will not comment on this further except tacitly in later chapters while suggesting that there are bases for an integrated view of species rather than a pluralistic one.

Individualists tend to credit Aristotle with being the first person to develop a theory of natural kinds (though Aristotle did not use the term "natural kind") that attempted to explain the ontological division of living forms. (For complications in determining which of the many natural kinds theories Aristotle actually held, see papers collected in Gotthelf and Lennox 1987 and Gotthelf 1985; and for the minority view that Aristotle did not think species were natural kinds at all, see Balme 1987). The

"Aristotelian" or "traditional" species as kinds view that individualists target (e.g. see Ereshefsky 2001; Hull 1965; 1973 39-40; Mayr 1982; Ghiselin 1997) is one that views the notion of a natural kind as emerging from the relation between *substance* and *essence*. Let us take each of these two notions in turn.

In Aristotle's *Categories* (2b7), substances come in primary and secondary varieties, where organisms are paradigm examples of the former and species are paradigm examples of the latter. Primary substances such as organisms are the world's "true" subjects, the individuals of predication that are not themselves predicated of other things. If Spot, for example, is an individual dog then he is a primary substance, where "dog" is predicated of him and where the reverse cannot hold: Spot cannot be predicated of dog. Dog is not a Spot.

On the other hand, as natural kinds and secondary substances, species can be the subjects of predication in addition to being predicated of other things. Species are subjects, for example, when a genus is predicated of them (e.g. *Canis* is predicated of the species *Canis familiaris*). Nevertheless, organisms ground the system of predication so that genera are not so much kinds of species as both they and species are kinds *of organisms*. It is only in virtue of primary substances, such as organisms, that both species and genera exist (*Categories* 1a25 and 2a6). This is one sense in which primary substances such as organisms are the "true" subjects of the world, or substances "most of all" (2a13).

Widening the gap between primary substance and secondary substance in this way, however, leaves somewhat unclear the status of secondary substances, and so the status of natural kinds and species too. Upon developing a more robust notion of substance in *Metaphysics* that indeed drives an ontological wedge between species and primary substances, Aristotle repeatedly suggests that, as natural kinds, species are not true substances but are instead "universals" (Tweedale 1987; e.g. see *Metaphysics* 1034a6-8). What Aristotle means by "universal" is a vexed question that scholars debate, but given his continued rejection of Platonic forms it seems universals are at least *not* abstractions that exist independent of substances, space and time. Rather, it seems best to think of universals and so natural kinds simply as "numerically many" (Tweedale 1988). In other words, species are naturally distinct *collections* of true substances.

One further point on the relation between primary substance and species. The privileging of organisms as primary substances in Aristotle's system does not imply that species as natural kinds are "just another kind," i.e., a kind that is ontologically equivalent to a genus. The species is a privileged kind in that an organism's species is what that organism *fundamentally* is (*Metaphysics* 1029b15; *Generation of Animals* 767b32-767b33; J. Wilson 1999, 28-29). For Aristotle, all substantial individuals have a single fundamental nature and other kinds (e.g. genera) to which it belongs are more inclusive, or "above" its fundamental kind in the hierarchy of natural kinds (*Categories* 1b10; Ereshefsky 2001, 46-48). This helps ensure for Aristotle that the living world layers into a hierarchical structure of natural kinds that is *integrated* (e.g. see Mitchell 2002, 66) and in which the species category has primacy. Aristotle is committed to the view that species are the fundamental kinds of organisms because of his essentialism—the view that every individual is essentially of the species it is (Furth 1988, 62; Lowe 1989, 5).

As we broach the issue of essentialism I want to go slowly, since the essentialist doctrine presumed to lie at the heart of the theory of natural kinds is what critics of the species as kinds view most directly fix their critique upon. This essentialist doctrine says that an organism belongs to the natural kind it does because that organism bears the essential properties that define or individuate the kind. We might say there are four criteria that any putative essence must meet.

First, many think that essences must be *intrinsic* to their bearers. To see this, consider that Spot belongs to the natural kind *Canis familiaris* because he has intrinsic properties that *cause* him to be a dog. Many authors interpret this to mean that substances, like Spot, have intrinsic "causal powers" that ensure those substances play some active role in the world's causal processes, rather than being merely passive to causal laws that impinge from without (Ellis 2002, 35-38). The intrinsic essential properties that individuate the natural kind *Canis familiaris*, and which are properties of Spot, could be morphological or genetic properties of some sort. Both are intrinsic in the sense Aristotle requires.

Second, it is traditional to think of essences as *necessary* properties that kind members bear. If the properties essential to dog-hood are morphological or genetic, being necessary for membership entails that all dogs have these properties. And since the division between kinds of species is natural, only dogs have these properties. Necessary properties contrast with accidental ones. For instance, the property "being white" does not make Spot a dog. Rather, Spot is accidentally white and he could have been brown and yet still a dog. If, on the other hand, Spot did not have the essential, say, morphological properties defining *Canis familiaris*, he would not be a dog.

Necessary properties seem crucial to a theory of natural kinds because a property's being necessary for kind membership implies that a member has that property *by its very nature*, thus forming a basis for distinguishing kinds as natural rather than merely nominal. Necessity ensures that natural kinds have a ground in nature that supports successful inductions, thus distinguishing them from kinds defined by properties that merely correlate by coincidence. There are at least four different notions of necessity though. Three of these notions of necessity, namely *metaphysical, logical* and *linguistic* necessity, each imply that a kind member will have its essential properties in all possible worlds (Ellis 2002, 14-18). The fourth notion of necessity, which I call *natural* necessity, is less strict and implies only that essential properties are had by necessity in the actual world. Individualists presume that essences are metaphysically necessary, so that with respect to a given thing they are the "properties or structures in virtue of which it is a thing of the kind it is, and which it could not lack, or lose, while still being a member of the kind" (14-15). By contrast, logical necessity is grounded in the "meanings of the connectives and operators of...language"; linguistic necessity is grounded in the "meanings of words" (15); and naturally necessary properties are properties that come to define a kind in virtue of a contingent series of events that could have played out otherwise.

The third criterion of essences is *immutability*. The idea that a natural kind's definitive essences are unchanging ensures that natural kinds are part of the fixed causal order of things (R. A. Wilson 2005, 11). Thus, the essential properties individuating *Canis familiaris* always have and always will be the essential properties of domestic dog-hood. This seems to comport with the necessity criterion of essential properties, since it is *prima facie* difficult for the essential properties defining a kind to be capable of change if successive members must always have just those properties by necessity.

Finally, essential properties are *explanatory* in virtue of featuring in laws of nature. Because essential properties cause kind members to be as they are, they can explain the nature of kind members. This includes explaining the sorts of relations into which individuals can enter with individuals of their kind and of other kinds, because the essential properties that individuate natural kinds relate with each other in "regular" causal ways. Moreover, all individuals are individuals of some kind and thus all individuals are caught up in these regularities (i.e. there are no "bare particulars"; see Furth 1988, 62; Lowe 1989, 5). It is not hard to see why authors often refer to these regularities as laws of nature and as laws that are exceptionless, given that the essential properties realizing them are immutable and had by metaphysical necessity. In short, if laws of nature are indeed exceptionless, the *explanatory* nature (criterion 4) of *intrinsic* essential properties (criterion 2) owes in large part to those properties being *immutable* (criterion 3) and *necessary* for kind membership (criterion 1). On the realist's view of the world then, scientists are in the business of uncovering these essential properties and taxonomizing them in a system of corresponding natural kinds because essential properties and the relations between them feature in laws of nature. Natural kinds are grounds for our inductions and explanations.

This look at the traditional theory of natural kinds puts us in position to summarize the traditional species as kinds view. As a natural kind, a species is individuated by immutable essences that are intrinsic to all of its organisms and which are explanatory so that the species features in laws of nature that we can discover.

3. DARWINIAN BASES FOR SPECIES AS INDIVIDUALS

Let us now turn from Aristotle to Darwin, as a grasp of the standard view of the theory of evolution by natural selection will be crucial for gaining an appreciation of the individualists' bases for rejecting the Aristotelian view of species and for advancing the species as individuals alternative.

For brevity we can begin with "the three Darwinian principles of evolution by natural selection," which I paraphrase from Richard Lewontin's entry on evolutionary theory in the *International Encyclopaedia of the Social Sciences* (1968; also see Lewontin 1970, 1):

1. *Phenotypic Variation*: Organisms in a population vary inherently with respect to phenotype, e.g. morphology, physiology and/or behaviour.
2. *Differential Fitness*: Variable phenotypes correspond to variable levels of fitness in different environments, where fitness refers to survival and reproduction rates.
3. *Heritable Fitness*: Phenotypic variation and so fitness are heritable.

As Lewontin says, "While [the principles] hold, a population will undergo evolutionary change." (1970, 1; we should add the caveat that the population will change if other evolutionary mechanisms do not "cancel out" the effects of natural selection). Roughly, this is to say that the frequencies of phenotypic traits among organisms in a population will change. It is now standard (though not without controversy) to express this as "change in allele frequencies." Though allele frequencies will change in a population when the three Darwinian principles hold, these gradual (or anagenetic) changes constitute microevolution until there is a speciation event, where (on the standard view) a population becomes reproductively isolated from the parent species (Meier and Willmann 1999, 31-32). Species comprise populations and thus even just microevolution in a population implies that a species is evolving (Futuyma 1998). That is, species can evolve and remain the same species, or they can "bud-off" new species. In sum, the fundamental idea that the three principles capture is that natural selection is a main cause of change in a population's phenotypic trait or allele frequencies.

Consider the evolution of allele frequencies for *saliva type* in the goldenrod gall fly. Variation in the saliva of these flies (*Eurosta solidaginis*) induces variation in the size of the protective "galls" (globular growths as large as golf balls) in which each larval fly matures within the stem of a goldenrod plant (see principle 1) (Futuyma 1998, 424; see Weis et al. 1992).

While in the gall, the fly is vulnerable to parasitoid wasps (e.g. *Eurytoma gigantea*) that pierce the gall formation with their ovipositors and deposit eggs, the hatchlings from which will kill the larval gall fly within the protective gall. The wasps pierce smaller galls more successfully, thus variation in fly saliva that produces variation in gall size correlates to variation in fly fitness (see principle 2). Flies that have a saliva type that induces larger galls are fitter than those inducing smaller galls.

Notice that for there to be variable fitness, flies need not struggle against each other in what we consider a direct sense. There need only be a struggle to reproduce and/or survive, as there is when flies that vary with respect to saliva try to out-live the next wasp infestation.

Now, because phenotypic traits like saliva-type are heritable (in this case via alleles or genes for saliva type), so too is fitness heritable (see principle 3). Over generations (of short duration from our perspective), fitter flies will tend to have more success at surviving and passing on their phenotypes so that the phenotypic make up of the population of flies will change on the whole. Allele frequencies for saliva type will change such that the population evolves. Put another way, as the frequency of certain saliva types increases in response to the wasps' preference for small gall formations, the phenotypic make up or allele frequencies of the population change.

Notice: it may seem that saliva-type frequencies in the fly population could reach an equilibrium (e.g. all flies have saliva that produces large goldenrod galls) so that the population is no longer evolving with respect to that trait, but in such a case natural selection is still operating. It *maintains* the equilibrium (Griffiths 1999, 220). Moreover, populations often experience changing environments that preclude such equilibrium (Futuyma 1998, 424). Lewontin mentions "different environments" in the second Darwinian principle to account for this and we see it vividly in the case of larval goldenrod gall flies, for while the flies are within their galls they are also vulnerable to woodpeckers that actually prefer larger galls, unlike the wasps. Thus, as population densities of woodpeckers and wasps fluctuate, so too will the direction in which natural selection operates with respect to saliva types and gall sizes. So long as there is the potential for environments to change and the populations under those environments comprise organisms that vary inherently, there will be the potential for evolution.

A number of points relevant to species ontology now present themselves. The first has to do with variation. As the first Darwinian principle indicates, conspecifics vary inherently, i.e. they vary by their very nature. One reason a population of gall flies changes with respect to saliva type frequencies is that there is no single, natural saliva type. Rather, there is a natural tendency for variation in saliva types and this variation is that upon which evolution works. Indeed, advances in genetics since the modern synthesis (i.e. the joining of Darwinian evolutionary theory with statistical Mendelian genetics) seem to show that variation of a genetic sort "goes all the way down," so that there is no single gene that all conspecifics share necessarily. It is difficult to overestimate the impressiveness of such variation. For example, many gene locus can be constituted by multiple distinct alleles, and with organisms typically having thousands of gene loci, the number of possible combinations or "unique genotypes" within a species is massive. Even if we presume simplistically that each gene loci can differ on account of just two alleles that are thus able to form either a homozygous dominate, heterozygous, or homozygous recessive pair, then with 3000 loci that are potentially variable as such (the estimated number in each human), the number of distinct genotypes possible equals 3 to the power of 3000, or as Futuyma (1998, 244) says, "an unimaginably large number." Simply put, following studies in the 1970s, we realized species to be "far more genetically diverse than almost anyone had previously imagined" (244), and that there is sometimes even more genetic variation within a species than between species (Lewontin 1972).

That genetic variation seems "to go all the way down" is just one reason for being cautious of proposals (e.g. Lambert et al. 2005) to determine conspecificity through appeal to short, species-specific sequences of DNA known as "species barcodes." Species barcodes may be imperfect tools that can *guide* decisions about the species to which an organism belongs. But the phenomenon of inherent variation ensures it is problematic to presume that any sequence of an organism's DNA is *essential* (in the traditional "Aristotelian" sense) to its being of the species it is, or that any such sequence is identical to the analogous sequence in all conspecifics.

Moreover, it takes little reflection to see that the inherent variation within a species challenges the traditional species as kinds view. For there is now ground for doubting that there are any intrinsic properties that all conspecifics necessarily share. This violates the necessity criterion of the essentialist doctrine that underlies natural kinds theory. Moreover, the mere fact that species change at least forces re-examination of the immutability criterion. The arguments against the species as kinds view go beyond mere appeal to inherent variation and species evolution though. Generally, individualists mount a four-fold attack against the traditional species as kinds view, where they direct each of the four arguments at one of the four criteria of essentialism and then conclude that species are not kinds because no definitive aspect of essentialism applies to them. Given evolutionary theory, it seems that all four aspects of essentialism fail to apply to species.

As it is the job of the next chapter to show there are ways of liberating natural kinds theory and so the species as kinds view from the four traditional criteria of essences, such that species need not satisfy a strict essentialism in order to be natural kinds, I set aside further explication of the individualists' arguments against the kinds view until then. Delaying discussion of the individualists' negative arguments will allow us to draw out in the remainder of this section the Darwinian bases for their positive arguments. This will guide our explication of that argument in sections 4-6 below.

The phenomenon of inherent variation that we have just introduced can help draw out these bases because it connects with reasons for treating species as spatially contained entities that authors liken to individuals.

Consider that appreciation of the inherent variation among conspecifics motivated evolutionary biologists to reduce their focus on individual organisms and, especially, to reduce their focus upon *explaining away* the variation between organisms. Inherent variation directs biologists to "ascend" to the population level of organization to study how variation "is lawful and causally efficacious." (Sober 1980, 369) Rather than explain away variation, variation *does the explaining*. Variation frequencies explain why populations are as they are. Ernst Mayr (1959) aptly coined this change in focus the ascension to "population thinking" and as a result of this ascension we now treat populations as units of organization that have properties that are not reducible to the organisms they comprise. "The population is an entity, subject to its own forces, and obeying its own laws." (Sober 1980, 370) This is visible in our gall fly example where it is the population of gall flies that has certain allele frequencies, not any single fly. And these frequencies are of import when tracking the population's evolutionary change within selection processes. Some authors infer from this that populations and species have a certain *agency* that is not reducible to organisms, which we did not appreciate prior to Darwin. As Hull (1981, 146) says, "[s]pecies are the sorts of things which evolve, split, bud off new species, go extinct, etc." On some views then, species seem to have a more robust status as entities than do the "collections" that the traditional species as kinds view envisages. But to what does such status amount?

For one, being entities that participate in the evolutionary process suggests that species have boundaries of some sort. Or at least they are "spatiotemporally restricted," as the second Darwinian principle hints with the mention of environments. To explain, consider that temporally, species seem to come into being (e.g. when new reproductive isolating mechanism between sub-populations of gall flies arise and constitute a speciation event) and go out of being (e.g. when all gall flies have died) (but see Mayr 1982, 286-297; Ruse 1987). Additionally, they are spatially restricted entities in that the spatial location of the organisms constituting them can help determine the identity of a species. For instance, if a population of gall flies is split into two, perhaps because of a massive campaign to expunge goldenrod plants that leaves one gall fly population north of the plant-free zone, and another to south of it, then each new population might face significantly different selection pressures. An unusually large woodpecker population might hamper the southern fly population, while wasps are the significant issue for the northern one. In any case, selection pressures could vary to the point that substantial allele frequency changes occur and the populations become reproductively isolated from each other (i.e. allopatric speciation). Such a speciation event owes in part to geographic isolation, or in other words, to spatial location. So, species seem spatiotemporally restricted or bounded in a way.

Moreover, species seem *continuous* in at least a temporal sense, since they each remain the same species from speciation to extinction. Insofar as authors take conspecifics to be defined in part by their ability to exchange genetic material, one begins to see how species continuity has a spatial aspect as well (Mayr 1942). On standard allopatric speciation models it is just when geological barriers disrupt this continuity that the separated populations are likely to evolve "away" from each other as separate, reproductively isolated entities.

Allusion to reproductive isolation also brings out the sense in which biologists take species to be bounded not just spatiotemporally, but in an evolutionary sense. Biologists take variation within a population to be "lawful" in its own right (in part) because that variation is *contained*, or in Sober's (1980) terms, the variation within the population interacts causally with selection forces in a manner separate from the variation within other species. Importantly, this sort of evolutionary boundary—often cashed out in terms of a "closed gene pool"—is not really a physical boundary that one could, say, touch. It is, I think, a metaphorical boundary, though one that might rightly point to how species are set off from one another in an evolutionary sense. At least, this notion of a boundary seems to suffice for most biologists to refer to species as "evolutionary units" (see Ereshefsky 1991 for discussion and complications). As many authors do, I refer to the evolutionary *unity* that this implies species have as *species cohesion* (see Ereshefsky 1991 for references). More precisely, species cohesion is the causal unity a grouping of organisms has when the organisms are similar such that they tend to share an evolutionary fate (for precursors to this formulation, in which I hope it is clear that "fate" refers to evolutionary outcomes and not mysterious orthogenesis, see Ruse 1987, 353; Ereshefsky 1988, 429; 1991, 89; Williams 1989, 301). At first blush, this construal of species cohesion is rather simple. It refers to the causal unity a species taxon embodies that marks it out as an evolutionary unit, that is, *as a* species that is distinct from other species taxa and from both higher (e.g. genera) and lower taxa (e.g. varieties). However, we shall come to appreciate the complexities of the notion.

This look at how Darwinian evolutionary theory shapes our conception of the nature of species will now allow us to appreciate the species as individuals view more thoroughly than my introduction in section 1 allowed. Specifically, we can elaborate upon two of the view's aspects that I briefly discussed there. The first aspect is the sense in which I said we should follow the lead of Jack Wilson (1999, 62) and Joseph LaPorte (2004, 17) in distinguishing between a weak and a stronger version of the species as individuals view, where the weak version merely claims species are historical entities of some sort and the strong version claims species are bona fide individuals demonstrating boundedness, continuity and cohesion. To do this we shall take a brief tour through the history of the individuality thesis that will further justify the distinction I endorse and help clarify the target of my argument. The second aspect of the species as individuals view that our Darwinian insights will help us to elaborate upon is the parity-thesis-strategy of the individualist and its focus upon boundaries, continuity and, especially, cohesion. Completing the first of these tasks of elaboration should help with the second.

4. INDIVIDUALISTS AND INDIVIDUALS

The original architects of the individuality thesis, Michael Ghiselin (1974) and David Hull (1976, 1978), built the thesis upon an exhaustive metaphysical distinction between *classes* and *individuals*, which, in light of the supposed strength of the arguments against the kinds view, made the species as individuals view virtually irresistible. Consider David Hull's (1978, 294) introduction to his seminal paper articulating the individuality thesis.

> The only category distinction I discuss is between individuals and classes. By "individuals" I mean spatiotemporally localized cohesive and continuous entities...By "classes" I intend spatiotemporal unrestricted classes, the sorts of things which can function in traditionally-defined [that is, exceptionless] laws of nature.

Hull also clarified in the footnote of his earlier (1976, f.n.9) paper that his category distinction reflects "a particular philosophical outlook...which is a lineal descendant of logical empiricism." With such a philosophical outlook, the argument of the individuality thesis is deceptively simple. As Gary Borjesson (1999) notes, it has the form of a disjunctive syllogism, where the first premise is that species can only be one of two sorts of things, such that if the second premise rules one of those options out, we must conclude that the other option is true. It is relatively uncontroversial that species cannot be classes, if classes are strictly spatiotemporally unrestricted entities (but see Kitcher 1984). We have seen that on evolutionary theory species are indeed spatiotemporally restricted. Given the disjunctive syllogism then, species must be individuals. Recently, and thirty-one years after proposing the idea that species are individuals, Ghiselin (1997, 302, ch.3-4) has restated this disjunctive syllogism in roughly the same form.

A number of authors seem to have suspected that this sort of argument rests on a dubious categorical distinction and as early as 1994 Kim Sterelny claimed that confusions surrounding the distinctions between, and notions of, individuals and classes were "widely recognized" to have vitiated early debate surrounding the individuality thesis (10). But this, I think, is a generous characterization of all parties to the debate. It is only recently that authors have clarified suspicions of a simplistic categorical distinction (Keller et al. 2003, 94). A few of these clarifications have amounted to explicit rejections of the class-individuals distinction (e.g. Keller et al. 2003; Borjesson 1999; Stamos 1998), while others have implicitly challenged the distinction (e.g. LaPorte 2004, 10ff.; Millikan 2000, ch.2; Boyd 1999; R. A. Wilson 1999; Griffiths 1999; Griffiths 1997).

One general problem that such recent work raises, and one which I alluded to above, is that on the original class-individuals distinction, individuals are simply spatiotemporally restricted entities and this is a category of ontologically disparate entities. On this view, for example, mere spatiotemporal regions around which we have some reason for drawing a boundary count as individuals (see Quine 1981, 10). Also counting are only slightly more robust mereological sums that are not causally or spatially connected (e.g. my big toe and your pinky finger). Heaps, piles and aggregates count, of course, even though there are not causal interactions between their components. And finally, more recent theories of natural kinds, such as LaPorte's (2004) "historical kinds," would imply that some natural kinds count as individuals too (see Millikan 2000, ch.2 for a related "lumpy" notion of "substance").

Without going into detail, I shall submit that there are *prima facie* striking and important ontological differences between the sorts of entities listed above and that it is misleading and uninformative, at best, and simply wrong, at worst, to say they all count as individuals. One of the basic presumptions of my arguments is that mere spatiotemporally restricted entities are not individuals. Individuality is a vexed notion, but it is uncontroversial to hold that, *minimally*, individuals are necessarily cohesive entities (van Inwagen 1990, 81). If we construe cohesion very generally for now as a property an entity has in virtue of its components being causally unified, then claiming that individuals are necessarily cohesive is to claim they comprise causally unified parts. This rules out as individuals Quinian space-time regions, mereological sums, aggregates, heaps, and piles. As the metaphysician Michael Slote (1979, 388) puts it in his analysis of individuality, the term "individual" is one "that clearly applies to tomatoes, rocks, and chairs, and…clearly does not apply to magnetic fields, shadows, bodies of gas, or piles of leaves." (For other metaphysicians supporting some conception of individuals as causally unified, see Armstrong 1980; Shoemaker 1979; Ayers 1999, 229-253; van Inwagen 1990; Wiggins 1980).

Admitting cohesion as a necessary criterion of individuality makes the original disjunctive syllogism of Ghiselin's and Hull's a lot less compelling. To show that species are individuals it is no longer adequate to point out that they are historical entities with temporal beginnings and endings. Only a few individualists still maintain this weaker thesis in any case and thus I shall bracket out these few from the group I refer to as individualists, and consider them as arguing for a view of species as historical entities that may, in the end, be compatible with revised species as kinds views. To be an individualist, one must hold that cohesion is a necessary condition of individuality. Notably, this manoeuvre may seem to imply that Ghiselin is actually not an individualist. Along with his recent restatement of the original class-individual distinction, he has clarified (1997, ch.4) that he believes cohesion is not a necessary condition of individuality. Moreover, he has insisted (52) that while in conversation with Hull, Hull has bruited the same opinion.

There are grounds for still considering the original progenitors of the individuality thesis to be individualists though. For instance, Ghiselin (ch.4) *does* think cohesion is *sufficient* for individuality, and that species *are* cohesive, and that these two points coupled together form one of the strongest reasons for considering species to be individuals. Thus, my critique of species cohesion will apply to Ghiselin to some extent. As for Hull, despite his conversations with Ghiselin, much of his published work implies he actually does consider cohesion to be a necessary condition of individuality that species do display, such as when he says "integration by descent is only a necessary condition for individuality; it is not sufficient. If it were, all genes, all organisms and all species would form but a single individual. A certain cohesiveness is also required." (Hull 1976, 183; see also 1976, 177; 1987, 172; 1999, 32) In an insightful paper (1984) that discusses species cohesion, Kent Holsinger supports my interpretation that Hull and

Ghiselin have cohesive individuals in mind, when he summarizes on behalf of them and individualists more generally that cohesion is necessary for individuality:

> Quine (1960, pp. 170ff.; 1981, p.10) has suggested that we "admit as object any portion of space-time, however irregular and discontinuous and heterogeneous"...The proponents of the view that taxa ("species" is the term used in their discussions) are individuals, however, mean something more by their assertion. There is nothing in Quine's definition of a physical object that would prevent us from recognizing as an individual physical object the group composed of lions, trout, dandelions, and fruit flies. But the physical object so composed would not be recognized by a systematist as a taxon. Similarly, the physical object so composed is not an individual in the sense intended by [individualists]. [T]axa are those physical objects (*sensu* Quine) such that an individual organism is a part of a whole...that exists as a discrete unit, complete unto itself and coherent...Taxa are not merely collections of physical objects, they are collections that have a certain degree of internal structure and organization. The individual organisms that are part of a taxon interact with one another in a variety of ways. They cohere... (296-298).

Other parties to the species ontology debate who explicitly agree with Holsinger on the necessity of cohesion include Mayr (1987, 155), Ereshefsky (1988, 432; 2001, 112-119), Sober (1993), Williams (1985, 1989), Splitter (1988), Horvath (1997), de Queiroz (1999), Brogaard (2004, 228), Mishler and Donoghue (1982), Wiley (1981), and Eldredge and Gould (1972).

So to be an individualist is at least to view species as the sorts of individuals that are necessarily cohesive. But for the individualist, what else must an individual be? There is not a well worked out answer to this question. It is only recently that some authors writing in the species ontology literature have shown they appreciate, as Aristotle discovered in his quest for primary substance in *Metaphysics*, just how thorny the notion of individuality is. Jack Wilson (1999), Judith Crane (2004) and Ronald de Sousa (in press), for example, have tried recently to clarify individuality within a biological context. J. Wilson's effort constitutes an entire book, but in a subsequent paper

(2000), he admits the book only began to clear the brambles, and even failed at key aspects of that.

The following list of eight different ways that authors within the species ontology debate have described the sorts of individuals they take themselves to be discussing reflects the trouble that individuality has caused authors:

- "physical individuals" (Gayon, 1996; Stamos 1998)
- "substantial beings" (Borjesson 1999)
- "individuals with respect to evolutionary theory" (Williams 1989)
- "concrete particular persisting individuals" (Crane 2004)
- "physical objects" (Holsinger 1984)
- "causally integrated individuals" (Ereshefsky 2001; and see Mayr 1987; Sober 1993)
- "biological individuals" (implied in de Queiroz 1999)
- "spatiotemporally localized cohesive and continuous individuals" (Hull 1978)

There are potential disparities lurking in this list. However, it seems that all of the authors (and descriptions) listed agree or at least see their notion of individuality as compatible with the last description in the list, that of Hull (1978). Repeatedly, authors cite the three definens of *boundedness* (i.e. being localized), *continuity* and *cohesion* as necessary conditions of individuality, though the exact nature of each is often left ambiguous.

Our Darwinian insights into the nature of species and the fact that individualists discuss this *tripartite conception of individuality* in the context of their parity thesis will help us to clarify the ambiguities. And although boundedness and continuity are not foci of this work, it will pay to try clarifying the individualists' appeals to each in the next section, for we should have in place a rounded appreciation of the species as individuals view. Moreover, in the last chapter I shall briefly return to our understanding of the boundedness and continuity of species while suggesting that there are intimate and important connections between all three "conditions" within the tripartite conception of individuality.

5. BOUNDARIES AND CONTINUITY

The individualists' parity thesis implies that the boundaries, continuity and cohesiveness of species are *just like* the cognates of each in organisms, or those in paradigm individuals more generally. I take this to imply that differences with respect to these three things can only be of degree, not kind, if species are to be individuals in virtue of being just like paradigm individuals.

Given what evolutionary theory says about species boundaries though, it is not easy to clarify what the individualist means by organismic boundaries and species boundaries being the same in kind. The general problem is that species seem *spatiotemporally restricted*, but that this seems something short of the *continuous physical boundaries* we take organisms and other paradigm individuals to enjoy. This problem is especially acute in light of the nature of species continuity. Both boundedness and continuity have synchronic and diachronic dimensions though, and the problems seem less immediate in the diachronic instances. Thus, to state the parity thesis, let us consider boundaries and continuity together, first in their diachronic senses, and then in their seemingly more complex synchronic senses.

We saw that on evolutionary theory a species has a beginning and (probably) an ending in space and time and that these spatiotemporal "boundaries" in part define that species. Species *Y*, for example, might have resulted from one of three sorts of speciation events. First, species *X* might have given way to both species *Y* and species *Z*, such that *X* ceased to exist. Second, *Y* may have "budded off" from *X* in an allopatric speciation event, such that both *X* and *Y* still exist. Third, *X* may have ceased to exist as it graded into *Y*. In all of these cases there is a time at which *Y* began and many individualists think that *that* origination event is a necessary property of the species, such that organisms looking and behaving identical to members of *Y* would nonetheless fail to be members of *Y* if they existed prior to the speciation event that produced *Y*. Thus, diachronically, all species are strictly spatiotemporally restricted.

The diachronic spatiotemporal restriction of species also helps species to satisfy the diachronic continuity criterion. For if species are strictly restricted as such, they seem continuous through time and space from their speciation to their extinction. Alluding to alien tigers can help exemplify the link between restrictedness and continuity. If we discovered that a species remarkably similar to tigers had evolved on another planet, and some of the alien tigers were brought to earth and were even able to successfully interbreed, share a niche, or share a mate recognition system with earthly tigers (*Panthera tigris*), the alien tigers would nonetheless fail to be members of *Panthera tigris*. Interbreeding, niche sharing and mate recognition system sharing each represent interactions between conspecifics that, within competing definitions, are said to define "species." Nevertheless, the alien tigers are not "parts" of the spatiotemporally delimited earthly tigers. They are not part of the continuous entity *Panthera tigris*.

This, of course, is a conceptual claim and though many accept it, conceiving of species as restricted and continuous as such is not free from empirical challenges. Some exemplary empirical challenges include fuzzy boundaries and polyphyletic taxa.

The spatiotemporal beginnings of species are fuzzy, for example, in that speciation processes can span over long periods, where it is perhaps impossible to pinpoint a moment at which the species is "born." A similar problem arises on some definitions of species which seem to only apply at a time, or over narrow time intervals, such that it is conceptually impossible to tell retrospectively whether a population today is a part of the same species that existed in the past (see Mayr 1982, 286ff.; Splitter 1988). However, these problems are not unique to the individualist about species. Problems of identity over time plague our thinking about paradigm individuals too. And although organisms and other paradigm individuals have *relatively* less fuzzy beginnings (and endings), the difference between organismic and species fuzziness seems, *prima facie*, one of mere degree rather than kind. In principle, for example, at which temporal point we say that a mammalian zygote becomes distinct from its mother is no clearer than when it is that species *Y* budded off from species *X*. Further, despite the fuzziness in each case, there are clearly times and spaces where a given species or organism exists and others where it does not.

Polyphyletic taxa are *actual* analogues of the alien tiger scenario. Polyphyletic taxa emerge when natural selection or chance genetic events produce, at different times and places, groups that seem to be of the same taxon, where members from each discontinuous instance of the taxon are virtually indistinguishable and can, say, interbreed, share a niche, or share a mate recognition system. For example, one type of goatsbeard, *Tragopogon mirus*, seems to have "arisen independently in several localities" (Futuyma 1998, 507) and this phenomenon is widespread among plants, some fish, frogs, weevils, grasshoppers, salamanders and lizards (504-505).

If we subsume polyphyletic groups of, say, goatsbeard under a single species, then it seems we sacrifice the spatiotemporal restrictedness of species and deny their continuity. The standard way to overcome this problem, especially since the phylogenetic revolution in taxonomy that aims to map the one true history of life, is to say that such independently evolved groups are indeed different species, as alien tigers are not members of *Panthera tigris*. This may seem ad-hoc in light of definitions of species that imply that organisms from polyphyletic taxa *are* conspecifics. However, it may be that the species definitions need reworking. Numerous phylogenetic species definitions propose just such revisions. Furthermore, even subsuming polyphyletic taxa under one species does not, as mentioned, raise problems for the spatiotemporal restrictedness of species that are unfamiliar to organisms. We think organisms have particular spatiotemporal beginnings and that they are continuous through time, but if a person undergoes a successful heart transplant where the heart comes from an older person, we seem inclined to say that the heart becomes a part of the person who had the heart transplant. If we do, then it is no longer clear that all of the young person's parts are continuous through time and that the organism is unambiguously bound across space and time. *Why* we are willing to say the "new" heart is a part of the person is a further issue that should become clear in subsequent chapters. But summing up for now, there seems at least a plausible case for saying that species and organisms are similar in kind in terms of diachronic spatiotemporal restrictedness (boundaries) and continuity.

Synchronically speaking, I have said that matters are murkier. To get a better sense of the possible conceptual gap between species boundaries and organismic boundaries, recall that we saw that the theory of evolution by natural selection *does* suggest that different spatiotemporal selection regimes shape species (e.g. in the case of gall fly populations being geographically isolated and then diverging) such that species will be spatiotemporally restricted "at a time." Such restrictedness is synchronic in the sense that we could freeze in time the relevant spatiotemporal evolutionary processes that ensure species are spatiotemporally restricted, and then mark out the corresponding location of a species that in part defines what it is for that species to be the species it is. To see, however, how such spatiotemporal restriction seems quite different from the *continuous physical boundary* that most organisms enjoy, consider the following improbable scenario that illuminates the continuous nature of an organisms' boundary.

If we were painting the outer surface of an organism, to finish the job the brush would not have to lose contact with the organism. Openings into the body cavity of the organism do not pose a problem; the boundary is still continuous as indicated by our ability to paint around such gaps without lifting the brush. In short, the surface is continuous and thus so is the boundary. The organism you have in mind might be a person, but the organismspainting scenario seems to hold for slime mold zooids, insects, crustaceans, bacteria and raspberry plants too. It also seems to hold for other sorts of "individuals" that metaphysicians discuss, such as chairs, stones and lumps of gold.

(Note: one might say some "corporate organisms" or "superorganisms" lack the sorts of continuous boundaries that typical organisms do, but the status of these entities as organisms is far from clear; see R. A. Wilson 2005, 80-84, 155-157; Turner 2000).

We could not paint the spatiotemporal restriction of a species, on the other hand, without "lifting the brush from the species." Most species do not have a continuous surface, so to speak. Here is where we see the issue of synchronic boundedness connecting with that of synchronic continuity. It seems an organism has a continuous physical boundary just because its parts are *materially continuous*. In the case of an organism, such continuity refers not to mere contact, as (say) leaves in a mere pile are in contact, but at least to attachment of some sort. Of course, not all the parts of an organism attach to each other. Yet they seem continuous in at least a serial manner. From a cell in my big toe, to one in my baby finger, there are continual lines of attachment one could draw. Because of such material continuity, the organisms' outermost parts, which have a portion of their surface not continuous with any other of the organisms' parts (e.g. the surfaces of the skin cells exposed to the air on the back of my hand), will *collectively* form the organism's surface or continuous physical boundary. The *boundary* is continuous just because there is *continuity* among the parts.

Now, without such continuity among the conspecifics that make up a species, the species has no continuous physical boundary. If we attempt to paint the boundary around one "part" of the species (i.e. a certain organism) and then extend it in a continuous manner to include another nearby "part" (i.e. another organism), we will have to arbitrarily included space within the boundary that lies between the two conspecifics, and within which there are no parts of the species that are even serially attached (or, for that matter, necessarily in contact). That is, we shall have to deny material continuity and admit arbitrary "parts" that in principle are not parts. The only way to avoid this is to lift the paintbrush, such that each of the species' parts (e.g. organisms) is enclosed in its own boundary. But then the species does not have a continuous physical boundary. It seems, then, that synchronically speaking, a species cannot have *both* the continuous physical boundary an organism has *and* the material continuity an organism has.

Given these considerations, to state plausibly the individualists' argument with respect to synchronic boundedness and continuity, we seem forced to choose from two options. First, it may be that organismic boundaries and continuity as I have described them are indeed different from species boundaries and continuity, but only different in degree, not kind. Given the distinctions I have highlighted, this seems forced. However, one would need to argue further than I will here to conclude that the differences are indeed of kind, as the individualist cannot allow. Likewise, individualists need to specify further the nature of the distinctions here.

The second option is to admit differences of kind, but hold that the kinds that *species* display are the kinds that individuality requires—organisms simply exceed the requirements with more "impressive" kinds of boundedness and continuity. This may well be the case; perhaps "continuous physical boundaries" and "material continuity" are more than an entity needs in order to be an individual. Notice, however, that deflating a difference in kind in this way takes us away from the parity-thesis-strategy.

To go this route, the individualist must appeal not to organisms but to some independent, perhaps abstract concept of individuality along with the "lesser" notions of boundedness and continuity to which it corresponds. This is just the thorny path individualists wish to avoid when utilizing the parity thesis in the first place. Thus, there are options for the individualist here, but they require further work. Let us leave that work for the individualists and now turn our attention to the third definens of the tripartite conception of individuality.

6. SPECIES COHESION, INTERBREEDING AND GENE FLOW

As with boundaries and continuity, individualists claim that species cohesion is the same in kind with organismic cohesion, or more generally that species cohesion is the kind of cohesion that is constitutive of paradigm individuals. Yet individualists fail to explain what it is for cohesion to be of *that* kind in a way that would justify the equation they draw. I refer to this as *cohesion complacency*. To be clear, this does not imply that individualists have not discussed species cohesion at all. Indeed, they are relatively clear that species cohesion generally refers to the tendency for conspecifics to be similar so as to share an evolutionary fate, as I mentioned in section 3. Individualists have also said much about which causal, biological interactions and processes they think are responsible for species cohesion, and this will help us show that individualists unwittingly have a certain kind of cohesion in mind when discussing species cohesion. Rather, what individualists are complacent about is the more metaphysical nature of cohesion: they generally have not investigated the nature of the instances of cohesion to which they allude. Marc Ereshefsky (1991, 97) is observant of such complacency as well, saying, "the cohesiveness which Hull and Williams attribute to species is... some sort of uniformity [and] Hull (1976, 1984) provides no further information about the nature of this uniformity." In chapter three, one of my key claims will be that there are two distinct kinds of cohesion and that individualists have overlooked this because of their cohesion complacency. (Note: Mishler and Brandon 1987, 399-400, and Ereshefsky 1988, are the only individualists to mention, in passing, that distinct kinds of cohesion have been overlooked) Thus, cohesion complacency is one of the very roots of the problem I have identified and am trying to solve. This makes it important to substantiate here that cohesion complacency is typical of the literature on species ontology.

The sheer number of terms that authors allow "cohesion" to stand in for is a first sign that cohesion complacency is pervasive. For example, authors frequently slide between reference to cohesion and reference to a host of other terms, including "internal organization," "integration," "unity," "uniformity," and "causal interactions." That the meanings of each of these terms are not obviously equivalent suggests "cohesion" may be doing double duty, or worse. Even if we allow "cohesion" to subsume all of these notions, the initial distinctions between the referents of the terms in the list are at least suggestive of their being different kinds of cohesion. It may even be that *each* of the terms in the above list is general in a sense and subsumes different kinds. Take, for example, the notion of "integration." When Ghiselin (1981, 271, my emphasis) writes that the parts of an individual are "integrated in one way or another—joined as by *physical* or *social* forces or common descent [i.e. *historical* forces]," there should be at least an initial suspicion that physical, social and historical forms of integration may each be distinct in kind from one another.

There are more overt sorts of cohesion complacency. For example, consider that Hull (1999, 32) says, "[m]ost organisms do exhibit more internal organization than most species, but this difference is one of degree, not kind," and that he then offers little or no argument for thinking the difference is one of degree. In exploiting the parity thesis as he does, Hull recognizes that species cohesion is different from organismic cohesion, but he uncritically makes a claim about the nature of this difference. He does mention that some plants (i.e. organisms) lack impressive internal cohesion and yet are still individuals and that, therefore, species too can lack the cohesion of vertebrate organisms and still be individuals. However, any argument for why even unimpressive plant cohesion is the same in kind with species cohesion is wholly lacking. In an earlier paper Hull (1987, 172) manifests the same complacency, noting, "spatiotemporally organized entities can be arrayed along a continuum from the most highly organized to the most diffuse. Organisms tend to cluster near the well-organized end of the continuum." He offers no justification for a continuum-view rather than the view that different kinds of cohesion are at work.

As we saw above, Holsinger is an individualist who does say a little more about the nature of cohesion, yet his complacency is evident all the same. Consider his following statement on page 296 of his (1984):

> ...an individual organism is not just a collection of individual organs. It is a tightly organized, homeostatic system in which these individual organs interact in complex ways to ensure the survival of the organism. In much the same way, a taxon is not just a collection of individual

> organisms. It is composed of individual organisms that interact with one another, are related to one another in particular ways, and participate in biological processes in similar ways.

Following this passage, Holsinger never explains why the way in which organisms "interact with one another" is the same in kind with how "individual organs interact in complex ways."

Another way in which authors discuss the nature of cohesion is via the "tearing apart test," where entities that suffer more than others when torn apart presumably exhibit a greater degree of cohesion. Cohesion complacency surfaces in this context as well. For example, Berit Brogaard (2004, 228-229) has recently written that

> [m]ost species taxa can withstand some disruption of their population structure but some cannot. Conversely, most organisms cannot continue to exist if their internal structure were moderately changed; but other organisms can withstand some tearing apart. But notice that these may be differences of degree, not kind. If so, then these differences need not suggest that species taxa could not belong to the same ontological category as organisms.

Brogaard offers no significant reasons for thinking differences *between species and organisms* are of degree, not kind. Although he carefully implies that the nature of species cohesion "need not" entail that species and organisms are of different categories, other individualists do not hedge the claim so much. From the assertion that organismic cohesion and species cohesion are same in kind, for example, they conclude, "organisms and species (along with genes and cells) are members of the same general category of individuals." (de Queiroz 1999, 67)

Now, to help us grasp the general direction my overall argument will take while clarifying the confusion over cohesion, let me expand upon some of the passing remarks I have made until now about the nature of cohesion—a task we will take up more judiciously in chapter three.

If we take cohesion to be a property of entities that refers to the causal unity the entity's parts display, as I have suggested we do (see J. Wilson 1999, 52 for a similar suggestion), then what is responsible for cohesion is the causal interactions among the entity's components. The idea of causal interactions being "responsible" for cohesion is one we shall have to later make more precise, but having said this much, it is clear that different sorts of causal interactions among components might be responsible for different sorts of cohesion being realized as a property at the "higher level" of the entity. The key question is whether different "sorts" of operative causal interactions correspond to different "kinds" of causal interactions. If they do then these different kinds of operative causal interactions will be responsible for different kinds of cohesion.

Even though individualists have been guilty of cohesion complacency and do not themselves spell out the exact way in which causal interactions among components relate to the resulting cohesion of the entity comprising those components, many of them appreciate that causal interactions as such are somehow what "keep species together" (Dupre 1993, 46; and see e.g. Ereshefsky 1991, 97; 2001, 29; Hull 1976, 177; Holsinger 1984, 296; Ghiselin 1981, 271). It is in this sense that individualists *do* discuss the biological interactions and processes responsible for species cohesion. Which biological processes and causal interactions are the operative ones with respect to species cohesion is an empirical matter that depends upon competing solutions to the species definition problem, which I have said is distinct from the species ontology problem.

The species definition problem concerns how we define the term "species" such that species taxa are distinct from each other and from units above (e.g. genera) and below (e.g. subspecies) them in the evolutionary hierarchy. Biologists package competing solutions to this problem in one or another *species concept*, each of which derivatively or explicitly implies which biological processes or causal interactions "keep species together" so that each species is set off from other taxa.

Debate over species concepts is fierce and at last count 22 of them were competing in the literature (Mayden 1997). However, by far the most widely accepted of these is the *biological species concept* (BSC), which defines species as "groups of interbreeding natural populations that are reproductively isolated from other such groups" (Mayr 1999, 17). It is standard to interpret the BSC as implying that interbreeding interactions and the gene flow that interbreeding enables are the operative causal interactions with respect to species cohesion. As John Dupre (1993, 46) has put it while exploring the link between the BSC and the individuality thesis, "if species are considered as...individuals, gene flow is what quite literally holds the parts of the individuals together."

Because of the widespread appeal of the BSC, many individualists have presumed that gene flow interactions are *the* causal interactions that are responsible for species cohesion: gene flow is responsible for conspecifics tending to be similar so as to share an evolutionary fate. Though I have suggested that individualists do not investigate what kind of cohesion it is that gene flow interactions enable, while drawing the equation between species cohesion and the cohesion of paradigm individuals a great majority of individualists *have* appealed to the idea that gene flow interactions do hold conspecifics together in *the same way* that parts of paradigm individuals are held together. In short, individualists advance an *argument from gene flow* to support the idea that species cohesion is the kind of cohesion that is constitutive of individuality. The ideas enshrined in the heralded BSC are thus often thought to form the basis for one of the most powerful arguments for the species as individuals view: if species are interbreeding populations connected by gene flow, then surely they are individuals (e.g. Ereshefsky 1988, 97; 2001, 112-119; Hull 1976; 1978; Ghiselin 1974; 1997; Holsinger 1984; Horvath 1997; Mishler and Donoghue 1982; Mishler and Brandon 1987; Crane 2004).

7. THE ARGUMENT'S PATH

Having documented cohesion complacency, and having briefly discussed the nature of cohesion and the relation between species cohesion and gene flow interactions, we can now clarify the most substantive of the two conclusions for which I will argue over chapters three to five.

By attending in chapter three to the unanalyzed notion of cohesion it will become clear that there is a key distinction between two kinds of cohesion. A thorough articulation of the nature of these two kinds of cohesion and the relationship between them will make it evident that one of these cohesions is the cohesion of individuals, while the other is not on its own indicative of individuality. Minimally, species must display the kind of cohesion that is constitutive of individuals if they are to be individuals.

In chapter four, I explain what is involved in applying our analysis of cohesion to the more specific notion of species cohesion and I then range over the leading species concepts in the literature on the species definition problem. This shows that only the BSC and reproductively based views that are similar to it can make even an initial case for the idea that species cohesion corresponds to the cohesion of individuals. However, there are under-appreciated conceptual complexities to the BSC and gene flow based definitions of species. We shall attempt to clarify these so that we see exactly what the individualists' argument from gene flow implies, and that the argument is indeed an argument from gene flow and not an argument from interbreeding. The argument from gene flow is a crucial one for the species as individuals view in light of other species concepts failing to offer an account of species cohesion that helps the individualist.

Chapter five takes direct aim at the argument from gene flow, mounting two objections to it, each of which shows in related ways that gene flow interactions do not have the requisite causal power for "holding species together." The first objection documents the empirical shortcomings of gene flow based views of species. Underappreciated studies show that interbreeding interactions are neither necessary nor sufficient for species cohesion. This casts not only empirical doubt on the adequacy of gene flow views, but conceptual doubt as well, for if gene flow is neither necessary nor sufficient for species cohesion, then it seems we have been conceptually confused to think otherwise. The second objection attempts to explain *why* it is conceptually confused to attribute causal powers to gene flow. Gene flow only comes to matter to species cohesion when natural selection plays a primary role in bringing such cohesion about. But the selection-based cohesion of species does not correspond to the kind of cohesion individuals display. Thus, any time gene flow seems significant, species cohesion will actually owe to selection and will thus be inadequate for the individualists' needs. Since the argument from gene flow seemed the only possible argument for demonstrating that species have the cohesion of individuals and we show that it does not go through, there remains no reason for thinking that species are individuals in virtue of their cohesion. The cohesiveness of species may well ensure that in certain processes, species are biological agents of some sort, but their cohesiveness is not the kind they would display if they belonged to the ontological category "individual."

Finally, the arguments for this conclusion will position us in chapter six to discuss, somewhat speculatively, the implications of our critique of the individuality thesis and focus upon cohesion for issues involving species concepts, classification, levels of selection, and the general relations between cohesion, agency and individuality.

To head, however, towards our main conclusion against the species as individuals view, and towards the implications of the arguments for that conclusion, let us return to the species as kinds view that individualists hope to replace so that we may see which underappreciated options one might have if the individualists' hopes fail to materialize.

2

The Neglect of Natural Kinds

The nature of kindhood is open to revision
in light of scientific advance

Paul Griffiths 1997

1. REVISION POTENTIAL

Not unlike species, theories of natural kinds have evolved and begot new theories of natural kinds. Although it seems Aristotle was the first to flesh out a theory of natural kinds, others after him, especially the 16th and 17th century mechanists and the 19th century naturalists, proposed marked revisions that resulted in distinct conceptions of natural kinds (Ayers 1981; Hacking 1991; Winsor 2003). Different conceptions of natural kinds do not necessarily conflict with each other, however. A number of authors have argued convincingly that within "a" theory of natural kinds "there are different categories of natural kinds" (Ayers 1981, 269; also see Ruse 1987; Boyd 1991; Griffiths 1997, ch.8; Hacking 1991). So not only is it misleading to speak of "the" theory of natural kinds, but also confused to presume that any theory of natural kinds necessarily contains a singular concep of kindhood.

In their arguments against the traditional species as kinds view, individualists neglect the fact that there are many and variable conceptions of natural kinds, all of which are open to revision. Such neglect has blinded individualists to the facts that the traditional species as kinds view they target is not the relevant one and that there are numerous resources at the ready within current natural kinds literature for bringing the species as kinds view in line with evolutionary theory. Or so I shall argue in this chapter. Since I think the root of the individualists' neglect is a failure to appreciate the *revision potential* of natural kind theory, let me say more about such potential.

We should not be surprised that theories of natural kinds have revision potential; they are amenable via critique-and-revise processes, as are many theories. Thus, when Locke critiqued the Aristotelian notion of natural kinds in his influential *Essay*, a number of Lockean advocates took up a revised theory of natural kinds that claimed our abilities to discover natural kinds are more limited than Aristotle presumed and that if there are natural kinds individuated by essences, the essences are not intrinsic Aristotelian forms, but instead are microstructural properties of corpuscles (Kornblith 1993, ch.2; see Ayers 1981, 260 for the view that Locke's real essences were only a rhetorical device).

There are, however, at least two deeper reasons why theories of natural kinds have proven to have impressive revision potential. First, because scientists set out to discover and study natural kinds, we should allow that science is an important arbiter on our conceptions of natural kinds. Paul Griffiths (1997, 212) puts this more pointedly in the quote at the head of this chapter. We might also think of such revision as one example of how critique-and-revision processes often span across disciplines and help ensure the possibility of fruitful multidisciplinary study. What Griffiths has in mind specifically is that both theory and practice in biology imply that biological kinds are of a different nature than, say, chemical kinds. The former have boundaries that are not as sharp as the latter, for example (211-213). Taking these biological insights back to the philosophy of science can inspire revisions of metaphysical views, which in turn might be taken back to science proper, in order to help work out conceptual problems there. Such potentially productive multidisciplinary instances of critique-and-revision, along with scientific realism's growth in the wake of logical positivism's decline, give us added incentive to entertain science-inspired revisions of the traditional view of natural kinds that individualists censure.

The second deeper reason why natural kind theories have impressive revision potential is that while science and realism drive such revisions, at a more general level it is powerful and widely held common sense conceptions that support theories of natural kinds. There is thus an obligation at least to attempt to revise a conception of natural kinds to reconcile it with new scientific findings, before abandoning altogether the application of natural kinds in a given instance. Not only did Aristotle begin building his metaphysical framework, essentialist doctrine and theory of natural kinds upon a common sense conception of nature (Pellegrin 1982), but recent studies in anthropology and ethnobiology underscore the universality of these conceptions. For example, no matter the vast differences in culture, history, and/or language, people around the globe tend to ground their inferences about the biological world in roughly the same hierarchy of biological kinds (Berlin 1992). And for the city slicker in Michigan or the bush dweller in Guatemala, the "species" level of the hierarchy has primacy and picks out roughly the same level of biological organization as does the scientist's "species" (Atran 1999). That natural kinds theory comports with common sense so impressively cannot prove the truth of a species as kinds view, but it at least helps to explain why authors throughout history have allowed that theories of natural kinds have revision potential upon being challenged. And it suggests that individualists entertain the species as kinds view more carefully than, as we shall see, they have.

To show that individualists have neglected natural kinds I will argue for the plausibility of a "revised" species as kinds view while deflating the individualists' four arguments against the species as kinds view. More specifically, each of the individualists' arguments targets a different one of the four criteria of essences from which many authors have thought traditional natural kind theory springs. The general problem with the negative portion of the individuality thesis that these four arguments compose is that it commits the species as kinds view to a conservative brand of essentialism to which it need not commit. Rather than conceiving of essences as intrinsic, necessary, immutable, and explanatory in an exceptionless-law sense, there are good reasons for liberating essentialism so that essences can define evolving, spatiotemporally restricted species and support successful, if not exceptionless inductions involving them.

My deflationary argument here will raise two questions that I plan to leave largely open, though I shall say a word about each now. First, it is an open question whether deflation of the individualists' negative argument amounts to a rejection of it. On the one hand, my deflation will *not* consist of a fully worked out conception of species as kinds and I will suggest that further work is required to accomplish this. On the other hand, however, the deflation *will* show that individualists neglect plausible, revised conceptions of natural kinds and that, therefore, they must regroup and offer new arguments. Within some corners of debate over the species as kinds view, we shall see that a cluster of authors has explicitly tried developing a revised notion of natural kinds and thus has already provoked individualists to regroup. We shall also see though, that individualists have generally not even engaged this cluster of authors. Regardless of whether shifting the burden to the individualist amounts to a "rejection" of the negative portion of the individuality thesis, it should become clear that the thesis' positive portion, which already rests so heavily on the notion of species cohesion, must be strong indeed if we are to abandon the long standing and resilient natural kinds view, and its revision potential, in favour of claiming that species taxa belong to an altogether different ontological category.

Mentioning that individualists have failed to respond to authors selling a revised notion of natural kinds helps reveal the second open question I will comment on here. With at least some authors explicitly bypassing the traditional kinds view in favour of revised ones, we begin to wonder whom it is that individualists target with their negative thesis. Put differently, who, if anyone, holds, or ever did hold, the "traditional" species as kinds view?

Although I have allowed that the "traditional" view might be Aristotelian, a number of authors now argue that even Aristotle's essentialist doctrine was not nearly as conservative as the one that individualists bemoan (e.g. see Balme 1987; Ellis 2002, 14; Sober 1980). In fairness to the individualist, this may indeed be a reflection of recent scholarship, and perhaps the pre-Darwinian naturalists, whom individualists often explicitly single out for stalling an "accurate" view of species ontology and taxonomy, *did* operate with a different and stricter "Aristotelian" conception of species as kinds. Hull (1965) and Mayr (1982) have told such a story, and if correct, the individualists' negative arguments have a valid target.

Unfortunately, historians of science have recently shown that the Hull-Mayr version of taxonomic history is likely a "myth" (Winsor 2003). It may be (though we do not know) that pre-Darwinian naturalists had a strict essentialist *world view*, but in any case such a view seems to have seldom permeated to their taxonomies, since their *methods* corresponded remarkably with the sorts of revised and liberated notions of natural kinds that metaphysicians have only recently begun to articulate (e.g. Boyd 1999). More specifically, in the colonial 18th and 19th centuries, traveling naturalists were "discovering" biological diversity so rapidly that past taxonomies were being contradicted. To make sense of the influx of anomalies, new methods of collection, description and naming were devised. These methods promoted the frequent exchange of "type" specimens in such a manner that "essentialistic" types became less important than open and malleable descriptions (Muller-Wille 2003). Type specimens became mere means for keeping names in order and naturalists no longer considered the characters of the type specimen to be necessary for membership in the species it represented.

Furthermore, the trends that revised 18th and 19th century taxonomic methods embodied remain enshrined in present day codes of nomenclature. This surprises those who are unfamiliar with using the term "type specimen" to refer not to essentialistic types, but rather to "a concrete abstraction in nearly diametrical opposition to earlier...conceptions and practices linking individual specimens with the type of the species" (Daston 2004, 158). Currently, these historical findings and contemporary usages are corroborating Richard Boyd's (1999, 145) suspicion that the sorts of strict criteria that I said are characteristic of the "traditional" natural kinds theory that individualists target, are representative of an essentialism that actually stems from "a profoundly outdated positivist conception of kinds."

So the positivist and her essentialism may be the target of the individualist. If so, the individualist's target does not seem to include the current philosophers of biology, biologists and pre-Darwinian naturalists against whom individualists take themselves to be arguing. Though we shall not delve further into historical scholarship here, the four deflationary manoeuvres to which I now turn should further support the views of Boyd and the above-cited historians, and in an indirect manner, further support the claim that individualists have chosen their target poorly.

2. THE ARGUMENT FROM NATURAL LAWS

Understanding how to resist the individualists' negative *argument from natural laws* can help legitimate resistance of the other three negative arguments, and so we shall begin with it. Baldly stated, the argument from natural laws points out, on the one hand, that essential properties are supposed to instantiate causal regularities such that the natural kinds they individuate feature in laws of nature, while noting, on the other hand, that species do not seem to feature in any laws of nature. The inference is then that species must not share essential properties as traditionally conceived. Therefore, species are not natural kinds (Ghiselin 2002, 154; Hull 1978, 309).

As other authors have noted, this argument trades on a specific conception of natural laws, where such laws hold universally over members of a kind. I shall call these *exceptionless natural laws*. That individualists have such a conception of laws in mind—one that rules out mere "generalizations" as laws—is clear because everyone agrees "there are certainly true generalizations about the members of species" (Dupre 1993, 40). Moreover, individualists would certainly not help themselves if they counted as natural laws such uncontroversial generalizations as "all elephants have trunks" and "fire ants reproduce stable nest cultures in virtue of environmentally transmitted pheromones." (See Keller and Ross 1993 for evidence that the reliable replication, across generations, of nest cultures in the fire ant *Solenopsis invicta* is mediated by the reliably replicated pheromonal contexts in which queen ants develop).

Fortunately for individualists, it is relatively uncontroversial that species do *not* feature in thoroughgoing, non-trivial exceptionless natural laws; and most think that paradigm natural kinds such as the chemical elements *do* feature in such laws. Let us contrast polar bears (*Ursus maritimus*) and silver (Ag) to see this.

We can make many generalizations about polar bears. Polar bears have a common anatomy that distinguishes them from even the largest of bears in other bear species. For instances, polar bears have longer necks and comparatively elongated heads. Polar bears also tend to share behaviours and abilities that, when compared with other bear species, are unique to them. They can swim for miles in icy waters, for example, and tend to be the only kind of bear that will stalk humans as prey (though there is some evidence that male black bears occasionally do so; attacks by grizzlies are not thought to be predatory in nature). Nonetheless, these generalizations are not exceptionless laws. They do not necessarily hold across all polar bears, or they often hold to varying degrees. A polar bear may have a natural and life-long circulatory deficiency that ensures it cannot retain its inner heat for long stretches in icy waters. Or it may, by chance, lack the webbing in its feet that enables most polar bears to swim well. Nonetheless, a deficient polar bear is still a polar bear and the generalizations regarding its species seem prone to exception.

On the other hand, there are statements we can make that feature the capacities and behaviours of silver that seem far more robust than generalizations about polar bears. For example, silver conducts electricity and indeed seems to do so better than most other materials. Silver is malleable and soft and is chemically incompatible with certain other elements and compounds, such as ammonia and hydrogen. Unlike in the case of polar bears, these statements about silver seem to hold across virtually all instances of silver. Generalizations about silver seem not mere generalizations, but something more like exceptionless natural laws. Moreover, this seems to owe to the fact that silver, like other elements, is defined by essential properties that are characteristic of all silver atoms, the most obvious one being that all silver atoms have 47 protons. If an atom does not have 47 protons, it is not a silver atom. With polar bears seeming not to feature in exceptionless natural laws, the inference that the argument from natural laws trades upon is that polar bears do not have essences as do atoms of silver, and that this is why they do not feature in law statements as silver does. Chemical kinds are natural kinds, but biological species are not.

Ghiselin (2002) makes use of this reasoning in his own version of the argument from natural laws. Given the exceptionless conception of laws that the contrast between polar bears and silver brings out, he seems justified in saying that "[i]f species were natural kinds, there would have to be at least one law of nature for at least one of them. No legitimate example has been presented." (154) Moreover, with the prospects of a sudden discovery of an exceptionless natural law about species seeming bleak, it seems species are not natural kinds defined by essential properties. Indeed, if we take the inherent heterogeneity (embodied in the first Darwinian principle of chapter one) of species seriously, it may be down right *impossible* for species to feature in an exceptionless natural law: any deep similarities between conspecifics are merely contingent and are not exceptionless universal features of them.

Of course, the way to resist this argument from natural laws is to deny or supplement the conception of natural laws on which it depends. The clearest way to do this is to insist we should not deny law-status to common and reliable generalizations. Such generalizations, or "law-like" statements as other authors refer to them, may not be exceptionless but they nonetheless underwrite inductive success and are what the explanations of many scientists traffic in. Law-like generalization derive from causal regularities in the world and indeed hold regularly; and essential properties need only feature in these *regular natural laws* (as I shall call them) in order to satisfy the key criterion that essential properties are explanatory. On this looser conception of an essence, essential properties still *explain* why the individuals bearing them *tend* to move through the causally integrated world as they do. In fact, on the sort of revised species as kinds view I wish to make plausible, the fact that essences remain explanatory in a scientifically interesting way is crucial to their ability to individuate what we should still consider to be natural kinds.

The best way to fill in this resistance to the argument from natural laws a little further, i.e. to argue for regular natural laws, is to appeal to the notion of counterfactual force. Griffiths (1999, 216) rightly points out that counterfactual force is "the key feature of a law of nature...because it explains how laws differ from mere widespread coincidences," but he also thinks our "mere" regular natural laws (e.g. those about species) have this requisite counterfactual force.

Statements have counterfactual force and are of a regular (and perhaps sometimes even an exceptionless) sort when the theories yielding them license their corresponding conditionals. For example, it may be true that all populations of *Ursus maritimus* have individuals that weigh less than 1 000 kg. But nothing in our biological theories licenses the conditional, "if this *were* a polar bear, it *would* weigh less than 1 000 kg" (for a similar example, see Griffiths 1999, 216). Thus, the conditional has no counterfactual force and is not a law statement.

Griffiths (1999, 216-217) gives a nice summary of why the notion of counterfactual force associated with law statements generalizes easily to regular natural laws, and he insists that the corresponding natural kinds still have teeth and admit of successive revisions that further secure their naturalness:

> Minimally, any generalization that is a better predictor of phenomena than a suitably designed null hypothesis has some counterfactual force. This allows us to frame a minimal conception of naturalness for kinds. A kind is (minimally) natural if it is possible to make better than chance predictions about the properties of its instances. Surprisingly, this utterly minimal conception of a natural kind is not toothless. It does not license the conclusion that any way of classifying nature is as good as any other. Natural kinds are ways of classifying the world that correspond to some structure inherent in the subject matter being classified. They contrast to arbitrary schemes of classification about which the nominalist claim that the members of a kind share only a name is actually true. Furthermore, the minimal account of naturalness lends itself to successive restrictions that allow us to distinguish between kinds of greater or lesser naturalness and hence of greater or lesser theoretical value.

Certainly this liberated conception of natural kinds and laws broaches a number of epistemological questions, such as "how can we know which of two competing taxonomies of purported natural kinds to go with?" Rather than take these up, I want to mention that others have done so with some success (Griffiths 1999, 217-219; Kornblith 1993; LaPorte 2004) and turn next to make a related point, which is this. If such epistemological problems surface in the biological and special sciences when we countenance a notion of kinds that will account for the centrality of regular natural laws, then these epistemological problems surface in *all* the sciences and in many of the instances in which we presume scientists are studying natural kinds. This is because, as Nancy Cartwright (1990) has argued, exceptionless natural laws are rare.

Cartwright's (1990, 54-73) work on laws in physics compels us to admit mere regular natural laws as central to our inductive successes if we want there to be laws at all, since even many law statements in physics are false (and so of course not exceptionless) if we take them to be describing actual phenomena. This is because the laws instantiating causes of these phenomena are usually many, and yet they do not all add together in a neat componential way that our law statements track (see 67-69).

Causes entwine, in a sense, and when they do our law-statements often fail. Laws thus only hold "all things being equal," and yet all things are seldom equal (Dupre 1993, 41). As a result, many of our putative laws of nature in our "most" scientific domains seem prone to exception. Thus, come what epistemological problems may, we should not discriminate against regular natural laws that have counterfactual force and admit of exceptions. Nor should we discriminate against the sorts of natural kinds that feature in such *ceteris paribus* laws.

On the one hand, then, I am suggesting that we admit counterfactually robust generalizations as laws that our knowledge of natural kinds (like species) can explain. On the other hand I am suggesting that these are the sorts of laws that "robust" natural kinds feature in anyways.

To make the first suggestion more vivid, consider that even though generalizations about polar bears do not hold without exception, they certainly have counterfactual force. Our knowledge of them, for example, ensures we have license to say, "if this bear is a polar bear, it can swim through the icy water from here to there," or, "if this bear is a polar bear, then it will have a relatively longer neck than that grizzly bear." One's odds of being right when they utter these counterfactuals are far better than the mere chance with which Griffiths contrasts reliable generalizations.

To make the second suggestion more vivid, consider that the seemingly exceptionless laws within which silver features may not be laws about silver, and/or may not be exceptionless, as we first presumed. Saying that silver conducts electricity, for example, has to do with the essence of metals more generally, not with the essence of silver *per se*, and this, I suspect, will be the case for many of our generalizations about silver (and about polar bears for that matter, i.e. generalizations will turn out to involve bears or mammals more generally). And as a result of silver occurring in two relatively equally represented isotopes in nature, each of which can demonstrate nuclear behaviour that varies dramatically, some of the statements we might think hold over all instances of silver will not (Faure and Mensing 2005). For example, nuclear decay differs depending upon the isotope, and though isotopes of the same element generally exhibit similar chemical and physical properties, this is not always true either. The deuterium isotope of hydrogen has twice the atomic mass of hydrogen's protium isotope and thus deuterium reacts much more slowly than protium, even though both have the same essential property: having one proton. Because hydrogen is especially vulnerable to this *kinetic isotope effect*, many generalizations about the way in which hydrogen reacts will seem to be *ceteris paribus* laws.

Granted, it may be that chemical kinds feature in more reliable laws than species do, but they are nonetheless *not* exceptionless. Thus, there is a real issue for the individualist about why we are to draw the magical "natural law line" below the chemical kinds but above species. If statements about neither are exceptionless, why discriminate against species? Griffiths, we saw, at least gave us a good reason for not discriminating against species. Moreover, if we move from chemistry to other scientific fields that are even "more comparable" with biology, the issue for the individualist becomes more pressing. Scientists studying geology and meteorology, for example, presume (at least implicitly) that they are studying natural kinds such as "continental plate," "earthquake," "atmospheric pressure," and "tornado," each of which features in no more impressive "law-like" statements than "polar bear," and yet individualists have not attempted to show the presumptions of these scientists are wrong. Once removed from the rhetoric of the species ontology debate, I doubt they would try.

If we admit generalizations as regular natural laws and accept that properties of conspecifics need only instantiate these rather than more formidable exceptionless natural laws in order to be explanatory essences, then the individualists' argument from natural laws loses its sting. A short hand way of putting this is to say that essential properties *need to be explanatory*, and *need not determine exceptionless laws*. As we shall see, explanatory essences as such marry well with revised notions of natural kinds, and this in turn will aid the deflation of the individualists' negative thesis.

3. THE ARGUMENT FROM THE MERE FACT OF EVOLUTION

The individualists' second negative argument homes in on the putative immutability criterion of essential properties. Though seemingly simplistic, Philip Kitcher (1984, 319) thinks it is fair to characterize the argument as follows. Species evolve; natural kinds are atemporal entities; hence natural kinds cannot evolve through time; therefore, species are not natural kinds (for slightly expanded versions of the argument see Hull 1978, 299-300; 1981, 146; Ghiselin 2002, 153; 1981, 304).

The simplicity of this argument is deceptive though and parties to debate over species ontology have yet to make clear that there are two distinct ways of interpreting it. On the one hand, it may be concerned at bottom with anagenesis (species change, where a species evolves but may remain the same species) and the mutability of essences. Call this the *mutability version* of the argument from the mere fact of evolution. On the other hand, the argument may be concerned with cladogenesis (species change, where one species begets another or others) and the supposition that natural kinds are spatiotemporally unrestricted universals. Call this the *historicity version*.

In at least one sense, the immutability criterion of essences is the easiest for a friend of the species as kinds view to refuse to commit to, since even if it was a feature of the traditional view of natural kinds, many essentialists have seemed happy to give it up. As Robert Wilson (2005, 11) notes, "a modified essentialism about species, one that viewed them as natural kinds, albeit with essences that could change over time, has largely been taken for granted throughout the history of biology and philosophy." (also see Sober 1980, 355-356) Essences need not be immutable and so species can have essences and undergo anagenetic change. Both Kitcher and John Dupre suggest specific ways of spelling this out and in so doing they imply that natural kinds might still be spatiotemporally unrestricted universals. Kitcher (1984, 318), for example, thinks the mutability version of the individualists' argument does not go through because technical features of set theory show that species can be atemporal "sets" and yet evolve in virtue of comprising successive time slices with different property distributions. This seems to me an unduly complex manoeuvre though, and one that is unlikely to appease individualists who think species are entities without temporal "parts." More simply, Dupre (1993, 40) argues species as atemporal kinds can be consistent with at least anagenetic change by noting that water is a paradigm natural kind and that "all the water everywhere could get hotter and dirtier" and yet we would "not need to say that the kind water has changed." Likewise, all the members of a species in one generation might be different from those in another with respect to, say, saliva type frequencies, perhaps because a spike in the woodpecker population drives the evolution of those frequencies. Yet *Eurosta solidaginis* remains a natural kind.

Dupre's reasoning seems to me sound and in any case he is merely spelling out an idea that, as I have said, many authors implicitly or explicitly accept. It is, then, the historicity version of the individualists' negative argument from the mere fact of evolution that is more formidable. How can new species emerge in the contingent manner evolution suggests if natural kinds are universals that do not emerge nor go extinct? Although Dupre does not distinguish between versions of the argument as I do, he does seem to imply it is unproblematic for natural kinds to "emerge," as in a footnote to his discussion of water as a natural kind he continues on to say that "on most conceptions of species, if *enough* properties change we will have a new species" (40, f.n.5). Furthermore, he is certainly right to recognize the emergence of natural kinds. After all, those paradigm natural kinds the chemical elements had beginnings in time via evolution of a sort as well. Elements of lower atomic number gave rise to those of higher number during fusion reactions following the Big Bang (Rollinson 2001). But to resist convincingly the argument from the mere fact of evolution, we should like some account of what it is for a natural kind to emerge. For example, are emerging natural kinds spatiotemporally unrestricted universals or not?

There are at least two general senses in which a natural kind might emerge, one of which is unhelpful in the case of species and another I do wish to endorse.

The unhelpful notion claims that natural kinds are indeed spatiotemporally unrestricted universals and that species emerge "into" them. On this view, "[s]pecies are not an actual element in the evolutionary process, but a set of abstract types into which the products of that process coincidentally fall." (Griffiths 1997, 204) Whatever philosophical lemmas this account of the emergence of natural kinds might face, it nonetheless does not fit with empirical insights into species. If species are universal types into which the products of evolution fall, the process of evolution is not contingent in the sense that biologists tell us it is. Rather than natural selection corresponding to *relatively* open-ended phenotypic possibilities that are contingent upon actual world factors, presuming that species are universal types implies that the laws of biological form constrain the number of types into which populations can evolve. Yet the "idea that species occur because only a few regions of the space which lineages explore are compatible with the laws of biological form...currently has little empirical support." (204) In short, there has been no dearth of criticism of such typological thinking in the context of species taxa (e.g. see Mayr 1982).

The contrasting account of the emergence of natural kinds I prefer simply denies that natural kinds need be universals of the sort that individualists have in mind. Rather, natural kinds can be spatiotemporally restricted in the sense that speciation and extinction require. It is not clear that this is even a marked revision from Aristotle's species as kinds view. As we say in chapter one, some authors think it is best to conceive of species as "universals" in terms of species being spatiotemporal collections, rather than abstract entities (e.g. Tweedale 1987). However, even if the traditional kinds view did posit a notion of "universal" that ensures evolutionary species could not be kinds, the revision I am proposing still stands. Natural kinds are not spatiotemporally unrestricted universals. Individualists have unfortunately failed to even engage this revision possibility despite the fact that numerous authors working on natural kinds have implied that such revisions are not only possible but desirable in light of scientific advance (e.g. LaPorte 2004, 10ff.; Crane 2004, 165; Griffiths 1999, 219-222). If these authors are correct, spatiotemporal unrestrictedness is not an unavoidable feature of natural kinds that rules out *a priori* the possibility of the evolution of natural kinds.

The most concise explanation of how a natural kind can be spatiotemporally delimited is Joseph LaPorte's: "[a] historical kind would simply be one whose membership conditions involve members having some causal connection to an independently specified item—for example, the beginning of a lineage" (2004, 11). Interestingly, Griffiths (1999) gives this idea of "historical essences" a Darwinian basis when he quotes the following passage from Darwin's *Origin of Species*: "On my theory, unity of type is *explained by* unity of descent" (my emphasis; see reference in Griffiths 1999, 220). If Aristotle was correct to say essential properties *explain why* something is the way it is, and evolutionary theory shows that conspecifics are as they are in part *because* they share a common origin, we have reason to think, as Darwin seemingly did, that historical properties might be essences that define historical kinds.

Griffiths (220) elaborates on the explanatory power of historical essences, noting that

> the principle of heredity acts as a sort of inertial force, maintaining organisms in their existing form until some adaptive force acts to change that form...[This] licenses induction and explanation of a wide range of properties—morphological, physiological, and behavioral—using kinds defined purely by common ancestry.

It seems that kinds of this sort would not be universals in the sense that the historicity version of the argument from the mere fact of evolution requires in order to go through. Whatever his actual conception of "universal," Aristotle himself may have been amenable to the idea that natural kinds "emerge", since, for example, in *Generation of Animals* (II 746a30) he flirted with the idea that new species might emerge from fertile hybrids. Given his rejection of Platonic forms, it is then tempting to presume that he also considered the possibility that natural kinds could be "historical", since presumably types would not exist prior to hybridization. This is one place, then, where Boyd (1999) may be correct to allege that the abstract notion of natural kinds that individualists target belongs to the positivists and not to those the individualists have presumed.

Finally, making the point that there are plausible options for revising our notion of kinds with respect to their spatiotemporal status does not constitute a complete account of historical kinds and so may not *refute* the argument from the mere fact of evolution. Yet at the very least, it suggests individualists have deserted the kinds view too quickly on this score. In the swath of literature on the individuality thesis, Marc Ereshefsky (2001) seems the only individualist to give historical kinds much attention. Unfortunately, this occurs in a footnote wherein he uncritically presumes that explanatory historical properties cannot individuate natural kinds but rather are indicative of a different sort of thing: a "historical entity" (294, f.n.3).

4. THE ARGUMENT FROM INHERENT HETEROGENEITY

The third negative argument we will take up requires the most attention. It is the *argument from inherent heterogeneity* and it claims that species cannot satisfy the stipulation that kind members have their essential properties necessarily.

While introducing the necessity criterion in chapter one, I mentioned that there are at least four sorts of necessity. Three of these are strict in the sense that when a kind member has their essential properties necessarily, they have them in all possible worlds. The fourth notion of necessity, on the other hand, implies less strictly that it is only within the actual world that kind members have their essences necessarily. We also saw that the traditional view of natural kinds presumes that one of the strict notions of necessity applies to essences, namely metaphysical necessity. Metaphysical necessities are "propositions that are true in virtue of the essences of things. Of course, if one does not believe that there are any natural kinds, or if one does not accept that things have essential natures, then one will not believe that there are any metaphysical necessities." (Ellis 2002, 15) In effect, metaphysically necessary essences of a thing are those "properties or structures in virtue of which it is a thing of the kind it is, and which it could not lack, or lose, while still being a member of the kind." (14)

With metaphysical necessity in mind, individualists proceed to observe as we have that conspecifics vary inherently, i.e. by their very nature. The first Darwinian principle of chapter one captures this notion, implying that variation within a species is naturally normal and *not* something a naturalist should explain away. Mutation, drift and (in interbreeding organisms) genetic recombination are just some of the biological phenomena that conspire to ensure members of species are inherently heterogeneous. Because such heterogeneity or variation is inherent, any homogeneity we do find is merely contingent. It might happen that all members of a gall fly population have (say) "gene *X*" (assuming that "gene" refers unambiguously), but on the standard view of evolution this situation obtains contingently, not by necessity. The next breeding season could scramble the gene pool again so that soon there is no gene all flies share; and yet they would all still be gall flies. Thus, with conspecifics seeming to share no phenotypic or genotypic properties by their very nature, that is, by metaphysical necessity, individualists conclude that species do not satisfy the necessity criterion of essentialism, and so are not natural kinds that essences define (Ghiselin 2002, 155; 1974, 539-540; Hull 1978, 304-308; 1965, 205; Sober 1980; Ereshefsky 2001, 98-100).

To resist this argument, I want to show we plausibly can and perhaps should loosen the necessity criterion of the essentialist doctrine in two sorts of ways such that inherently heterogeneous species can be kinds defined by essences in a revised sense.

The first loosening manoeuvre amounts to claiming that essences need only be naturally necessary, not metaphysically necessary. The second manoeuvre involves envisioning essences as clusters of properties, where kind members necessarily have the cluster, but where no single property in that cluster must obtain. Both manoeuvres rely on the fact that essential properties need not be had by metaphysical necessity in order to still be robustly explanatory in the sense demanded by the regular natural laws of section 2 above. To detail these manoeuvres and demonstrate their plausibility, it will pay to better appreciate where the metaphysical necessity criterion comes from.

I implied in chapter one that individualists (and probably many others) interpret the Aristotelian metaphysical framework as giving rise to the metaphysical necessity criterion (e.g. see Hull 1965; 1973, 39-40). Yet Elliot Sober (1980, 363) thinks that something called the "Natural State Model" was fundamental to Aristotle's metaphysical view; that this was a model for explaining variation among kind members; and that it allowed kind members to lack some of the properties definitive of its kind (e.g. because of environmental interference, etc) while still being kind members. So perhaps Aristotle did not think the necessity criterion need apply to every essential property at a given time.

Moreover, the Natural State Model implies that Aristotle's necessity was of the natural, not metaphysical variety. For example, by a contingent turn of events in this world, a man could lose his capacity for reason and yet still be a man. The more likely source, then, for thinking that the necessity stipulation need be of the metaphysical variety and apply to every single essential property at a given time, is Locke's *Essay*. At least, it is through the *Essay*'s influence that the metaphysical necessity criterion became entrenched. Locke argued that a property's being necessary to a kind of thing is what allows that property to represent a *real essence* of a thing rather than just a *nominal essence* (*Essay*, 4th edition, III.iii.13-17). If the properties by which we individuate things into kinds are not properties that kind members have by metaphysical necessity, then inevitably we run the risk of picking out kinds that are the "workmanship of the understanding."

Brian Ellis (2002, 16) and the so-called "new essentialists" also invest in the distinction between real and nominal essences and think that essential properties must be metaphysically necessary in order to preserve the naturalness of kinds. Because of this, and the idea that it is the naturalness (i.e. objectivity) of natural kinds that ensures they support successful inductions and law statements, many new essentialists (as well as individualists) presume it is problematic to account for successful inductions and law statements if a thing does not have its essential properties by metaphysical necessity (Chakravartty forthcoming). In other words, it is mysterious how a natural kind can be a natural kind and feature in natural laws without the metaphysical necessity criterion of essentialism. Similar reasoning motivates the individualist when she notes there are no properties all members of a species necessarily share and then concludes that species are not natural kinds.

The distinction between real and nominal essences is no doubt important, but we do not require the metaphysical necessity criterion in order to make sense of it. Given the discussion of natural laws above, I take it that an essence is real and explanatory when it accounts for the regular natural laws in which a natural kind features and this does not demand that it be a metaphysically necessary property. As mentioned above, we find such explanatory power in the historical, contingent properties of species.

Take the tiger, *Panthera tigris*, as an example. In our world, the tiger is a species that necessarily descends from the ancestor it does, and say the ancestor is *P*. The species' extension is necessarily the lineage following from *P*, and includes all the tigers within that lineage. As such, all and only tigers belong to that lineage (LaPorte 2004, 61). But the lineage contingently evolved as it did; selection pressures might have ensured things played out otherwise. It seems, then, not by metaphysical necessity that the lineage came to have the essence it did, i.e. being the lineage directly descended from *P*. Rather, it was by natural necessity: the lineage happened to evolve as *P*'s direct descendent in this world and so the lineage is now the direct descendent of *P* by necessity.

So each individual organism that is a member of *Panthera tigris* will—not in all possible worlds (metaphysical necessity) but in all worlds where it is a member of *Panthera tigris* (natural necessity)—also be a member of the lineage directly descended from *P*. In this way, *Panthera tigris* can be a *natural* kind, with members belonging to it of natural necessity, not metaphysical necessity. Kinds can be natural and not nominal without the metaphysical necessity criterion.

Notice also that the foregoing implies that if *Panthera tigris* and tigers have their origins *without exception* in at least this natural world, then there also seems ground for thinking it is at least possible that such naturally necessary historical essences can underwrite robust natural laws of the sort chemical kinds enjoy. This would be another way of deflating the individualists' first argument from natural laws.

But we are not committed to there being exceptionless laws for species. The point is that despite the traditional theory of natural kinds, there seems to me simply no good reason why the necessary nature of an essence must correspond to metaphysical necessity. Properties that conspecifics like tigers share by natural necessity are nonetheless had *necessarily* within the world with which we are concerned, and they are still explanatory. If such necessary properties are explanatory not in an exceptionless sense, but in non-trivial counterfactual senses, we have the ground we need to count them as real rather than nominal essences.

Having loosened the necessity criterion in our first pledged sense, we have already shown one way in which the argument from inherent heterogeneity may miss its mark. We can now proceed to the second loosening manoeuvre, which is to argue that kind members need *not* have *each* single essential property by even natural necessity.

To make this argument I want to proceed in two steps. First, I will claim that kind members need not have a property in any of the four senses of necessity in order for that property to be explanatory. Explanatory power, not necessity, is required for a property to be "natural." Second, I will enlist the notion of property *clusters* in order to show how the clustering of natural, explanatory properties *does* form the ground for "kinds." Explanatory property clusters individuate natural kinds, even when there are some properties in the cluster that not all kind members have.

Although this is not the place to delve into a long discussion of explanation, it is *prima facie* straightforward to see that properties can be real or natural, and explanatory, without being properties that all members of a kind have necessarily. Ellis and the new essentialists do not think biological species are natural kinds (since species do not meet the metaphysical necessity criterion I claim we should relax), yet they of course recognize that natural properties underwrite regular natural laws about species. As Ellis puts it, organisms of a species are "sufficiently similar...for explanations of the sorts characteristic of the physical sciences to be discoverable" (155); and this is so even though the properties realizing such organismic similarity are not properties that organisms have in any necessary sense. The properties that I wish to call essences in a revised sense, then, are indeed natural and explanatory, they are just not necessary.

Moving to the second and more protracted step of the argument for every kind member not needing to have every essential property necessarily, we can ask the following question. If properties can quite clearly be natural and explanatory without being necessary, what prohibits such non-necessary properties from being definitive of natural kinds?

To this question individualists, or even new essentialists, would offer something like the following answer. Properties can certainly form the basis for natural explanations, but any random property might be an explanation of *something*, and so mere real, explanatory power does nothing to ensure that properties underwrite explanations involving *natural kinds* more specifically. To be a property definitive *of a kind*, there must be some link between that property and kind members—a specific link which members of *distinct* kinds do not enjoy. The only way to secure this link is for kind members to have their properties by necessity.

Or so the individualist presumes. The presumption is overly conservative. In order for real, explanatory properties to be more than just "randomly explanatory" and instead be definitive *of natural kinds*, each kind member need not have those properties necessarily. Rather they need only *tend* to have those properties. This suggestion exploits the idea that natural kinds need only feature in regular natural laws, not just exceptionless ones. When a certain generalization about organisms of a given species fails to hold for an organism, it may be because the organism lacks a certain real, explanatory property that its fellow species members tend to have. Yet so long as the tendency for conspecifics to have that property is robust enough to support reliable inductions and regular rather than exceptionless natural laws involving the species, it can be the *sort* of essential property that individuates the *sort* of natural kinds with which a science interested in *ceteris paribus* laws is concerned.

Of course, this relaxes the necessity criterion with respect to single essential properties. But surely, for essentialism to have any teeth at all there must be something that all members of a kind share. That is, there must be something the absence of which explains why a certain individual is not of a given kind. And there is. Members of a kind, and certainly organisms of a species, share a *cluster* of essential properties that members of other kinds do not.

To say that conspecifics share a cluster of essential properties is to say that *the* cluster, not *each* property within it, is stable across conspecifics. New essentialists grant that such clusters obtain within species (154-155), as do a majority of individualists, since they hold the commonly accepted view that conspecifics tend to have a cluster of properties that realizes, for example, "sufficiently similar pattern of behaviour" among organisms (155). So although conspecifics may not necessarily share any one real, explanatory property, they do share *a* cluster of them, where the cluster tends to consist of certain properties. Building on the notion of real, explanatory property clusters, numerous authors now argue that in lieu of individual properties being had necessarily (natural necessity, metaphysical necessity or otherwise), such clusters provide the all the necessity we need to say that the biological forms to which they correspond are natural kinds (Chakravartty forthcoming; Wilson 2005, ch.3; Keller et al. 2003; Root 2000; Boyd 1999; Wilson 1999; Griffiths 1999; Kornblith 1993).

It is worth underscoring that science is the central motivator of the cluster approach to natural kinds. Hilary Kornblith (1993) points out, for example, that the explanatory properties forming stable clusters are responsible for scientific explanations being "astoundingly successful...In light of these successes, we can hardly go on to doubt the existence of the very kinds which serve to explain how such successes were even possible." (42) In short, then, what I am claiming is that individualists (as well as new essentialists) employ a view of essentialism that discriminates against the world's causal tendencies and against real, explanatory property clusters.

For evidence that stable, necessary clusters explain our generalizations or regular natural laws and afford successful inductive projections in biology, consider "goal directed biological activities, such as medicine, ecosystem management, and population control" wherein "inductive projections are crucial" (Chakravartty forthcoming). In these activities, the categories we investigate and which ground such crucial inductions often lack single defining properties that members necessarily share. Rather, the members of such categories have property clusters that tend to feature the same properties.

For example, biologists do not always categorize proteins in virtue of necessary properties that all members share; kinds of proteins are instead cluster kinds "on which the inductive projections of immunology depend." (Chakravartty forthcoming) Likewise, species support the inductive demands of, for example, ecologists, and they do so in virtue of property clusters, such that each organism has the cluster and tends to have each property in the cluster, but does not always (Dupre 1993, 43). A particular larval gall fly, for instance, may be altogether incapable of producing the saliva that induces gall formation in goldenrod plants, but the deficient fly may survive in a large enough plant nonetheless, so that while counting the local gall fly population, an ecologist will still count (if she detects it!) the atypical gall fly. She counts it because gall flies so deficient have other properties in the cluster definitive of gall flies, and because, as a result of having these other properties, certain inductions about the population will turn out false if she does not count deficient gall flies.

Given the inductive successes of biologists in myriad fields of study and the clusters of explanatory properties that underwrite them, we should allow that these properties individuate clusters, and so kinds, that we call natural. If we do, we have loosened the necessity criterion sufficiently to resist the individualists' argument from inherent heterogeneity. Species members need not have each essential property by metaphysical necessity.

At this point the individualist is likely to have at least two questions, and though they may be distinct, we can, I think, offer a single solution that answers each.

The first question concerns a problem of *induction*. Those new to the cluster view will want some account of *why* inductive practices of biologists are successful, given that biologists cannot appeal to the necessity of single properties when grounding the lawfulness of statements. This cluster talk, for example, sounds much like Wittgensteinian family resemblance, which, notoriously, we can usefully apply to artificial kinds, but not to natural ones. So even if the cluster is a necessary property of kind members, as I have suggested, *why* is it that particular properties tend to cluster together in a way that makes the notion of a shared cluster intelligible? In short, what makes clusters natural, necessary features of cluster kinds, rather than being of an artificial Wittgensteinian sort? The solution I will present to this problem lies in the idea that *links* we find *in nature*, rather than in the language games we play, connect properties in a cluster. This *natural links account* of cluster stability ensures that clusters have natural stability that underwrites successful inductions.

The natural links account will also help answer the individualists' second question, which is a problem of *individuation and identity*. The putative problem stems from recognizing that if clusters just are the sum of the "essential but not strictly necessary" properties constituting them, then it will be difficult to individuate the clusters. What basis is there for drawing boundaries around two clusters and saying they define the same kind, if the contents (i.e. properties) of each cluster can vary? This question is also one about identity, for how we answer it will determine how we identify clusters.

For example, the individuation criteria we employ will determine whether two organisms have "the same" cluster and are thus of the same species, when the cluster of one of the organisms comprises properties *a*, b, c, d, e, f, and the cluster of the other comprises properties b, c, d, e, f, *g*. The natural links account will help solve this problem of individuation and identity because it shows that clusters are not just the sum of the properties constituting them. They are those properties *plus* the natural links between them. Properties in a cluster tend so often to go together because such tendencies have a natural ground—in the links between them.

The natural links account, we shall see, affords an *a posteriori* solution to the problem of individuation and identity. In this way, then, we can treat both this problem and the problem of induction as one problem: the *Cluster Stability Problem*. The solution to it is the natural links account of cluster stability. I shall now turn *not* to fully develop a natural links account, but rather to introduce how others have developed such an account, which individualists have not engaged. Even just this introduction will let us see how we can answer more concrete examples of the Cluster Stability Problem.

Anjan Chakravartty (forthcoming) covers important metaphysical preliminaries in appreciating any natural links account, by momentarily abstracting away from the notion of a cluster and explaining how properties on their own can underwrite successful inductions about kinds without being necessary properties. He notes, "[c]ausal laws relate the *properties* of things regardless of whether they belong to essence kinds or cluster kinds." Here, Chakravartty uses "essence kind" to refer to kinds with properties that members have necessarily, whereas we need not define a "cluster kind" in terms of necessary properties. Though unlike Chakravartty I am willing to retain the word "essence" even when kind members do not have their properties necessarily, my point and his are the same: properties need not be held necessarily in order for causal laws to relate them. Chakravartty puts this another way as well: "[l]aw-like behaviours obtain not merely as a consequence of the possession of [necessary] properties...but as the consequence of the possession of *any* causal property by *any* sort of object." A thing enters causal relations in virtue of its properties being causal, not always in virtue of its properties being necessary, and this is one reason why law statements often refer to the causal properties of objects rather than to the object's kind.

With a basic appreciation of the metaphysical explanation for the ability of non-necessary properties to ground inductions, we can return to thinking about such properties within the context of clusters, wherein necessity does enter the picture again, via cluster stability, and ensures there is a reason for saying non-necessary explanatory properties form the basis for a *kind.*

The most promising natural links account of cluster stability is referred to as the *homeostatic property cluster* (HPC) view of natural kinds. It was over 15 years ago that Richard Boyd (e.g. 1988; 1991) first advanced the idea that underlying homeostatic causal mechanisms routinely *cause* certain properties to cluster together and since then a number of authors have explained how such clusters can pick out real kinds in the world rather than nominal ones, even though no single or even n-tuple of properties in the cluster is necessarily had by kind members (e.g. R. A. Wilson 2005, ch.3, 5; Griffiths 1999; 1997; Kornblith 1999). Mechanisms that cause certain properties to cluster together are real features of the world, and they are homeostatic in that they *systematically* and *routinely* cause such property co-instantiation. In short, "the instantiation of certain properties increases the chance that other particular properties will be coinstantiated because of underlying causal mechanisms and processes." (R. A. Wilson 2005, 56) Authors have discussed such causal mechanisms with respect to kinds of chemicals and kinds of diseases (Kornblith 1993, ch.3), kinds of emotions (Griffiths 1997), as well as kinds of cells, and the kinds "life" and "gene" respectively (R. A. Wilson 2005, 103-110, 58-59, 125-126). Most notably for the individualist, authors have explicitly argued that the HPC view applies to species (R. A. Wilson 2005, 110-111, Boyd 1999; Griffiths 1999, 215-219).

R. A. Wilson (2005, 111) gives a window into what homeostatic mechanisms within a species taxon might be:

> clustering is caused by only partially understood mechanisms that regulate biological processes, such as inheritance, speciation, and morphological development, together with the complex relations between them...[such that]...organisms in a given species share morphology in part because they share genetic structures, and they share these because of their common genealogy. This is not to suggest, however, that any one of these properties is more basic than all of the others...for the dependency relations between these properties are complex and almost certainly many.

On the HPC view, then, the cluster of properties is the essence of the kind in question, rather than any single property in the cluster. Kind members need have no single property necessarily but instead a *sufficient subset* of the cluster of properties. What counts as sufficient will depend on empirical investigation and will certainly admit of borderline cases and vague boundaries; but such lack of crispness derives from a biological world that Darwin showed is inherently heterogeneous and changing.

To get a clearer sense of what a homeostatic property cluster might look like, consider how it might apply to the kind "life" (as discussed in R. A. Wilson 2005, 55-59). Life is notoriously difficult to define and there is almost certainly no single essential property a thing can have that will ensure it is alive. Introductory biological texts typically list 7-10 "properties" that define life, noting that not all living things have all of these properties. R. A. Wilson thinks a promising list of properties that living things tend to have and which are definitive of them as living things would include (paraphrased from p.57):

- having heterogeneous parts
- having a variety of internal mechanisms
- containing nucleic acids and proteins and other diverse organic molecules
- growing and developing
- reproducing
- having the ability for self-maintenance and repair
- metabolizing
- bearing adaptations
- tending to construct niches they occupy

A quick glance at this list might lead one to think it is disjunctive, or a list of Wittgensteinian family of concepts. But scrutiny is likely to show that "[t]his set of properties forms a homeostatic cluster in that there are mechanisms and constraints that promote the coinstantiation of many of them." (57) The properties reinforce each other. Many of the *functional* properties in the list (e.g. will reproduce, grow, develop, etc.) are possible in virtue of the *structural* properties (e.g. having the proper organic molecules, internal mechanisms and heterogeneous but organized parts). And yet, the structural properties also depend in part on functional properties (e.g. the organization of heterogeneous parts is unlikely to come about in the absence of growth). The natural links causing the coinstantiation of these properties are homeostatic mechanisms.

We are now in position to comment further on the Cluster Stability Problem. Insofar as individualists are curious about the natural ground of induction when single properties are not had by necessity, we see that homeostatic mechanisms that ensure *certain* properties tend to go together will also form the basis for saying the cluster those properties form in individuals are stable, natural, and allow for such induction in no more a mysterious way than single necessary properties do. An organism's status as a living thing ensures it can feature in the conclusions of various inductive arguments and this is because as a living thing it shares a property cluster with other living things.

Now, how to individuate and identify such a cluster. I already said that individuals have the requisite property cluster if they have a *sufficient subset* of properties that tend to constitute the cluster, and that what counts as sufficient is an empirical matter. This implies there is no strictly *a priori* answer to, say, which forms we count as living and which we do not count as living. In some cases, it will be relatively clear that a certain form lacks one or two of the properties in the HPC list for life, but is nonetheless living, such as when microorganisms demonstrate limited growth and development, yet upon empirical investigation clearly display a sufficient number of the properties in the list that we say they are alive. Likewise, some plants radically restrict their metabolism in harsh conditions but are clearly alive (57). So in comparing microorganisms with plants, we have a case just like the one I mentioned above, where we said one individual may have properties *a*, b, c, d, e, f, and another may have b, c, d, e, f, *g*. It is not arbitrary to say a plant and a microorganism are both living things, despite the plant having *a* and not *g*, and the microorganism having *g* but not *a*, because properties the individuals *do* share with each other go together naturally rather than arbitrarily. It is because of the homeostatic mechanisms causing the coinstantiation of properties, and not just because they share *b* through *f*, that we are justified in saying the individuals are of a kind. Demanding that they share *a* through *g* is to impose a mistaken ideal on an inherently heterogeneous but nonetheless orderly world (58).

Granted, in other cases matters will not be so clear. Some individuals will share fewer properties from the list with other individuals. Viruses, for example, lack a number of properties in the above list. But viruses *just are* borderline cases, and the HPC view readily explains there status as such (57). Vagueness exists in nature, not just in our definitions. And yet vagueness is not so pervasive as to overwhelm the HPC view; my hunch is that so many living forms *do* display a majority of properties in the above list just because the causal homeostatic mechanisms linking their properties are so robust. Moreover, because I think such mechanisms are far more numerous in the integrated living world than many authors have considered, property clusters are likely to include more properties than less, and this dilutes the effects of periodically absent properties.

Hence, we see how a natural links account of cluster stability has the resources to solve the Cluster Stability Problem. This, I think, secures the second manoeuvre I said was available for deflating the individualists' argument from inherent heterogeneity. The first part of that manoeuvre was to show that single properties can be explanatory and yet not necessary for kind membership, and the second part was showing how the notion of property clusters nonetheless retains all the necessity we require. Thus, property clusters form the bases for natural kinds. When we couple this two-part manoeuvre with our first manoeuvre, wherein we showed that only natural necessity and not metaphysical necessity is required for kindhood, we have powerful reasons for not committing to the high standards of necessity that individualists envisage. Inherent heterogeneity may ensure there are no phenotypic or genotypic traits that are unique to the members of a given species, but this does not defeat the species as kinds view. Indeed the HPC view seems especially "apt for characterizing the inherently heterogeneous kinds of thing whose individual variability may be critical to their being the kinds of thing that they are." (58)

As a final note in this section, let it be clear that the HPC view is just one version of the natural links account of cluster stability. Following his discussion of metaphysical preliminaries, Chakravartty (forthcoming) offers a different account in terms of the "sociability" of properties. But the HPC view is the account that authors have explicitly developed in the context of species ontology. Griffiths (1999, 219), for example, utilizes it when explaining that species can have historical essences that cluster together with other essential properties and he summarizes the view for the individualist nicely, saying "the causal homeostatic mechanism that guarantees the projectability of a kind plays the traditional role of an essence" (Griffiths 1999, 219).

It has been a number of years since Griffiths and others (see references above) first suggested this view's application to species taxa and thus far no one has attempted to flesh out the empirical details of this application, nor attempted to show why we could not. I have in mind here applying the view to a variety of species taxa, where the specifics of homeostatic mechanisms are either evidenced or found to be wanting; and further developing an explanation (of the sort I have developed here) of cases in which two members of a species share *no* single essential properties, yet each still has a sufficient number of the properties in a large cluster to be conspecifics. Unfortunately, individualists have not engaged the HPC view enough to raise even these worries. Michael Ghiselin (2002), David Hull (1999) and Marc Ereshefsky (2001) each mention the view in only a few lines. As I write this I have just learned that Ereshefsky and Matthen (2005) have very recently published a paper that pledges to engage the HPC view more formally. It will be interesting to see the results of their investigation.

5. ARGUMENT FROM RELATIONAL PROPERTIES

The fourth and final argument against the species as kinds view that we shall consider is the *argument from relational properties*. The argument is closely related to the argument from inherent heterogeneity that we have just discussed, as it effectively turns on the fact that evolutionary theory does not define species membership in terms of phenotypic or intrinsic properties that all species members share. We have seen that there are no such properties that all and only conspecifics share. Rather than only appeal to the criterion of necessity to raise a problem here, however, the individualist also turns to the intrinsic property criterion of the traditional kinds view, which says essential properties must be intrinsic to kind members. Individualists note that because of the lack of shared intrinsic traits, biologists define species not in terms of intrinsic properties, but rather in terms of *relational* ones, and this conflicts with the traditional kinds view (e.g. Sober 1980, 372).

To elaborate upon this argument, recall from chapter one the species definition problem that I said was distinct though related to the species ontology problem. Competing species concepts attempt to define the species category such that species are set off from each other, and form genera, and from subspecies, and so on. We shall consider this problem in more detail when appropriate in chapter four, but here we need only appreciate that all the leading species concepts offer relational definitions of the species category. Two of the most common sorts of definition are reproductive views, on the one hand, and genealogical views, on the other.

Reproductive views claim that what sets conspecifics off from others is the fact that conspecifics stand in certain reproductive relations to each other and not to members of other species. For example, conspecifics are members of a certain reproductively isolated population, or are able to interbreed with other conspecifics (Mayr 1999); or they share a mate recognition system with conspecifics that facilitates interbreeding (Paterson 1985).

Genealogical views, meanwhile, claim that an organism is conspecific with another because of the ancestry it shares with the other. The two organisms are conspecific because they are both members of a lineage with a particular origin, and we determine this relation through certain shared, phlyogentically significant traits (e.g. Meier and Willmann 1999). So on any of these mainstream accounts of the species category, species membership is relationally defined. As a result, "constituent definitions," such as those the traditional kinds view employs, have become "unnecessary" (Sober 1980, 372).

There are, I think, at least two reasons why the intrinsic property criterion of essences has seemed to some to be an important criterion for a theory of natural kinds to retain. The first reason has to do with invariability and the second with causal powers. Consider each in turn.

As mentioned while expositing the traditional kinds view, the idea that essential properties must be intrinsic to kind members is one that is closely related to the stipulation that they be metaphysically necessary. If a kind member has the essential properties it does by metaphysical necessity, then without such properties it fails to be or remain a kind member. For kind members to retain the stability they seem to retain, that is, to remain kind members as they do, their metaphysically necessary properties would seem to need to display a certain invariability. Authors have presumed that intrinsic properties are the best, if not only candidates for being invariable as such. And although Aristotle allowed that grosser features of organisms could constitute an organism's essence, many "essentialists" have thought that to have necessary invariability, essences must be microstructural (e.g. Ellis 2002). Hence, "hydrogen" is a good candidate for a natural kind because every hydrogen atom has the essential microstructural property of having one proton (13). This favouritism of the microstructural aligns with more general physicalist views in metaphysics that posit that all facts, properties, states and so on are determined by physical facts. *Micro*physical determinism more specifically is a prevalent view that says all facts, properties, states and so on are determined by microphysical facts that tend to have the sort of invariability needed to account for the world's causal regularities (R. A. Wilson 2004, 122).

Favouritism of the microstructural is apparent in causal theories of reference that authors have defended, especially over the past 30 years. These authors frequently imply that only microstructural properties can individuate natural kinds. Perhaps it is no surprise then that the individualists' arguments against the kinds view are likewise 30 years old, since while authors like Putnam were appealing to the microstructural essence of biological species, individualists knew (or at least had grounds for thinking) that, pace Putnam, the inherent heterogeneity of species ensured such essences do not exist in species. But it may be that while Putnam and company were getting their biology wrong, an overly strict microstructural essentialism blinded individualists. Michael Ruse (1987, 227, f.n.1) bemoans this ill-fated interdisciplinary exchange, saying "[i]n my view, most of the modern supporters of natural kinds end up somewhere to the right of Aristotle (e.g. Kripke [1972]; Putnam [1975]; Wiggins [1980]). Frankly, I am not sure how far these modern thinkers really intend their ideas to apply to biology, since they generally do not bother to refer to the works of practicing taxonomists, and at times show an almost proud ignorance of the organic world."

If the appeal of microstructural invariability is one motivation for the intrinsic property criterion, the other motivation may even underlie the first and stems from the idea that essential properties cause individuals to be the kinds of things they are, such that they confer causal powers upon the individual. To individuate by essences is thus to individuate by causal powers. For many "essentialists" it is crucial that essential, explanatory properties realize causal powers of kind members because of their view of the world's causal structure. Unlike Humeans, they think individuals (e.g. organisms) are *active* players in causal processes. Individuals and the essential properties of theirs that determine their kind are the things that realize lawful regularities, rather than individuals being things that *passively* follow laws of nature impinging upon them from without (e.g. Ellis 2002, 35-38). The further presumption that lets the intrinsic property criterion get its toehold is that *only* intrinsic properties of individuals can confer causal powers on individuals as such. Presumably, only intrinsic properties can feature in causal regularities (R. A. Wilson 2004, 97).

Given my tack against the previous three negative arguments, it is no surprise that I think we can resist the argument from relational properties without developing a full metaphysical account of relational properties that rescues them from subordination to intrinsic ones. Rather, we can point to authors who have already undertaken such tasks, explain why their bases for doing so are plausible, and show how scientists in a range of disciplines appeal to relational properties when individuating kinds. Let us begin resisting the individualists' last negative argument.

Both the "invariability motivation" and "causal powers motivation" for the intrinsic property criterion trade on a putative metaphysical asymmetry between relational and intrinsic properties that I think is an especially persistent hangover from corpuscularian dogma. Take the invariability motivation. It seems that many of an individual's sub-atomic properties are fixed, pace any environmental change, and therefore, such properties are good candidates for inherently invariable properties that an individual of a kind has necessarily (e.g. see Locke's *Essay*, 4th ed., III.iii.16). A ready way of pointing out that such thinking is an illegitimate straight jacket on the biological sciences is to refer to our previous discussion of necessity where we saw that on genealogical accounts of species membership, an organism's relational properties *could* essentially and invariably define its kind. This casts doubt on the metaphysical asymmetry between intrinsic and relational properties.

To see this, imagine that we exploit the notion of natural necessity, as many taxonomists seem to (Griffiths 1999, 219), and proceed to say that certain historical and thus relational properties a tiger has are invariable, and determine the kind of thing it is. For a tiger, being a member of a lineage that necessarily descends from *P* is an *invariable* property it has. And on many genealogical views of species, it is an *essential* property the tiger has. The tiger would not be a tiger were it not a member of the lineage descending from *P*.

But we could be more adventuresome and refer further to our discussion of cluster kinds and the non-necessity of single properties to show that invariable properties are not the only properties that can individuate a kind. Invariability does not form a good basis for privileging a property as an essence candidate.

For example, though on certain accounts of species membership conspecifics have certain historical properties necessarily, on reproductive accounts of species membership there may seem no option for necessary, single and invariable essential properties. If being a conspecific is to be a member of a certain interbreeding population, the property of standing in such a reproductive relation is variable. An organism might lose its capacity to interbreed; and yet biologists supporting an interbreeding account would not say the deficient organism now belongs to no species. (Note: though I shall later turn a critical gaze on interbreeding views, using such views to make my point here will not vitiate my later criticisms) One way to make sense of this is to take such relational reproductive properties to be one of many in a cluster of properties that conspecifics share, rather than a single necessary and invariable property. The cluster defining the species may include interbreeding relations, historical relations, niche sharing relations, as well as *tending* to have intrinsic properties such as having certain morphological features or satellite DNA. On this view, a spayed tiger at the zoo is still a tiger because it has a sufficient subset of the properties in the cluster that defines *Panthera tigris*. And of course, there are natural links between properties in the cluster, ensuring it can define a natural kind. For example, a tiger tends to have the capacity to interbreed with other tigers *because* it is historically connected with other tigers, shares a niche with them, and shares morphological features with them; none of these properties is more basic than another, and the tiger does not lose them all when it loses the ability to interbreed. This deflates the need for invariability and opens the door to those relational properties that may change over time without changing the ontological nature of the individual who bears them.

In short, then, we need not seek out microstructural properties or even intrinsic ones more generally when seeking invariability, and we may not need to seek invariable properties at all.

The causal powers motivation seems likewise illegitimate, or at least misunderstood. I agree with Ellis and the new essentialists that individuals, objects, and so on are not mere passive players in the causal nexus, but neither are they *so* active as to have their properties and dispositions be determined entirely intrinsically, independent of the contexts they are in and relations in which they stand. This may entail that we abandon the idea that causal powers individuate natural kinds, but it may not. It may only require that we tweak our notion of causal powers, granting that the relevant causal powers need only be powers *of* individuals, not *in* individuals. An individual can have "relational causal powers," if you will. In any case, the relational properties of individuals have important roles to play in determining the dispositions of individuals, how they move through the world, and the kinds to which they belong.

To see this in the case of dispositions, consider the crucial biological disposition of *fertility* within sexually reproducing organisms. Whether or not a certain plant is fertile certainly depends in part on its intrinsic constitution. But it also depends crucially on a host of features external to it, such as whether there are nearby plants with which to interbreed. If the plant is insect pollinated its fertility will depend on whether the relevant insect population is present and healthy; and if it is wind pollinated, it will depend on whether the trade winds are blowing this year. In such cases it is *not* just that a plant's disposition to be fertile is only *manifest* when it stands in certain relations to other plants, insects and winds. Rather, the plant only *has* the disposition when such relations obtain. The disposition itself is a relational property; the ontological base for it extends beyond the plant's boundaries. This does not imply that the most critical part of that base is extrinsic to the plant. Indeed, there may be good reason for considering the reproductive system of the plant to be, as R. A. Wilson (2004, 107, from Shoemaker) puts it, the *core realizer* of the plant's fertility. However, other plants, insects and/or wind seem critical to fertility as well, in a sense far more robust than the necessity of, say, gravity or oxygen as background conditions of actual interbreeding events. The other plants, insects and wind are also important "realizers," not mere background conditions, and together they form an integrated system with our fertile plant—a system that stretches beyond the boundaries of our fertile plant—such that the state of that system metaphysically determines the plant's disposition, thus ensuring its disposition to be fertile is a relational disposition (R. A. Wilson 2004, 125-127).

In the next chapter I will explain the notions of realization, systems, core realizers and extrinsic realizers in more depth as we exploit the language of realization to make sense of how cohesion is metaphysically determined, and this should add further support to my argument of this section. But our purposes here do not depend upon the details of the language of realization and a simple counterfactual scenario can help support the idea that the fertile plant's disposition is relational.

Let us say that a fan of the asymmetry between intrinsic and relational properties claims that dispositions of a plant *supervene* on the intrinsic properties of the plant. This means that the intrinsic properties of the plant metaphysically determine its dispositions such that if another plant had the exact same intrinsic physical constitution, it would have the same dispositions as the "original" plant in every possible world. That is, both physically identical plants will have the same dispositions regardless of the difference between the environments of each, and so each plant will have the same dispositions irrespective of the presence of other plants, insects and/or wind, and so on. In contrast to the fan of asymmetry, my claim is that being fertile is a disposition the original plant has that does *not* supervene on its intrinsic properties. If we hold just its intrinsic properties fixed and transport it from an environment where it was fertile to one without the relevant plants, insects and/or wind, it would no longer be fertile, even if all other background conditions were constant in both environments. The most crucial determining property of fertility may reside in the plant, but not all metaphysical properties that determine that the plant *is* fertile are intrinsic to it (for roughly the same argument involving the disposition of acidity, see R. A. Wilson 2004, 125-126).

Fertility is not an extraordinary *kind* of disposition. We can construct similar arguments for a range of dispositions important to scientists, such as weight, vulnerability, visibility, recognizability, acidity, miscibility, solubility, rigidity, heat sensitivity, conductivity, stability and trustworthiness (126). Relational properties individuate all of these kinds of dispositions. Across the sciences, relational properties similarly individuate natural kinds. In biology, take kinds of forests as an example.

Forests undergo processes of succession such that what kind of forest a certain forest is depends on the kind of forest from which it succeeded. An ecologist might draw conclusions about how to manage a certain forest based upon a forest's kind and so based upon how that forest stands in relation to previous forests, rather than based just upon the intrinsic properties of the forest.

Another biological example is kinds of *neural crest cells*. As R. A. Wilson (2005, 104-107) documents while drawing from neuroscience research, this category of cells admits of kinds such as *adrenergic* and *cholinergic* cells, which are individuated in part by relational properties. These "essential" relational properties include the cells' place of origin in early neurodevelopment, the neural pathways they take through development and the relations in which they stand to other sorts of cells. Moreover, the dispositions of the cells are relational as well, so if we were to individuate them by their "causal powers" as some might like, we would individuate, again, by relational properties. For example, experimenters have observed that when neural cells from a quail are transplanted into a chick embryo, instead of producing the neurotransmitters they would have in the quail from which they came, they produce those of a different kind that are typical of chicks. Thus, the environment in which neural cells reside can determine the dispositions or causal powers of those cells (106). It is in this sense that if we wish to retain the notion of "causal powers" we must admit that causal powers are powers *of* individuals (e.g. causal powers of the neural cells), and not always *in* individuals (e.g. causal powers in the neural cells). If we do not broaden the notion of causal powers as such, we must admit that we do not always individuate by causal powers (see 96-98).

A potential worry here is that if we admit that the grounds of metaphysical determination extend beyond the boundaries of an individual, then there seems the potential for the individual to stretch beyond the bounds of itself, and this at least seems odd. However, individuals on the view I endorse remain physically bounded, continuous and cohesive entities, where these features ensure they have the ontological autonomy requisite for being the entities that properties and dispositions *are of*. What it means for an individual's boundaries to stretch beyond themselves is an interesting if perplexing question, but in any case, it is not something we must worry about simply because an individual's, say, dispositions stretch beyond the individual. No matter the metaphysical import of facts beyond the plant's outer cells, its fertility is still a disposition *of it*, or that *it has*.

So far in this section I have attempted to undercut the variability and causal powers motivations for the intrinsic property criterion of essences, to show that species can be kinds in virtue of relational properties. To do this, I first showed that certain relational properties of species may satisfy a presumed need for essences to be invariable and then suggested that such invariability is a red herring in any case. This is based in part on my previous deflations of the exceptionless law and metaphysical necessity criteria. To argue past the causal powers motivation I have demonstrated how properties that are not intrinsic to the individuals bearing those properties can metaphysically determine or fix dispositional properties and kind membership. This forces one to either give up the idea that causal powers are always intrinsic, or instead give up the idea that (say) kinds are only individuated by intrinsic causal powers. It seems there is a metaphysical *symmetry* between intrinsic and relational properties that can underwrite the possibility of relational properties being essences. As such, we have resisted the individualists' argument from relational properties. Even if species members lack intrinsic essential properties, they need not lack essences, as my appeals to historical and reproductive species essences imply.

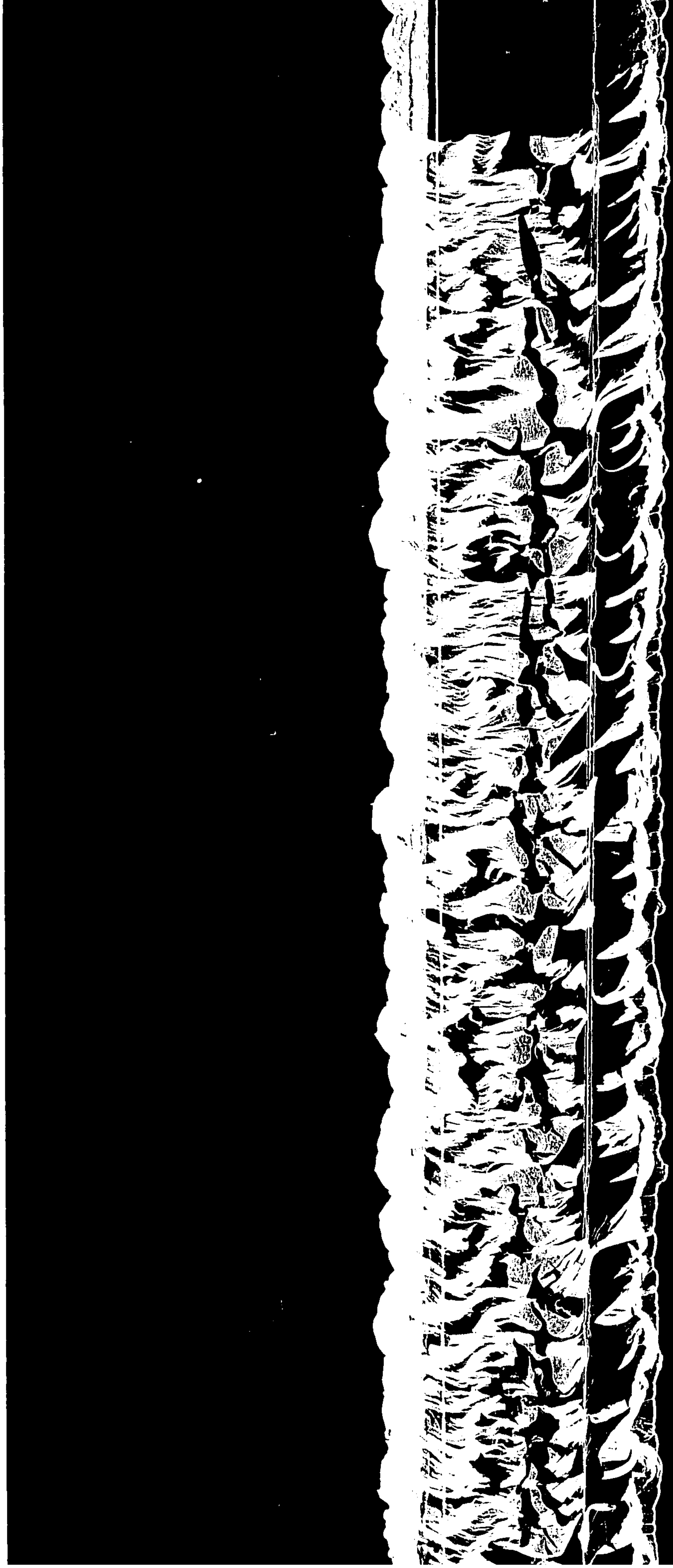
In reply to this conclusion, the individualist might like more metaphysical discussion than there is space for here. On the one hand, this is doubtful in the first place, since individualists have generally not explored the metaphysical presumption that kind essences must be intrinsic. Again, numerous authors (e.g. Griffiths 1999; Boyd 1999; R. A. Wilson 1999; Dupre 1993; LaPorte 2004) have suggested that species could be relationally defined kinds, but individualists have generally not engaged this suggestion. For example, Sober (1980), Ghiselin (1997), and Ereshefsky (2001) discuss essentialism at length, but implicitly or explicitly deny that relational properties can serve as essences. Indeed, although Ereshefsky is willing to entertain at least one revised conception of natural kinds, namely Richard Boyd's (1990), his rejection of its suitability for species taxa is based in part on the incorrect assumption that Boyd's account must still require essences to be intrinsic to kind members (Ereshefsky 2001, 108). Boyd's conception of kinds, however, does allow a place for relational properties (e.g. 1991, 142). Unfortunately, Ereshefsky (2001, 108) passes over this when he laments that, "no causal relations *among* the members of a kind are posited by [Boyd's account]."

On the other hand, if individualists *did* desire further metaphysical support for the significance I have here granted relational properties, there are numerous places to find it. The now large literature on *externalism* within the philosophy of mind is one place to start. More recently, R. A. Wilson (2004, ch.4-5) has summarized some of the relevant metaphysical issues in favour of relational properties. In doing so, he has shown that we cannot always reduce relational properties to the intrinsic properties that individuals have in virtue of standing in certain relations *to each other*. One reason for this is that an individual's relational properties are not always relational in virtue of *that* individual's relation to *another* individual. Instead, an individual often has a relational property in virtue of its relations to contexts and environments more generally, whereby it becomes difficult to make sense of the idea that the "environment" has intrinsic properties to which we can in part reduce the relational property in question (122-123).

6. MAKING THE TURN

Sections 2 through 5 have considered and resisted in turn the four arguments constituting the negative portion of the individuality thesis. Each argument takes issue with a strict notion of essentialism underlying a particular theory of natural kinds and we have seen there are good reasons for thinking that a friend of the species as kinds view need not commit herself to such essentialism, or a corresponding theory of natural kinds. Indeed, it seems that not many do commit to such essentialism, or at least few of those authors with whom individualists (should) argue make such commitments. Rather, authors have suggested that species are paradigm examples of the sort of natural kinds (e.g. HPC kinds) that are perhaps ubiquitous within many of the sciences. They are kinds that are individuated by naturally necessary properties, or instead clusters, where the operative properties may be relational and mutable, such that kinds have spatiotemporal dimensions and feature in mere regular natural laws. And yet individualists have not engaged these promising suggestions. It seems the individualist simply does not appreciate the revision potential of kindhood, as I sketched in section 1, and does not carefully consider the target of her argument. As such, not only has the burden shifted to the individualist here, but also the promising nature of revised kind theories implies that the burden is heavy. Granted, to resuscitate fully the species as kinds view, one would need to develop much further my discussions of laws, immutability, necessity and the nature of properties. Each of these tasks could easily fill a book in its own right. But our work seems complete enough to have now backed individualists into corner, where we might explore more deeply the positive portion of their thesis.

In making the turn to consider the species as individuals view, the next chapter argues for a certain conception of cohesion that posits at least two distinct kinds of cohesion, one of which is minimally necessary for individuality and the other of which is not necessarily indicative of individuality. While applying this analysis of cohesion to the notion of species cohesion in chapter four, we turn to consider which species concepts might imply that species cohesion corresponds to the cohesion of individuals. It appears that only gene flow-based species concepts may work for the individualist here, but chapter five takes aim at gene flow-based species concepts and accounts of cohesion, and thus takes aim at the species as individuals view more generally. The options for a revised species as kinds view bruited in this chapter shall thus come to seem more attractive still.



3

Two Kinds of Cohesion

...clearly the question is why *the matter is some individual thing.*

Aristotle, *Metaphysics*

1. THE CONCEALED CRITERION OF INDIVIDUALITY

In chapter one I explained that the following claim is a basic presumption underlying my overall argument: *individuals necessarily comprise causally unified parts.* We saw that numerous metaphysicians and individualists agree with this claim. In an introductory manner, I also said that cohesion is causal unity, where causal interactions among the components constituting an entity are responsible for the cohesion an entity displays. Given this, the claim that metaphysicians, individualists and I agree upon is one that says an entity must be cohesive in order to be an individual. Indeed, not only is cohesion a necessary condition of individuality, but as individualists such as Ernst Mayr (1987, 155) put it, cohesion is "by far the most important definens of an individual." However, we need to sharpen what it means to say that individuals necessarily comprise causally unified parts. A central question this chapter addresses, then, is: what is it for an individual to be cohesive? A key part of the answer to this question will involve stating what it is *not* for an individual to be cohesive.

Another individualist, Marc Ereshefsky (2001, 116), helps us begin to see what it is for an individual to be cohesive. Following his brief exploration of individuality, Ereshefsky concludes: "what seems to distinguish paradigmatic individuals and nonindividuals is that the parts of an individual must *appropriately* interact, whereas the members of a nonindividual *need not causally interact*." (my emphasis) The two key terms in this passage are "appropriately" and "causally *inter*act." The last of these makes the modest, uncontroversial, though important clarification that when individualists and I claim, "individuals necessarily comprise causally unified parts," we imply the following. An entity cannot be an individual unless there are causal interactions *between* its components. This is a *minimal* stricture on prospective individuals.

Of course, merely because causal interactions are between an entity's components does not make them *sufficient* for that entity to be an individual. (Note: when neutrality is called for, I shall refer to entities and their components, presuming these terms can correspond to groups and their members, individuals and their parts, etc.) Causal interactions between components are not sufficient for individuality because it seems other conditions such as boundedness and continuity may be necessary. For example, one reason why we might quickly acknowledge that a tight or cohesive group of friends—wherein the friends only causally interact by phone—do not form an individual is because they are not properly bounded or continuous. Yet this may not be the only sense in which interactions between components are insufficient. Such interactions may not even be sufficient for the *cohesion* of individuals, never mind *individuality* more generally. There seems, for example, something inadequate about the cohesion that a group of "phone friends" displays. Given that the cohesion of paradigm individuals owes to causal interaction between parts, the cohesion of the group of friends seems of an inadequate kind for the group to be an individual, because phone conversations do not instantiate an adequate kind of causal interaction between components. The kind of causal interactions determine the kind of cohesion. Some kinds of causal interactions between components are sufficient for the cohesion of individuals, and some are not.

In his passage above, Ereshefsky makes just this point through his use of the term "appropriately." Causal interactions between components of an entity will only suffice to ensure the entity demonstrates the *cohesion of an individual* if those interactions are of the *appropriate* kind. This is vague and I will attempt to say more in this chapter about which causal interactions between components are the appropriate ones, and which are not the appropriate ones, with respect to individuality (unfortunately, Ereshefsky is virtually silent here).

For now though, I want to appeal to the Ereshefsky's modest clarification to articulate a minimal criterion of the cohesion of individuals, and then verify that individualists and I can agree upon this criterion:

Minimal Integrative Cohesion Criterion of Individuality (MICCI): To be an individual an entity must minimally display integrative cohesion, which is cohesion owing to the causal interactions between the entity's components.

An individualist and I can both assent to this criterion because we both agree that individuals necessarily comprise causally unified parts and because it seems uncontroversial that this, as Ereshefsky has clarified, means that there are causal interactions between the parts of the individual. My term *integrative cohesion* merely gives a name to the cohesion that arises when the causal interactions that individualists and I agree are operative actually do obtain in an entity. After expanding upon the nature of integrative cohesion in this chapter we shall be able to sharpen the ideas contained in the MICCI and thus extend our formulation of the MICCI before applying it to species cohesion in the following chapters. With the MICCI laid out before as it is now though, it will pay to provide some textual evidence of the assent I have said it receives from individualists.

First, consider that while discussing individuality and his nuanced individualist position (where species are "cohesive populations") Ernst Mayr (1987, 159) elaborates on his belief that cohesion is individuality's key definens when he says, "what makes species 'individuals,' is the interaction of the members of the species. They exchange genes with each other in every generation..." He emphasizes this again when he says that "[i]n a genuine individual all parts interact with each other and do so directly." The notion of direct interaction is just what the MICCI captures via the idea that integrative cohesion arises when there are causal interactions between components of an entity.

While buttressing his version of the species as individuals view, Ghiselin (1981, 271) also appeals to such a conception of cohesive individuals that the MICCI enshrines when he notes that the parts of an individual are "integrated in one way or another—joined as by physical or social forces or common descent."

We have seen that Ereshefsky accepts the ideas of the MICCI, but he further clarifies the idea that the cohesion of individuals owes to interactions that are between components when he contrasts such interactions with those that are of an *independent* but *additive* sort. While discussing what sort of cohesion is required for individuality in his (1988, 430) paper, he rules out the sort of cohesion that is "merely the additive result of [components] working independently and not the result of any interactive process between [them]."

Though Ereshefsky never offers an analysis of cohesion and indeed explicitly mentions that he finds cohesion "too ambiguous" to analyze in the limited space of his book (2001, 114), we saw in chapter one while becoming familiar with cohesion complacency that other individualists typically discuss cohesion even less than Ereshefsky. Most individualists are happy to make the claim that organismic cohesion and species cohesion are the same in kind, then offer thin argument for this claim, and quickly move on. But even in saying as little as they do, these individualists implicitly imply that they have the ideas of the MICCI in mind. For example, one stock "thin argument" consists in noting that some organisms seem to display decidedly unimpressive cohesion when compared with more highly structured organisms, yet we still call them organisms or individuals, and thus the less impressive cohesion of species also suffices for individuality. However, the "borderline organisms" authors discuss—those that bridge the gap between highly evolved organismic cohesion and species cohesion—seem to clearly display cohesion that arises from causal interactions between parts, as the MICCI requires. For example, consider that Hull (1976, 177) says,

> with respect to the pseudoplasmodium stage in slime molds [i.e. where individual amoeba form a functionally integrated mass of discrete cells], is it a society or an organism? Such questions are extremely difficult to answer with respect to organisms. Comparable questions may be somewhat

more difficult to answer in the case of species. However, exactly the same questions arise for both. If organisms can count as individuals in the face of such difficulties, then so can species.

If we do consider the pseudoplasmodium to be a single organism and thus an individual in its own right, this is because each amoeba becomes a part, where between them *qua* parts there are causal interactions that are integrating them into a whole (J. Wilson 1999, 9). Even organisms that just barely (if at all) qualify as individual organisms still must demonstrate causal interactions between parts. It is by appealing to such cases that individualists who say little about cohesion nonetheless implicitly agree with the MICCI; other individualists who say little about cohesion but who are more explicit about such commitments here include Crane (2004, 165) and Brogaard (2004, 228).

The rare individualist helps to make explicit the ideas of other individualists who are somewhat vague about cohesion. For example, I quoted Kent Holsinger (1984, 296-298) at length in chapter one, but in our current context the crucial parts of that quote that are worth repeating are the last three sentences.

> Taxa are not merely collections of physical objects, they are collections that have a certain degree of internal structure and organization. The individual organisms that are part of a taxon interact with one another in a variety of ways. They cohere...

While claiming to represent individualists more generally (see quote in chapter one), these sentences indicate that the sorts of causal interactions that the cohesion of individuality requires are those that are between organisms, as the MICCI demands. For Holsinger and most individualists, organisms cohere and form a "species individual" when they "interact in a variety of ways."

Even authors who are sometimes critical of individualists admit that if the causal interactions between conspecifics are significant to their status *as conspecifics*, the species as individuals view has some appeal, or as Borjesson (1999, 9) puts it, "[e]ven those otherwise skeptical of the individuality thesis generally concede that if causal cohesion in the form of robust gene flow is present, a species is an individual."

The passages above come from the leading discussants in the debate over species ontology and they show, I think, that a variety of different sorts of individualists agree with something like the MICCI. The variety is representative, for example, of the consensus among authors with nuanced individualist positions (e.g. Mayr, Ereshefsky and Ghiselin), among individualists who implicitly or explicitly appeal to the significance of species cohesion (e.g. Hull, Holsinger, Crane and Brogaard), and among authors who are cautious about individualism (e.g. Borjesson). But staunch critics of the species as individuals view have also pointed out that, minimally, individuality consists in cohesion that arises from causal interactions between parts, though they do not develop or exploit this idea as I shall. Philip Kitcher (1989, 200), for example, has in mind something like the MICCI when he says,

> A proposal to count lineage-stages as stages of the same species should depend on the intrinsic properties of and direct relations among those stages. It should give the same results in cases which differ only in the existence or properties of organisms occupying a different branch of the lineage.

Chris Horvath (1997, 658) refers to this passage as Kitcher's "Intrinsic Properties Criterion of Individuality." Its appeal to "direct relations" among relevant parts aligns nicely with the MICCI and the necessity of integrative cohesion.

With most individualists, and parties to the species ontology debate more generally seeming amenable to the MICCI, one might wonder how the MICCI could possibly work against the individualist. There are two reasons it can work against them.

First, although a few individualists seem to have kept something like the MICCI fresh in mind when discussing species cohesion (e.g. Ereshefsky 2001; Mishler and Brandon 1987, 399-400), they have applied it incorrectly because of their mistaken view of interbreeding. This will come clear in subsequent chapters. Second, the vast majority of individualists *do not* keep something like the MICCI fresh in mind; they have little incentive to do so because they fail to distinguish between two kinds of cohesion as I shall do in this chapter. Cohesion complacency ensures that authors miss this distinction and as a result, the MICCI is a sort of latent or concealed criterion that individualists thus do not explicitly formulate and against which they do not properly test species cohesion.

The second kind of cohesion against which I contrast integrative cohesion is what I call *responsive cohesion*. Making this distinction between cohesions will ensure that the MICCI will suffice for our purposes even though it is a minimal criterion of individuality that does not embody a complete answer to the question of the cohesion of individuals. At the head of this chapter I provided Aristotle's terse articulation of this question, but an answer likewise eluded him during his quest for primary substance in *Metaphysics*. The distinction between cohesions will allow the useful application of the MICCI because we shall not only know what individuality *minimally* consists in (integrative cohesion), but also what it does *not* consist in (responsive cohesion).

To argue for the distinction between the two cohesions and then expand our formulation of the MICCI, I proceed through the remainder of the chapter as follows. In the next section I sketch the nature of the two cohesions and show how they can come apart in the world. Having a firm initial grasp of the two cohesions, I then analyze the two notions in terms of realization in sections 3, 4 and 5. Section 6 summarizes the analyses within definitions of each kind of cohesion and then in section 7 I show how the two distinct kinds of cohesion can sometimes be coinstantiated. The chapter closes in section 8, as we extend our formulation of the MICCI and package up the points that we will call upon most frequently in subsequent chapters.

Let us now turn to sketch the distinction between the two kinds of cohesion.

2. A SKETCH OF THE TWO COHESIONS

The most basic and important differences between the two cohesions is that integrative cohesion is an *intrinsic* property and does *not* consist in response to an external pressure, while responsive cohesion is a *relational* property and *does* consist in response to an external pressure.

An assortment of entities can have these properties, including paradigm individuals, as well as what seem to be groups. To get an initial grasp of each kind of cohesion, how we can find each kind in both individuals and in groups, and how the two kinds of properties are distinct kinds such that they can come apart in nature, consider the following intuitive examples, beginning with a paradigm individual, and then moving onto a group.

A brook trout (*Salvelinus fontinalis*) is a fresh water fish and a paradigm individual. It displays integrative cohesion in virtue of causal interactions between its parts. Its parts include various bodily systems, organs, cells, blood plasma, and so on.

Not just any kind of causal interactions obtain between these parts. The causal interactions are *integrative* in either a *structural* or *a functional* sense (or in both senses). That is, *integrative cohesion is the property an entity has when there are causal interactions between its components that affect the structural or functional nature of those components.* We shall later expand upon the notions of function and structure, but at an intuitive level, it should be easy to see that the trout's parts are both functionally and structurally integrated in virtue of the causal interactions and activities between them. For example, the activities of the hypothalamus of the fish's brain cause cells near the fish's kidney to release adrenaline into the fish's bloodstream, which in turn affects every cell in the fish's body. The adrenaline affects the functioning of cells and systems, typically by speeding up the activities of those cells and systems. The adrenaline affects the structure of the fish's parts as it allows muscle cells to adjust their structural relations to one another as they contract and expand. In similar ways *all* of the fish's parts causally interact such that *all* the parts are integrated with one another in at least a serial, chain-reaction type of fashion. At some (difficult to discern and inherently fuzzy) time in the fish's early life history, the fish's parts and the causal interactions between them *collectively* activated the integrative cohesion of the fish and it has displayed the property ever since. As such, facts beyond the fish's skin are background conditions to the determination of integrative cohesion. It is in this sense that integrative cohesion is always an intrinsic property.

So there is an intimate and necessary connection between the inner workings of the fish's parts and the fish' integrative cohesion; now let us contrast this fact with an example of responsive cohesion.

Imagine that a girl is fishing with her father and manages to catch our exemplar brook trout. As the father takes the hook out of the fish's mouth, he decides they shall keep it for dinner. He lays the fish on it side, along the bottom of the boat and the girl and father continue fishing in the hot afternoon sun. The causal interactions between the fish's parts begin to slow, but imagine for now that this takes some time and the integrative cohesion of the fish persists (whether such cohesion ceases upon death is a further issue). While lying in the sun, however, the fish comes to demonstrate an instance of responsive cohesion that is unrelated to its integrative cohesion. *Responsive cohesion is the property an entity has when its components respond to a pressure as a unit.* The fish's skin cells respond to the desiccating pressures of the dry, sunny environment independently of one another, but in an additive fashion, such that they respond as a cohesive unit. That is, despite the fish's protective slime coat and the scales embedded in its skin, each skin cell begins to dry out and because of the independent interaction between each skin cell and (say) the hot sun, the fish as a whole becomes desiccated. As such, the fish displays responsive cohesion in virtue of the independent but additive causal interactions in which its parts partake.

We shall see that additive interactions as such are not the only sorts of interactions that can give rise to responsive cohesion. But they can give rise to it. This is one crucial difference between responsive cohesion and integrative cohesion, for we have seen that independent but additive causal interactions will not do for integrative cohesion. Furthermore, this difference stems from a more basic difference between the two cohesions. Additive interactions will often suffice for responsive cohesion because this cohesion characterizes an entity's response to an external pressure such that this cohesion is a relational property, unlike integrative cohesion. The responsive cohesion the desiccated fish displays is a relational property because the pressures of desiccation are external to the fish. In such cases, facts beyond the cohesive entity play a crucial role in the determination of responsive cohesion. The sun and the dry environment of the fish more generally are not mere background conditions of the fish's desiccation. The intrinsic states of the skin cells *may* play the most crucial role in bringing about this particular instance of responsive cohesion in the fish. But responsive cohesion as such is a relational property because pressures external to the fish also play a crucial, determining role, and indeed are at least partial causes of the intrinsic states of the skin cells. In short, as the fish lies on the bottom of the boat it displays integrative cohesion in virtue of causal interactions between its parts which integrate those parts; and it displays responsive cohesion as its skin cells causally respond to an external pressure independent of one another but as a unit.

We can generalize the contrast between these examples of the two cohesions as follows. Integrative cohesion *does* consist in certain causal interactions between components and does *not* consist in the response to a pressure. The opposite is true for responsive cohesion. It does *not* consist in certain causal interactions between components and it *does* consist in the response to a pressure. Even though the brook trout displays both kinds of cohesion while it lays on the boat bottom, there is no necessary relation between the two instances of cohesion. These properties are, therefore, separable in nature.

Notice that the girl and father may likewise demonstrate unrelated instances of both integrative and responsive cohesion. Surely they demonstrate integrative cohesion, as causal interactions between their parts integrate those parts. But if they forgot to apply sunscreen they may also display responsive cohesion, as each of their skin cells burn in the sun. The cells respond as a unit insofar as each person has sunburn. It is, for example, as a cohesive unit that the father will suffer from the fever (e.g. heatstroke) that accompanies the sunburn, just as the fish suffers from desiccation. In an improbable thought experiment, the father's skin cells may have responded cohesively as they did even if they were pulled apart from one another and enjoyed some autonomy in space where yet they each still interacted with the sun. Severing causal interactions between parts, however, is just what integrative cohesion cannot tolerate, because it consists in such interactions and not in how the parts respond to external pressures.

With an initial sense of the distinction between integrative and responsive cohesion, we begin to see how the MICCI has traction. Because the caught brook trout displays integrative cohesion, the fish has cohesion that satisfies the MICCI. Though we know the fish is an individual, its integrative cohesion alone, as we have sketched it, does not guarantee it is an individual, since only a minimal criterion is satisfied. On the other hand, the responsive cohesion of the fish that we have discussed does *nothing* to satisfy the MICCI because it results from causal interactions that are not between the fish's parts, as the MICCI that captures our agreed intuitions about individuals requires. Thus, we see that responsive cohesion *does not preclude* individuality; it is just that in this case, responsive cohesion is *not indicative* of individuality.

Now, to see that integrative cohesion and responsive cohesion can be properties of what seem to be groups as well, consider one husband-and-wife couple, a second husband-and-wife couple, and their respective penchants for tennis and golf. Andy and Mandy sometimes play tennis with Akiko and Machiko. When the four play a doubles match, we might say that *the* group of four displays integrative cohesion. While playing tennis, there are certainly causal interactions between the players. For example, Mandy considers the position of each player on the court when after Akiko hits a ball near her, she tells Andy "I've got it," and then returns the ball near to Machiko. All four players are integrated in at least a functional sense; the presence or activity of each person affects what the other *does* (see Mishler and Brandon 1987, 399-400). In a somewhat loose sense, the players seem structurally integrated too, where the structure of the group of four refers to the members' specific spatial relations to each other. The players are not structurally attached as with the parts of a fish, but the spatial relations between them owe in part to the causal interactions between them. Where on the court Andy stands in relation to Mandy will depend upon the positions of Akiko and Machiko. Again, in this case as always, the integrative cohesion is a property intrinsic to the group: the players *in* the group and interactions *between them* collectively activate the group's integrative cohesion. The gravity, and oxygen, and so on, which they require for their play, are mere background conditions of their integration as such.

After their tennis match, Andy, Mandy, Akiko and Machiko drive to a nearby golf course and enter a couples golf tournament. The organizers split up the foursome, so that Andy and Mandy play through the course with two other strangers, and Akiko and Machiko play in another foursome with two other strangers. There are many such foursomes competing in the tournament, but it is the highest scoring four individuals in the tournament pool as a whole that advance to a final round where they each play-off in a four person round. Winner takes all. As it happens, Andy, Mandy, Akiko and Machiko register the four highest scores. As such, they respond as a unit to the pressures of the tournament—they advance to the final round as a cohesive entity set off from all the other players who do not qualify. This group of four now demonstrates responsive cohesion and does so in virtue of independent but additive interactions. Perhaps the crucial factors in this example of responsive cohesion are the golf scores of each Andy, Mandy, Akiko and Machiko, for it is in virtue of those scores that they respond to the tournament pressures as a unit. But these scores did not derive from interactions between players. Instead, the scores derive from each player's independent interactions with (say) course topography, weather, media pressures, and so on. Presume for now that we can make sense of the pressures of the tournament being *external* to the group of advancing golfers. This may raise issues of boundedness similar to those we introduced in chapter one, but for the sake of argument allow that anything *beyond the players and the direct causal interactions between them* are external to the group they constitute. If so, then the responsive cohesion the group of advancing golfers displays is again a relational property. Facts beyond that group play a crucial role in determining the group's responsive cohesion, even if the states of each golfer are the most crucial factors.

In the tennis and golf examples, integrative and responsive cohesion are not cotemporaneous properties, as they were in the trout. Integrative cohesion comes about during a tennis match and responsive cohesion during a later golf tournament. But we could have easily constructed an example where the cohesions were contemporaneous. And as I have presented them, we still see how the two cohesions are independent from one another and are thus distinct properties and distinct kinds of cohesion. If while playing *tennis* the four friends do not keep score and there is no tournament, there seem no relevant pressures to which they are responding as a unit. They do not display responsive cohesion. But with the players in the tennis group seeming integrated in the relevant senses, that group satisfies the MICCI. Again, this does not guarantee individuality, but a minimal criterion is satisfied. On the other hand, while playing *golf,* the four players exemplified responsive cohesion in the group they constituted as they advanced to the final round, and yet the group displayed no integrative cohesion. The responsive cohesion of the group in the golf tournament is not indicative of individuality at all.

We now have an initial grasp of the distinctions between integrative and responsive cohesion. Before analyzing these kinds of cohesion in terms of realization, I want to appeal to the golf example to raise three additional and important points that get us into the difficult details of cohesion. To grasp the first two points, consider that Andy and Mandy were playing through the course together, but separately from Akiko and Machiko. Perhaps the score of each of the four was so good, that mathematically the four of them had succeeded as the advancing golfers by the time they each reached the 16th of 18 holes in the first round. If so, while the couples continued playing through to the end of the first round, their causal interactions with the course topography, and so on, had already reached a sort activation point that gave rise to responsive cohesion. They had already cinched their success. The couples, completely separate from one another, formed a responsively cohesive group with respect to the tournament process. However, between the persons *in each couple* there may have been causal interactions that were significant to bringing about the responsive cohesion of the group of advancing golfers. For example, while Andy and Mandy played through the 18 holes, they might have found that Mandy had better golf balls than Andy did and so perhaps the two shared the good balls and this contributed to their high scores. Perhaps Akiko and Machiko shared strategy. So although *all four players* were not integrated with each other and the successful *group* did not display integrative cohesion, Andy might have been integrated with Mandy in a sense, and Akiko similarly might have been integrated with Machiko.

The first additional point to make, then, is that components in an entity can causally interact such that they are integrated in a sense, when yet the entity they in part constitute does not display the property "integrative cohesion." This helps explain how the integration between some parts of an entity does not guarantee the integrative cohesion of the entity. Though integrative interactions existed within couples, they did not exist between the couples. Thus, while the four played through the first round they were not causally connected (and so not integrated) in even a serial sense.

The second additional point to make, in light of the first, is that the responsive cohesion of an entity can sometimes seem to *owe in part* to integrative causal interactions between components, without owing to or indicating that there is also integrative cohesion. The responsive cohesion of the successful group of golfers might have come about in part because of the interactions between Andy and Mandy on the one hand, and between Akiko and Machiko on the other. I said above that responsive cohesion is distinct in one sense from integrative cohesion in that responsive cohesion does not owe to certain types of causal interactions in the strict way integrative cohesion does. We see that here. Responsive cohesion can owe in part to independent but additive causal interactions, but integrative interactions between components can have a role too. However, having made our first additional point, we see that integrative causal interactions involved in responsive cohesion do not entail that integrative cohesion is also involved.

The third additional point to make requires tweaking our example a bit. Imagine that it is not the four highest scoring individuals that advance to the final round of golf. Instead it is the two highest scoring couples. Thus, Andy and Mandy might advance in virtue of their strong combined score, but they may do so even if they each responded to the "external pressures" in quite different ways. Mandy's score is so strong that it compensates for Andy's poor score. In this sense, responsive cohesion can have a *componential origin,* where responsive cohesion comes about in part because different components bring about the required states in very different though componential ways. And responsive cohesion that has this componential feature need not imply that the differently responding though componentially producing components are integrated as they are in the tweaked example. For instance, Andy and Mandy might have been split up at the start of the tournament and forced to play without interacting with each other at all, and yet having registered as a couple, Mandy's strong score still compensates for Andy's while Andy plays with other golfers, and thus Andy and Mandy advance along with Akiko and Machiko in a group that displays responsive cohesion.

The difficult details of cohesion that we have begun to appreciate are important within biological contexts because the biological world is complex and non-standard instances of cohesion tend to profusion. So let us summarize our three additional points in a way that will recall to mind their explanation when we appeal to them in later chapters. First, we have the *integrative-interactions-are-no-guarantee* point, which says that integrative cohesion need not obtain just because some integrative interactions do. Second, we have the *responsive-cohesion-can-owe-to-integration-without-owing-to-integrative-cohesion* point, which simply builds on the first point, but within the context of responsive cohesion. Finally, we have the *responsive-cohesion-can-have-a-componential-feature* point, which, in light of the previous additional points, also implies that the componential feature of an instance of cohesion may or may not have to do with integrative interactions.

Now, to offer a more global summary of our sketch of the two cohesions, we have seen that both cohesions can obtain in individuals and groups. Integrative cohesion does *not* consist in response to a pressure and is always an *intrinsic* property. Contrastingly, responsive cohesion *does* consist in response to a pressure and is a *relational* property.

As such, integrative cohesion always requires causal interactions between an entity's components that integrate those components in a structural or functional sense; and it forms the basis for the MICCI that prospective individuals and their cohesion must satisfy.

Responsive cohesion, on the other hand, does not always require causal interactions between components and may indeed owe to independent but aggregative interactions that ensure it is a relational property; thus, responsive cohesion *alone* does not satisfy the MICCI and is not indicative of individuality.

Finally, we got an initial appreciation of how the two cohesions are distinct kinds of cohesion such that they can come apart in nature. One can come about in the absence of the other, as in the tennis and golf examples. Or, the two can come about at the same time, in the same entity, but nonetheless be unrelated, as in the case of the brook trout that laid on the boat bottom, displaying both integrative cohesion—in virtue of certain interactions between its parts—and responsive cohesion—in virtue of its parts responding independently to a pressure, but responding as a cohesive unit nonetheless.

It is time to take our initial characterizations of the two cohesions and analyze each in terms of realization. Thinking of cohesion in terms of realization will afford additional insights into the relation between the two cohesions and will sharpen the insights we have already gained.

3. REALIZATION AND COHESION

For over forty years authors have invoked the concept of realization within the philosophy of mind and psychology, though only recently have authors begun theorizing about the philosophy of realization itself (R. A. Wilson 2004, 100). This theorizing has leant insights into the nature of realization that are not uncontroversial but seem to me plausible and I will draw most of what I say about realization from R. A. Wilson (2004, ch.5-6; 2005, ch.3) and R. A. Wilson and Craver (in press). Authors have not yet applied the concept of realization to the concept of cohesion, but the application seems natural enough. To begin, let us clarify the notion of realization.

Metaphysicians commonly talk of relations of *determination*, where some property, relation or state determines or fixes another property, relation or state. *Subjects* such as entities or events are typically what *have* the properties, relations and states that are either determining or determined. Many metaphysicians think of causation, for example, as a *diachronic* two-place relation of determination that holds between entities or events. Thus, the property or state of one entity can fix or determine the property or state of another, over time: *C* causes *E*. In contrast to causation, realization is a *synchronic* two-place relation that typically holds between entities or events at "different levels" and "at a time" (R. A. Wilson 2005, 33). Realization is a softened sort of identity relation. So, if *R* is a realizer and *P* is a property that is realized, it is not that *R* (at a lower level) just is *P* (at a higher level) in a way that allows us to reduce *P* to *R*. Rather, the realized property, relation or state is metaphysically distinct from the realizer, even though the relation occurs at a time.

R is not *P*.

R does not cause *P*.

R realizes *P*.

None of this means that causation has no role to play when it comes to realization. Let us imagine a certain *state* realizing another *state*. The state of Andy's nociceptive system (a system at a "lower level") realizes the mental state of pain (a property at a "higher level"). Being in this mental state, we say Andy has the property "being in pain." For Andy's mental state to be realized as such, the nociceptive system need only be in a certain state, but being in that state might derive from causal interactions. Causal interactions might *activate* the lower level state that realizes the higher level state. For example, some stimulus may cause C-fibres in the nociceptive system to fire. When enough of these causal interactions occur, firing C-fibres will activate a state of the nociceptive system that satisfies a particular activation threshold, such that *at the moment* the activation threshold is reached, the state of the nociceptive system realizes Andy's mental state: Andy is in pain. Here, the "lower level" is the nociceptive system and the "higher level" is Andy's mental state. The nociceptive system does not cause Andy's pain, but causation has a role to play in bringing Andy's nociceptive system to the state that will realize Andy's pain, across "levels," at a time.

The relation between causation and realization will be critical in following chapters and so I want to generalize it here as follows: *diachronic causal interactions activate the states that synchronically realize properties.*

Let us apply this to cohesion. We might do so in two steps. First, causal interactions among the "lower level" of components ensure that those components are in particular states. Second, certain of these states will satisfy activation thresholds and thus realize the cohesive state of the "higher level" entity. If so, the entity has the property "cohesion." Thus, diachronic causal interactions do have a role to play in bringing about cohesion. In the *first step,* these interactions are crucial at the lower level (components) as they bring about certain states of the components that the realization of cohesion requires. But the actual realization of cohesion occurs in the *second step,* where the state of the components and not the causal interactions *per se* realize the cohesive state of the entity at a time. Construing cohesion in this way allows us to say that *causal interactions activate the states that in turn realize cohesion.* In our sketch of section 2, I captured the relation between the causal interactions and states involved in cohesion by saying that causal interactions "collectively" realize cohesion, and we may continue to speak loosely as such when it not misleading.

Knowing that states realize cohesion, we now need to get clearer on what those states are states *of.* I have spoken loosely of the state of components, but more precisely components are parts of *systems* that have certain states (R. A. Wilson 2004, 108). For instance, we saw that the state of Andy's nociceptive system realized his pain, where the state of that system owed to certain causal interactions among its parts. Thus, the states *of lower level systems* realize states and properties *of higher level subjects*. The realized properties of subjects are indeed properties *of the subject* because the relevant lower level systems are systems *of theirs* (142). In cases of cohesion, which system comprises the components whose interactions are relevant to realizing cohesion will depend on the case of cohesion in question. In the case of the responsive cohesion of the brook trout, the causal interactions between skin cells and the sun ensured the cells collectively reached a state that realized cohesion. It seems, then, that the relevant system was the integumentary system, which, depending on the organism, consists of skin, hair, nails, scales, fur, and so on. This is a hasty assessment though, and in the next section I will explain how the integumentary system is only part of a larger system that realizes the fish's responsive cohesion.

In the case of the integrative cohesion of the fish, the integrative interactions among all of its parts ensured those parts where in a state that realized integrative cohesion. Thus, it was the state of the *bodily system* more generally that realized integrative cohesion. Causal interactions between parts of the system activated a state of the bodily system that satisfied an activation threshold, which in turn realized integrative cohesion. I mentioned that this activation point was satisfied at some moment early in the life history of the fish.

Summing up the applicability of realization to cohesion thus far, recall our most basic characterization of cohesion from chapter one. The concept of cohesion refers to causal unity: an entity demonstrates cohesion when its components are causally unified. The components of an entity that are relevant to its particular cohesion are also components of a certain system. Causal interactions among components activate a state of this system that realizes cohesion or causal unity of the entity more generally. Put another way, causal interactions collectively realize cohesion. Now, let us apply the language of realization in a more directed manner to each kind of cohesion.

4. KINDS OF REALIZATION AND KINDS OF COHESION

There are different kinds of realization. One way to begin to appreciate this is through the idea that cohesion can be an intrinsic or a relational property. Integrative cohesion is always intrinsic, but responsive cohesion is relational. Moreover, the pressures to which responsively cohesive entities are related are external to the entity. Thus, causal interactions extending beyond the boundary of the cohesive entity, together with other entities beyond the boundary of the cohesive entity, constitute facts that crucially determine the cohesive entity's responsive cohesion. When we find determining facts beyond the boundaries of the cohesive entity as such, responsive cohesion has a *wide realization,* as opposed to the *intrinsic realization* of integrative cohesion. Wide realizations and intrinsic realizations are two of at least three kinds of realization. There are also *radically wide realizations*. To understand these three kinds of realization, let us turn to perhaps the most recent articulation of them, which we find in R. A. Wilson (2005, ch.5-6).

The distinction between kinds of cohesion are based in part on two fundamental features of realization: first, on the "parts" of any given realization, and second, on the idea that realizations are "metaphysically sufficient" for the properties they realize.

By "parts" of a given realization, I refer to what R. A. Wilson calls core realizations, total realizations and noncore realizations.

Core realizations are the properties or states that we take to be the most crucial parts of some realization. What counts as a core realization will often be "up to us" in important ways, just as deciding what "the" cause of a house fire might be. As R. A. Wilson (2005, 108) puts it,

> in the special case of the core realization of a property, conceived of as the most salient part of some larger system in which that property is instantiated, we have an epistemic dimension...What we find of greatest causal salience depends on our conceptual and perceptual abilities. It also depends on the questions we ask, the background information we have, and, more generally, our epistemic orientation.

The epistemic dimension of core realization will not completely confound our attempts to say what the core dimension of some property, state or relation is. It is not just "causes" that science does a reasonable job of discerning, but also "core realizers."

Total realizations are the states of the *systems* in which we find core realizations. With core realizations being parts of the systems that form the total realizations, there is a sense in which core realizers are parts of total realizations. From our example above then, the state of Andy's nociceptive system is the total realization of his being in pain, where the core realization is the firing of a certain C-fibre or a group of C-fibres within the nociceptive system (108-109). Importantly, total realizations are not plagued with and do not simply inherit the epistemic dimension of core realizers, because total realizations are states of systems that have objective boundaries in the world (109-110; 138-139). Systems are not mere spatiotemporal regions but instead are entity-like things in their own right, even if their boundaries are inherently fuzzy (134). Rather than inventing the nociceptive, circulatory and digestive systems, we discover them (139). Whether the strength of the heart is the core realizer of the property of having a heart rate of 120/80 may be up to us in some senses, but facts such as how the heart works, how blood is distributed through the body, and how parts of the circulatory system connect with world outside of circulatory system (e.g. where oxygen comes in and carbon dioxide goes out), are facts that determine what constitutes the circulatory system (139).

Given our understanding of core realizations and total realizations, it is easy to see that *noncore realizations* will be the parts of the total realization other than the core realization (108-109). If there is a case to be made for the state of the heart (e.g. how strong is it?) and the state of the arteries (e.g. how clogged are they?) being the core realizers of a certain blood pressure, red blood cells may be noncore realizers. As parts of the relevant objectively delimited system though, they will not be mere background conditions of the realization of a certain blood pressure.

Moving on from the "parts" of realizations, the second fundamental feature of realizations upon which we can base distinctions between kinds of realization has to do with metaphysical sufficiency. Authors take realizations to be metaphysically sufficient for the properties they realize: they exhaustively constitute what is sufficient to bring about the property in question. Given this "metaphysical sufficiency thesis" and our understanding of the "parts" of a realization, R. A. Wilson argues that many realizations in the biological world will be *context sensitive*. Indeed, we need not appeal to parts of realizations to see this, for aside from the parts mentioned above, we commonly think of realizations as having background conditions (i.e. the context that is "in back of" the system that marks out the total realization) that are metaphysically *necessary* to bring about properties. We have seen that background conditions are objectively demarcated from the total realization that they are "in back of," but another key feature of background conditions is that they often extend beyond the boundaries of the subjects that bear the properties being realized, not just beyond the systems that contain the core realizations of those properties. Given that realizations must be metaphysically sufficient, and that beyond-the-subject conditions are often necessary for the properties being realized, many realizations will at least be context sensitive.

For example, take the realization of the following property of the brook trout: "having a blood plasma salt concentration of 0.9 percent." For short, call this the "point nine property." The core realizer of this property might be healthy kidneys, or abundant ATP energy at the ready in the gills where chloride cells pull chloride and sodium ions into the blood. The system in which these core realizers are contained is the trout's water balancing system. Thus, the total realization may be the state of that system when the point nine property is realized. The core realizers alone are not sufficient because those realizers need to be located in a certain way within the water balancing system. The background conditions that are required for these "certain ways" to hold include the water in which the trout must reside if its water balancing system is to function at all (see Turner 2000, 17-18 for the relevant fish physiology discussed above).

Given what we know of metaphysical sufficiency and the parts of realizations we can now appreciate the three kinds of realizations.

Intrinsic realizations are those in which the system containing the core realization of the property in question is contained entirely within the subject of the property. In other words, the total realization is entity bound. It is intrinsic to the subject of the property. Both the point nine property of the trout and Andy's being in pain have intrinsic realizations. More importantly, integrative cohesion always has an intrinsic realization and the necessity of this distinguishes it from responsive cohesion. The causal interactions among parts that are characteristic of the cohesion of individuals are always between parts, and those parts and interactions constitute a system that is entirely within the bounds of the cohesive entity whose integrative cohesion is realized by the state of that system. Put another way, the total realizations of integrative cohesion are intrinsic to cohesive individuals, as in the case of the state of the trout's bodily system that is the total realization of its integrative cohesion.

But as my discussions of relational properties in the last chapter and of responsive cohesion in this chapter indicate, there are many properties in the biological world that have total realizations that extend beyond the "boundaries" of the subjects that have those properties. For example, the property "being a predator" is a relational property that will have a *wide realization* rather than an intrinsic one. The core realization of being a predator may be having particularly sharp teeth, which is a property that is intrinsic to, say, the larger cannibalistic trout that prey upon smaller trout. But here, the larger trout is only a predator in virtue of its relation to the smaller trout. More specifically, the relevant system is the predator-prey system. The total realization, then, is the state of the predator-prey system. That system extends beyond the boundaries of a trout, thus ensuring that being a predator has a wide realization. Not just the background conditions here are wide (R. A. Wilson 2004, 114-115).

This helps clarify the nature of responsive cohesion. Responsive cohesion has a wide realization as the trout's responsive cohesion did when it was desiccating in the boat. In that case, the trout's integumentary system was part of a larger system that extended beyond the trout's body, and the state of that wide system was the total realization of the trout's responsive cohesion. That system consisted of the entities beyond the trout with which the trout's skin cells interacted when becoming desiccated, and so that system also included the corresponding wide causal interactions that helped activate its state that served as a total realization of the trout's responsive cohesion. Again, this *wide system* will have a fuzzy boundary, such that it may be impossible for us to determine exactly which portions of the trout's environment were more than mere background conditions of the realization of its responsive cohesion. Yet this represents our limitations and does not imply there is no objective, fuzzy boundary around the wide system that realizes the trout's responsive cohesion.

Finally, realizations can be *radically wide* when not just total realizations and background conditions extend beyond the subject, but when even the core realizations are not entity bound either. Examples here are realizations of social actions. The most likely choice for the core realization of my voting for party X is my marking a piece of paper. This realization occurs outside the boundaries of my skin at a place where the pencil I hold meets the paper I mark (116). In this case, the action has a radically wide realization. I think it is doubtful that radically wide realizations will come to have traction in the case of responsive cohesion. The states of *some* internal components of a responsively cohesive entity will seem always to be crucial such that those states are at least parts of core realizers of responsive cohesion.

Summarizing the application of kinds of realization to our insights into kinds of cohesion, we can say that the causal interactions that activate states that realize integrative cohesion will always be entity-bound such that integrative cohesion has an intrinsic realization. If an instance of cohesion does not have an intrinsic realization, it does not satisfy the MICCI and is not indicative of individuality. The causal interactions collectively realizing responsive cohesion, on the other hand, will ensure that cohesion has a wide realization. The operative causal interactions will activate the state of a system that stretches beyond the bounds of the cohesive entity.

The final aspect of realization to explore is *kinds of realized properties*. This exploration will help clarify what it means for integrative cohesion to involve causal interactions that integrate components in a functional or structural sense. It will also help contrast such integration with the independent-but-additive sorts of interactions that can characterize responsive cohesion.

5. KINDS OF THINGS REALIZED

In a forthcoming paper, R. A. Wilson and Craver have clarified the distinctions between kinds of things that can be realized. The primary distinction is between the *material realization of entities* and the *explanatory realization of properties*. Focusing on the second arm of this distinction, there are three distinct kinds of properties that can have explanatory realizations: *aggregative properties, structural properties,* and *activities.* Given these course-grained distinctions, there are at least four kinds of things that can be realized. When integrative cohesion is realized, which of these things is realized? When responsive cohesion is realized, which kind of thing is realized? To answer these questions we can return to our brook trout and consider, in turn, entities, aggregative properties, structural properties and activities.

Recall that the brook trout has a water balancing system that helps ensure that roughly 0.9 percent of blood plasma mass is sodium chloride (Turner 2000, 17-19; and further water balancing physiology details taken from here also). The fish's water balancing system must work to retain this salt concentration, combating diffusion and osmosis. When swimming in fresh water, salt wants to diffuse into the salt-poor water column and water wants to rush into the fish by osmosis. The key organs in the water balancing system are the gills and kidneys.

Specialized chloride cells in the gills utilize ATP energy to pull chloride ions into the fish, across the osmotic gradient. The chloride ions attract oppositely charged sodium ions at the same time and salt enters the blood plasma, as the fish requires.

The kidneys are constituted in part by numerous subunits known as nephrons. Nephrons are tubules where at one end they connect with capillaries of blood plasma in the body, and at the other end, they open into the water column in which the fish swims.

We find two key *structures* where the tubules connect with capillaries. First, there is the *Bowman's capsule,* which is a cup shaped expansion of the nephron tubule. Second, there is a knot of capillaries known as the *glomerulus,* which connects with the nephron tubule as it is enveloped by the cup structure of the Bowman's capsule. The connections between the glomerulus (capillaries and blood plasma) and the Bowman's capsule (nephron subunit of the kidney) form a porous filter between blood and tubule.

Structured as such, these connections facilitate *activities* among the parts of the capillaries and nephrons that facilitate the *functions* of filtration and reabsorption. *Filtration* occurs when blood plasma is forced across the glomerulus into the nephron tubule. The plasma that makes the journey into the tubule is mostly water, though it contains many salts and other solutes. Not making the journey are the blood cells and proteins that remain in the blood stream. Once into the tubule, the water and salts are on a sort of exit ramp leading out of the fish. For the fish's saline solution to be retained it is the water that must leave; the salts need to stay. Thus, the *reabsorption* function of the nephron tubule then takes over, whereby salts are transported back to the blood as the filtrate (i.e. water and salts) travels down the tubule. The filtration and reabsorption functions of the structures in the kidneys thus help retain needed salts, but of course, it is the structure and activities of the gills and their chloride cells that serve the function of bringing salt into the fish in the first place.

Now, within the context of the water balancing system, consider the realization of *material entities*. The nephron tubule is a material entity that is realized by its *material composition*. The different parts of nephron tubules, such as stretches of lipid bilayers and the atoms constituting those layers, realize the tubule. Such cases of realization appeal *only* to the parts of the entity, and to *all* the parts. As such, the realization of an entity seems not of much concern when discussing individuals and their cohesion. One thing that agreement upon the MICCI makes clear is that individuals are more than just their parts; those parts must be causally unified. Perhaps mere piles and heaps have a material realization, but the cohesion of individuals entails that individuals are realized by at least their parts *plus* the unifying interactions between those parts.

Consider now an *aggregative property* that is realized in the fish. The water balancing system of the fish contains red blood cells that face an osmotic problem similar to the one the fish as a whole faces. Red blood cells contain more organic solutes than extracellular fluid, which tends to pull water into the cells by osmosis, threatening to bloat and distort their characteristic disc-like shape until the cells burst (Alberts et al. 1994, 516). Two processes help combat this osmotic tendency. First, as we have seen, the water balancing system helps the fish retain nonorganic solutes (e.g. salts) within extracellular fluid, which counterbalances the solutes within the cells. Second, the cell membranes actively pump positively charged sodium ions out of the cell, through transport channels in the membrane's lipid bilayer (515). This results in a net current across the membrane of the cell and the net current of the cell membrane is an aggregative property: it is approximately a sum of the currents in each of the transport channels (see R. A. Wilson and Craver, in press, for a similar example). The mass of a pile of leaves is a similar property. The pile's mass is realized aggregatively by the mass of each leaf. Notice that unlike the material realization of an entity, not all the membrane's parts are involved in realizing its net current, while neither are material parts the only things involved. The causal interactions between the sodium ions and the transport channels ensure each of the transport channels are in particular states. That is, these interactions activate the states that, summed together, *form the core realization* of the "higher level" property that is the cell membrane's net current. Aggregative states form aggregative realizations that realize aggregative properties.

Characterizing aggregative properties as such shows, I think, that responsive cohesion will often be an aggregative property. When the trout lies on the boat bottom, the state of the wide system that includes its skin cells and portions of the dry environment form the *total realization* of the trout's responsive cohesion (i.e. desiccation). However, the *core realization* seems to be the aggregative state of the skin cells. Put another way, the causal interactions between each cell and the desiccating environment activate the crucial states of each cell; the state of the cells taken together, then, is aggregative in nature and does not owe to interactions between the cells; the aggregative state of the cells forms the core realization of the trout's responsive cohesion; as such, the trout's responsive cohesion is an aggregative property. Indeed, responsive cohesion will be an aggregative property whenever the operative causal interactions activating the relevant states are independent but aggregative in nature, such that core realizer states are aggregative in nature.

Importantly, the notion of an aggregative property is an ideal that, usually, is only more or less closely approximated (R. A. Wilson and Craver, in press). For example, aside from the sum of the currents of independent transport channels, the net current of a cell's membrane is also a function of the greater populations of solutes in and outside the cell (Alberts et al. 1994, 515). Therefore, a cell membrane's net current only approximates an aggregative property. We saw similar complexities in the case of the successful golf group's responsive cohesion, which owed in part to causal interactions between pairs of golfers. In that example, then, conceiving of responsive cohesion as an aggregative property may obscure the fact that causal interactions between golfers were important in producing the states of each golfer that together formed the core realization of the group's responsive cohesion. Such complexities will only have local resolutions. But the point remains that responsive cohesion will tend to approximate an aggregative property far more closely than will structural properties or activities, which will not approximate aggregative properties at all.

We can also find the realization of both structural properties and activities in the fish's water balancing system. *Structural properties* are realized by matter *plus* organization *and* interactions, or as R. A. Wilson and Craver put it, in the case of structural realization "lower-level properties are not summed, but may involve interaction and organization of the components."

Organization is key because certain structural properties would be impossible without certain organization. For example, the Bowman's capsule of the nephron tubule is a certain structure that is realized by the parts it comprises, but also by the way in which those parts are spatially related to each other, and perhaps by the way those parts are spatially related to components beyond the Bowman's capsule that help to give the capsule its characteristic cup shape and configuration.

Interactions can also be key for structural properties, as in the case of the red blood cell's disc-like structure. The causal interactions within the transport channels of the cell's membrane help ensure that nonorganic solutes are pumped out of the cell, so that water does not tend to rush in. Without these interactions, I have noted that the cell's structure begins to break down and can be destroyed altogether. It may be the state of a particular "nephron tubule system" or "red blood cell system" that realizes the structural properties of tubules and red blood cells, respectively, but those key activation states will derive in part from specific organization and causal interactions. We thus have reason to consider integrative cohesion to be a structural property. Integrative cohesion is collectively realized by the similar sorts of integrative interactions between components that R. A. Wilson and Craver think collectively realize structural properties.

Yet, in some senses, integrative cohesion also seems like the final kind of property that can be realized: an *activity*. As with structural properties, organization and causal interactions among components are involved in realizing certain activities. One thing that distinguishes the realization of activities from the realization of structural properties is the fact that activities tend to serve some function, such that the items involved in the activity have particular functions (R. A. Wilson and Craver, in press). We saw, for example, that the activity of filtration serves the function of exporting water from the blood system to the kidneys. The realization of this activity involves "parts" (e.g. glomerulus and Bowman's capsule), the organization of those parts (e.g. the particular fit of the glomerulus in the Bowman's capsule), and the interactions between those parts (e.g. the pumping of water and solutes from the glomerulus into the nephron tubule). The parts of the glomerulus and Bowman's capsule, then, have certain functions in virtue of the organization and interactions between them. In a sense, they are functionally integrated. Thus, activities are characterized by functional integration just as integrative cohesion can be.

If integrative cohesion is a sort of functional activity, then when it is realized a functional unit may also be realized. Consider that R. A. Wilson and Craver characterize "function" as follows: "the function of X is what X does or is supposed to do in the [activity] in which X operates." Thus, we can say that the function of the glomerulus is to move water into the kidneys during the activity of filtration. Understanding function as such implies there is a hierarchy of functions. For example, looking down "into" the activity of filtration, we see that not just the glomerulus has a function, but that certain parts of the glomerulus will have certain related functions too. Looking up "above" the activity of filtration, we see that the function of the glomerulus is just one piece of a large functional activity of the water balancing system more generally. The function of that system is (among other things) to maintain the fish's saline concentration. It seems to me, then, that the notion of function will *imply* that there is some greater functional unit that will make talk of functions intelligible. R. A. Wilson and Craver seem open to such an implication when they say that certain components of functional activities "perform specific functions, and in turn they are chunked together to form larger functional units."

A question now arises. Is integrative cohesion best characterized as an activity whose realization involves functional integration, or is it best characterized as a structural property whose realization involves structural integration?

I think we need only answer this question indirectly. Indeed, given the scope of our current task we can only answer this question indirectly. Notice, though, that the direct and complete answer that eludes us here would go a long way towards solving the more general riddle of *what individuality is*, and this, I think, demonstrates how the language of realization has the resources for articulating a long-standing metaphysical debate. Exploring this briefly will be instructive.

Howard Robinson (2004) thinks the three leading and competing metaphysical accounts of individuality are Wiggins' (1980), van Inwagen's (1990) and Ayers' (1974; 1999). We might say that each of these authors disagrees on how to answer the central question of this chapter: what is it for an individual to be cohesive? In attempting to fill in the MICCI, I am attempting to give a minimal answer to this question. Wiggins, van Inwagen and Ayers attempt complete answers.

Wiggins (1980) proposes that the sort of cohesion required for individuality will depend on the *kind* of thing each individual is, that is, it will depend upon the sortal concept under which the individual falls. Given this, each kind of individual will have a certain "principle of activity, a principle of function or a principle of operation." (70)

van Inwagen (1990) likewise thinks activity is important, but he does not endorse the sortalism of Wiggins. Instead, van Inwagen thinks an entity can only be an individual if it has a singular life activity. On this view there is only one general *kind* of individual: organisms.

If van Inwagen and Wiggins were to use my terminology, both of them would seem to agree that the integrative cohesion that constitutes individuality is functional activity of some sort. Ayers (1999) disagrees with these two authors. His view would instead characterize integrative cohesion as a structural property, since he thinks the cohesion definitive of individuals is material unity or *material cohesion*. Although Ayers thinks certain forms of activity will sometimes be responsible for material cohesion, as life activity may help constitute the material cohesion of an organism, nonetheless the material cohesion of individuals is ontologically basic for individuality (232). Ayers does not go into the empirical detail of material cohesion, but from his examples of materially cohesive entities (e.g. stones, ice cubes, pats of butter, organisms) such cohesion would seem to consist in the causal interactions that ensure particles are materially attached. From his work it is difficult to discern the degree to which such attachment consists in active interactions between particles, but in any case, such interactions seem important insofar as they are involved in realizing a certain structure and organization.

Articulating the leading metaphysical views of individuality in terms of realization helps isolate the disagreements between those views and main reframe the debate in a productive way. One wrinkle to work out within this reframing is whether Wiggins would indeed presume the cohesion of individuals is always activity of some sort, or whether he might think it can be a structural property as well; he may say that the sortal dependency of individuality ensures that individuality even straddles this course-grained distinction. In any case, articulating the views of the three authors in terms of realization and integrative cohesion is instructive in at least two senses. First, it shows more precisely how metaphysicians agree with the MICCI, that is, agree that something like integrative cohesion—cashed out in terms of function and/or structure—

is minimally necessary for individuality. Often being an aggregative property, responsive cohesion will not do. Second, it shows how to offer an incomplete characterization of integrative cohesion than will suffice for our task. Whether it is structural integration or functional integration (or both) that characterizes individuals, in either case all of the individuals' parts must be so integrated. For each of the three authors, all of the parts of an individual are caught up in a principle of function (Wiggins), singular life activity (van Inwagen) or material cohesion (Ayers). My indirect answer to whether integrative cohesion is an activity or a structural property is thus this. Integrative cohesion may be a realized activity, a realized structural property, or the coinstantiation of both, but in any case, there must be causal interaction between all of the components of the cohesive entity such that the components are integrated in a like manner. This helps sharpen the discussion of section 3, where we saw, via the *integrative-interactions-are-no-guarantee* point that surfaced in our golf example, that if only some components of an entity are integrated then the entity they constitute does not demonstrate integrative cohesion. Although the realization of structural properties and activities does not always involve all parts of the entity bearing these properties, in the case of integrative cohesion all parts are so involved.

6. DEFINING TWO COHESIONS

We can now summarize our application of the language of realization to the concept of cohesion in a way that will allow us to define the two cohesions.

Cohesion more generally is a property realized by the states of certain systems that are constituted by components and their interactions. Causal interactions among these components activate the states of the relevant systems, which in turn realize an entity's cohesion. For short, causal interactions among an entity's components collectively realize an entity's cohesion.

The systems whose states realize cohesion can be contained within the bounds of a cohesive entity or can be wide systems stretching beyond the bounds of the entity. The states of these systems are the total realizations of the corresponding cohesion. Thus, when the relevant systems are entity-bound, cohesion has an intrinsic realization; when they are wide, cohesion has a wide realization.

Cohesion can be an aggregative property, structural property or an activity. Aggregative properties are realized when the components of an entity that are involved in the core realization have a certain aggregative state that is activated by independent but aggregative causal interactions in which those components are involved. Structural properties and activities, on the other hand, are realized in a non-aggregative fashion, where the relevant realizing states are produced by spatial and causal relations between components that imply those components are integrated in structural or functional senses.

Given all of this, the definitions of the two cohesions are as follows.

Integrative cohesion: an intrinsic property of an entity that is realized collectively by causal interactions between the entity's components that integrate those components in a structural or functional sense such that all the components of the cohesive entity are so integrated, whereby integrative cohesion is a structural property or activity.

Responsive cohesion: a relational property an entity has when its components respond to a pressure as a cohesive unity such that this cohesion is collectively realized by causal interactions that may or may not be between components, and which may or may not have a componential nature, whereby responsive cohesion often approximates an aggregative property.

Notice that these definitions also capture in a vague way the difficult details of cohesion that I initially uncovered when raising the "three additional points" following our discussion of the golf group that displayed responsive cohesion. First, in the definition of integrative cohesion, reference to all the components having to be similarly integrated captures the *integrative-interactions-are-no-guarantee* point. Second, in the definition of responsive cohesion, saying that such cohesion may owe to causal interaction between components captures the *responsive-cohesion-can-owe-to-integration-without-owing-to-integrative-cohesion* point. Third, and also in the definition of responsive cohesion, reference to interactions of a componential nature captures the *responsive-cohesion-can-have-a-componential-feature* point. Recall that this last point implies not only that each relevant component need not be "similar" nor interact with an external pressure in a similar way, but also that "different" components can bring about a common outcome in a componential way without even interacting.

With the two cohesions now sharpened as far as we shall sharpen them, the final point to make involves how they can be coinstantiated. It is to the possibility of coinstantiation that I now turn.

7. COINSTANTIATION

A moment ago I said that responsive cohesion could involve integrative interactions but that this does not necessarily mean responsive cohesion will owe to integrative cohesion.

However, there are important instances when responsive cohesion *will* owe to integrative cohesion. Sometimes, causal interactions between components of an entity can activate states of systems that realize both integrative cohesion and responsive cohesion. Responsive cohesion does *not* closely approximate an aggregative property in these instances.

Consider again the brook trout. My description of the water balancing system responsible for the trout's stable saline solution may have made it clearer how the fish demonstrates integrative cohesion. The fish's parts are clearly integrated in functional and structural senses. However, while the fish maintains its saline solution, its parts are also responding to a pressure as a unit. The pressure is osmotic pressure—a pressure that seems both internal (i.e. owing to the fish's own saline concentration) and external (i.e. owing to the saline concentration of the water column). In virtue of its parts responding to osmotic pressures as a unit, the fish displays responsive cohesion.

To spell out what is happening here, think in terms of core realizations, realizer states and the causal interactions activating those states. In many instances of responsive cohesion, independent but aggregative causal interactions ensure components of a system derive an aggregative state—a state that forms the core realization for responsive cohesion. This was the case with the desiccating fish. The causal interactions between skin cells and the environment were independent of one another but aggregative. The skin cells derived an aggregative state that formed the aggregative core realization of the fish's parts responding to the dry environment as a cohesive unit. In that particular case, the aggregative state of the skin cells did involve similarity among the cells, rather than owing to a componential feature. Each cell became desiccated in a similar way, unlike when a good golfer and a bad golfer componentially produce a state involved in responsive cohesion.

In the case of the fish's parts responding to osmotic pressures as a unit, however, the key causal interactions *are* between components, i.e. the fish's parts. Integrative causal interactions between parts of the fish activate the state that forms the core realization of the fish's maintenance of salt concentration in the face of osmotic pressures. Therefore, integrative causal interactions are responsible for realizing the fish's responsive cohesion. In this case, the core realizers may be the "saline states" of all the fish's parts, especially cells. Interactions between parts of the water balancing system are especially important in activating these states, but we have seen that each cell maintains certain salt concentrations via interactions with extracellular fluid too. Thus, if we think of all the parts of the fish as having certain "saline states" in virtue of integrative interactions between those parts, then *integrative interactions* between *all* the fish's parts are what collectively activate the core realization of this instance of responsive cohesion (the total realization will still be the state of a wide system that includes the fish's parts but extends into the water column to include the osmotic pressures there). But saying there are integrative interactions of a like sort between all the fish's parts is just to say that the fish demonstrates integrative cohesion. Therefore, it seems in this case that responsive cohesion is *indicative* of integrative cohesion and thus individuality, and what allows it to be indicative as such is the fact that it seems to *owe to* integrative cohesion. The fish maintains its salt concentration against osmotic pressures and thus demonstrates responsive cohesion just because integrative interactions between its parts collectively realize its integrative cohesion. These interactions help ensure both cohesions are realized.

A few clarificatory points here, beginning with an elaboration on the notions that responsive cohesion "owes to" individuality and is "indicative" of individuality. There may be many cases in which responsive cohesion owes to integrative cohesion. Frequently, the parts of organisms seem to respond to pressures as a unit not just in virtue of aggregative interactions (as in the desiccating fish example) but in virtue of integrative interactions (as in the osmotic pressure example). If we detect an instance of responsive cohesion and wish to know whether it *owed* to integrative cohesion, often the best we can do is to decide whether the responsive cohesion is *indicative* of individuality. When integrative causal interactions activate the core realizer of responsive cohesion they only *indicate the possibility* of integrative cohesion because they represent operative causal interactions that are entity-bound; such interactions can provide a basis for the entity-bound or intrinsic total realization that integrative cohesion requires. The total realization of responsive cohesion will nearly always be wide, but so long as causal interactions activating its core realization are intrinsic, there is a basis for thinking that those interactions are also activating the necessarily intrinsic total realization of an instance of integrative cohesion. Case by case investigation will be required to determine just how indicative of coinstantiation an instance of responsive cohesion is. In the next chapter I will explore what is involved in positing coinstantiation in the case of species cohesion.

Second, during the coinstantiation of the two cohesions, the integrative causal interactions that activate the states that realize responsive cohesion need only be some of the integrative causal interactions that are involved in integrative cohesion. In the above example, interactions having to do with the maintenance of saline concentration are the ones that activate the states that realize responsive cohesion. But more than just these interactions between the fish's parts are involved in the fish's integrative cohesion. Nonetheless, there is a sense in which this instance of responsive cohesion would not be possible without certain integrative causal interactions that, in turn, would not be possible unless all the fish's parts were integrated in a like manner. It is in this way that responsive cohesion can owe to integrative cohesion. As a result of this clarification, we see that even when two distinct instances of responsive cohesion are indicative of integrative cohesion, one instance may be more indicative than the other because it depends upon, say, integrative interactions of a similar sort between many intrinsic parts of an entity rather than just some intrinsic parts.

Third, for the integrative causal interactions between components to surely be involved in responsive cohesion in a way that suggests the two cohesions are coinstantiated, *these causal interactions must indeed have the causal force to activate the states required for the realization of responsive cohesion.* If such interactions are found to be causally inadequate such that they are not the interactions that could activate the states that responsive cohesion requires, then we have no reason to think that responsive cohesion owes to integrative cohesion. Instead, it may be that the operative interactions are of an aggregative sort that is not indicative of individuality. Moreover, if the interactions are causally inadequate as such, there will be no reason to think they are the sort of integrative interactions that can causally unify the components of an entity in the way that parts of an individual are causally unified.

Finally, when responsive cohesion owes to integrative cohesion, it will *not* closely approximate an aggregative property. For in such cases, it is not merely aggregative causal interactions that active realizer states, but instead direct causal interactions between components that activate at least the core realizers. If an instance of responsive cohesion *does* approximate an aggregative property, it will seem clear that integrative cohesion is *not* coinstantiated with integrative cohesion.

In closing this section we can say that the two cohesions are sometimes coinstantiated when the integrative causal interactions between components that collectively realize integrative cohesion are also the ones that activate the states that are required for the realization of responsive cohesion. Responsive cohesion will often be merely more or less indicative of coinstantiation, and if responsive cohesion seems to be an aggregative property, there is no indication that the cohesion of individuals obtains in the entity.

8. THE MICCI REVISITED AND READY TO APPLY

Having completed an analysis of cohesion in terms of realization and distinguished between the natures of responsive cohesion and integrative cohesion, we are now in a position to extend our initial formulation of the MICCI, and then pull out the points of this chapter that will be most relevant as we turn to consider species cohesion.

Minimal Integrative Cohesion Criterion of Individuality (MICCI): To be an individual an entity must minimally display integrative cohesion, which is an intrinsic property of an entity that is realized collectively by causal interactions between the entity's components that integrate those components in a structural or functional sense such that all the components of the cohesive entity are so integrated, whereby integrative cohesion is a structural property or activity.

Formulated as such, the MICCI is still only a minimal criterion of the cohesion of individuals, but coupled with our now deeper understanding of the kind of cohesion that is *not* indicative of individuality, we shall be as prepared as we need be. Features of responsive cohesion will, when we can identify them, help to rule out an instance of cohesion as being the integrative cohesion the MICCI requires. Specifically, three crucial features of responsive cohesion help in this way. Each of them forms the basis for a sort of "sure sign" that the cohesion in question does not satisfy the MICCI, and I will refer to them as such in order that we can more easily recall the material from this chapter as we proceed.

First, there is the *wide realization sure sign*: if an instance of cohesion has a wide realization then we know it cannot be integrative cohesion, but rather is responsive cohesion (or some other cohesion as yet unidentified but not satisfying the MICCI).

Second, there is the *aggregative property sure sign*: if an instance of cohesion is an aggregative property, we know it will be responsive cohesion and not integrative cohesion, and that the MICCI is thus not satisfied.

Third, there is the *causal poverty sure sign*. There may be times when an instance of responsive cohesion is coinstantiated with an instance integrative cohesion and so the mere presence of responsive cohesion does not preclude individuality. But if the cohesion to which authors appeal is responsive cohesion and is to be indicative of individuality, *it must* be coinstantiated with integrative cohesion. In such cases, integrative causal interactions between components of a cohesive entity must have the causal force to activate the states that realize the responsive cohesion in question. If, however, interactions between components demonstrate a sort of causal poverty with respect to the states that responsive cohesion requires, then there is no reason to think the cohesion in question satisfies the MICCI—there is no reason to think the interactions between components are of the causally integrating sort that can causally unify components into an individual.

With the MICCI and its sure sign helpers in place, we now turn to the next chapter where I shall begin by recalling our formulation of species cohesion from chapter one, to show that this concept has been doing double duty for individualists. As authors often discuss it, species cohesion clearly corresponds to responsive cohesion. Thus, if species cohesion is to be indicative of individuality, it must also ambiguously refer to integrative cohesion that is coinstantiated with responsive cohesion. To determine whether such coinstantiation occurs, we must consult competing species concepts. Here, however, the modest clarification of Ereshefsky's with which we initially formulated the MICCI at the beginning of this chapter gains much traction. Most species concepts simply do not posit that the causal interactions relevant to species cohesion are between conspecifics. The only species concepts that offer the individualist *prima facie* hope are reproductively oriented ones, such as the BSC. We shall thus clarify the muddy conceptual waters surrounding the BSC and use the language of realization to structure the argument from gene flow on behalf of the individualist.

4

Species Cohesion and Gene Flow

Species consisting of populations that are bound by gene flow
satisfy the...notion of individuality

Marc Ereshefsky 2001

1. CONSPECIFIC SIMILARITY

AND THE DOUBLE DUTY OF "SPECIES COHESION"

If species are individuals, they must demonstrate integrative cohesion such that they satisfy the MICCI. Biologists and philosophers of biology frequently discuss "the" concept of species cohesion and individualists appeal to this concept when claiming that species are individuals. For the species as individuals view to go through, then, it is species cohesion, as commonly conceived, that must correspond to integrative cohesion.

In this chapter we explore what is involved in testing the common concept of species cohesion against the MICCI, and then begin that testing. In this first section I recall the notion of species cohesion introduced in chapter one and explain how to think of it in terms of realization. This will make clear what claims must hold in order for species cohesion to correspond to integrative cohesion, as the individualist requires. To see if such claims do hold, in section 2 we momentarily step outside of the debate over the species ontology problem and consider the species concepts that compete to solve the species definition problem. These concepts are a window into whether the individualist's required claims hold and upon considering them in further detail in sections 3 and 4, it becomes clear that only reproductively based species concepts such as the widely accepted BSC seem to offer any hope that species cohesion corresponds to integrative cohesion. Section 5 focuses on the BSC, explains how best to understand it, and how this understanding determines the sort of resource it will be for individualists. This allows us to construct the individualist's most powerful argument for species cohesion's satisfaction of the MICCI in section 6. Section 7 briefly explains how the next chapter shall object to that argument.

In chapter one, I explained how species cohesion refers to a sort of evolutionary unity. Each species is distinct from others in the sense that the evolutionary fates of its organisms cohere, ensuring that the species as a whole has a distinct evolutionary trajectory or fate. Drawing from a number of sources (e.g. Ruse 1987, 353; Ereshefsky 1988, 429; 1991, 89; Williams 1989, 301) I articulated species cohesion as follows.

Species cohesion: the causal unity a grouping of organisms has when the organisms are similar so as to share an evolutionary fate.

Articulated as such, we can see that in an instance of species cohesion (e.g. the cohesion that *Panthera tigris* displays) there is a relationship between "levels" just as we should suspect. The higher level entity is the species, whose cohesion refers to a distinct evolutionary fate. To have a distinct fate, the species' organisms must tend to share fates: they must each be similar *in an evolutionary sense*. Thus, the core realization of species cohesion seems to be the collective states of the conspecifics, and more specifically, that collective state refers to their *collective conspecific similarity*. This collective state at the "lower level" of the organisms realizes species cohesion at the level of the species. What activates the required state of collective conspecific similarity will be certain causal interactions. Causal interactions either between conspecifics or perhaps between conspecifics and other entities will cause conspecifics to be similar in an evolutionary sense. In sum, as it is commonly generalized, species cohesion is realized by the collective state of conspecific similarity and this state is activated by the *causes* of such conspecific similarity.

The key question that we must answer thus becomes this. *What is the cause of conspecific similarity?*

In light of analyzing cohesion in terms of realization, we know this question is key. Causal interactions activate states that realize cohesion. The distinctions between integrative and responsive cohesion often depend on distinctions in the kinds of causal interactions that activate the realizer states. Integrative causal interactions (where we construe integration in terms of structure and function) are always the causal interactions that activate the states that realize integrative cohesion. Aggregative causal interactions are often the interactions that activate the states that realize responsive cohesion, though other sorts of interactions can suffice for components to respond to a pressure as a unit and thus suffice for responsive cohesion.

Given our key question and why it is key, we can see what answer or claims must hold if species cohesion is to satisfy the MICCI. The causal interactions that activate the collective state of conspecific similarity must be integrative causal interactions between conspecifics that unify those conspecifics, or which "hold them together," as many authors say. For the individualist, causal interactions between conspecifics must cause conspecific similarity such that those conspecifics are integrated in either (or both) a functional or structural sense.

To decide whether the causes of conspecific similarity indicate that species are individuals, we can use our knowledge of both integrative and responsive cohesion. If the causes seem to be integrative causal interactions between conspecifics, there is reason to think species cohesion satisfies the MICCI; though to have significant reason as such we shall like to see that those interactions unite all conspecifics in a structural or functional sense. On the other hand, if it becomes clear that the causes of conspecific similarity are interactions that are of an aggregative sort, we shall know that species cohesion does not correspond to integrative cohesion. Likewise, if the causal interactions activating collective conspecific similarity extend beyond the species, we shall know that any instance of species cohesion will have a wide total realization, and thus will not correspond to integrative cohesion.

Before embarking on our search for the causes of conspecific similarity and deciding whether these causes represent the sort the individualist requires, two further points require discussion. The first of these regards coinstantiation and the second regards the complex notion of conspecific similarity.

Given the way I have said that authors commonly generalize species cohesion, from the get-go it is tempting to interpret species cohesion as though it corresponds to responsive cohesion, and I think we should. It takes only momentary reflection to see that for a species to have a distinct evolutionary fate as the concept of species cohesion suggests, its organisms will be responding to evolutionary pressures as a cohesive unit. In fact, the individualist Mary Williams (1992, 322) implies this explicitly when she summarizes the species as individuals view in terms of cohesion as follows. "The claim that species are individuals in biology is a claim that species are held together by cohesive forces (e.g., common selection forces on a common gene pool) so that they act as units *with respect to* the laws of evolution" (my emphasis). As an example, in an earlier paper (1989, 303) Williams points to the evolution of races within the butterfly species *H. melpomene* and *H. erato*. The external pressure or "common selection force" in these cases is the tendency for local bird populations to prefer eating certain colours of butterflies instead of others. Races of butterflies respond to this "external" force as a cohesive whole in the sense that, over time, components (i.e. individual butterflies) of the whole (i.e. race) will be coloured similarly.

Though it seems incontrovertible that species cohesion corresponds to responsive cohesion (also see Templeton 1989; Mishler and Brandon 1987, 400) we have seen that a majority of authors assent to something like the MICCI. Authors claim explicitly or otherwise that species cohesion owes to causal interactions between and which integrate members of a species. For example, recall that many authors think members of a species are unified in the face of evolutionary pressures just because interspecies interactions such as interbreeding and/or gene flow, are "quite literally what hold the parts [i.e. organisms] of the individuals [i.e. species] together so as to form the whole individual" (Dupre 1993, 46). Thus, without having distinguished between kinds of cohesion as I do, it is in an unwitting fashion that individualists seem to imply that a species' *responsive* cohesion *owes* to *integrative* cohesion. The causal interactions between conspecifics coinstantiate both cohesions and species cohesion ambiguously refers to both the resulting instance of responsive cohesion, and the resulting instance of integrative cohesion. "Species cohesion" is doing double duty.

Or at least, this must be the case if the species as individuals view is to hold. If we are right to claim that the common generalization of species cohesion corresponds to at least responsive cohesion, then to satisfy the MICCI, the responsive cohesion of species *must* owe to integrative cohesion. The observation that integrative causal interactions between conspecifics are what cause conspecific similarity would indicate that a species' responsive cohesion owes to, and thus implies the species has, integrative cohesion. We saw in the last chapter that for an instance of responsive cohesion to be indicative of integrative cohesion, the core realization of responsive cohesion must be activated by integrative causal interactions between components of an entity. Only this scenario indicates that there are operative causal interactions that are entity-bound and that could thus activate the state of an entity-bound system, as integrative cohesion and its intrinsic realization require. In the case of species cohesion, the core realization is the collective conspecific similarity within the species. *Therefore, integrative causal interactions between conspecifics must cause conspecific similarity* if species cohesion is to be a concept that is doing double duty, i.e. if the concept is to suggest responsive cohesion and integrative cohesion are coinstantiated in virtue of shared entity-bound causal interactions.

In terms more common to the literature on species cohesion, integrative causal interactions between conspecifics must be what "keep species together." And they must do so by preventing "differentiation in the face of different kinds of selection pressures at different places" (Ehrlich and Raven 1969, 1229), or more simply, by "keeping conspecifics similar" (1230).

The discussion thus far makes plain that the notion of conspecific similarity has a crucial role to play within talk of species cohesion and we need to be clear on what that notion does and does not mean. Williams' (1989, 303) above example of species being cohesive in virtue of conspecifics tending to respond to selection pressures by being of the same colour over generations actually misleads us here. Cases of *morphological similarity* may be vivid to us, but they do not do justice to the complex notion of *conspecific similarity*. Rather than referring to morphological similarity, it will pay to think of conspecific similarity as referring to a sort evolutionary similarity, as I briefly implied above. This is because conspecific similarity corresponds to the tendency for conspecifics to share evolutionary fates and as such, morphological similarity is neither necessary nor sufficient for such conspecific similarity.

Morphological similarity is clearly not necessary because males and females of a species can differ radically from each other in terms of morphology, yet still share evolutionary fates and thus exhibit conspecific similarity. Morphological similarity is not sufficient either, since organisms (e.g. mimics and viceroys) may be remarkably similar in morphological terms and yet not share an evolutionary fate and thus not exemplify conspecific similarity when, say, they each exploit different ecological niches. Therefore, we must try to see things through the eyes of evolutionary pressures. Conspecifics can be similar in the eyes of evolutionary pressures for a host of different reasons that imply there are many dimensions to conspecific similarity. One way to shore up the seemingly fragmentary notion of conspecific similarity is in terms of trait sharing. Let me further explain both the fragmentation and the shoring up.

Consider in a little more detail two cases in which conspecific similarity does not imply morphological similarity. First, Guy Bush (1969) showed that conspecific similarity might manifest in the form of similar food preferences when he found that maggot flies tended to share evolutionary fates in virtue of sharing a common food source, rather than in virtue of any particular morphological features. Certain maggot flies shared a certain fate because they each preferred hawthorn fruits, while other maggot flies shared a distinct fate because they preferred apple fruits. More specifically, even though the two sorts of flies were of the same species and shared the same geographic space, they diverged from each other because the micro-selection regimes on hawthorn fruits were different from those on apple fruits. The apple-preferring maggot flies thus came to share a distinct evolutionary fate from the hawthorn-preferring flies.

Morphologically distinct male and female birds, on the other hand, may tend to share evolutionary fates for a host of reasons. Biologists dispute which reasons are most important. They dispute over the nature of conspecific similarity in part because they think empirical research shows there are different causes of the tendency for conspecifics to share evolutionary fates. Part of the underlying disagreement here, then, is just what it is to share an evolutionary fate, and we shall consider this problem in the next section.

But just a glimpse here of the different reasons biologists offer for organisms tending to share evolutionary fates can further expose the many potential dimensions to the notion of conspecific similarity. For example, some biologists might say conspecific birds tend to share fates *because* they share a mate recognition system, such as a species-specific mating song (Paterson 1985). Others would say that the morphologically distinct birds manifest conspecific similarity in the sense that they share a certain adaptive zone or niche that is distinct from the niches of other bird species (Van Valen 1976). If this is the case, then conspecifics are similar in the eyes of evolutionary pressures *because* of shared habitat preferences and the selection pressures associated with them more generally (Horvath 1997, 661).

Thus, conspecific similarity can be manifest in morphological similarity, mating similarity, food-preference similarity, habitat-preference similarity, and so on. Nonetheless, in all of these cases there is something we might think is shared among conspecifics: evolutionary traits. In the eyes of selection, butterfly colour patterns are traits that butterflies of a species tend to share. Likewise, a preference for, or disposition to a particular mating system is a trait that morphologically distinct males and females of a species tend to share. From the vantage point of evolutionary pressures such as selection and drift, preferences for certain foods and habitats turn out to be evolutionary *traits* as well, which are shared by conspecifics such that conspecifics tend to share evolutionary *fates*. It is in these senses, then, that "conspecific similarity" is "evolutionary trait similarity." Thus, there is a sense in which saying "the collective state of conspecific similarity in a species is the tendency for conspecifics to share evolutionary fates," is the same as saying, "the collective state of evolutionary trait similarity in a species is the tendency for conspecifics to share evolutionary fates."

There is, mind you, some room to argue here that we should not think of the sharing of mating systems and habitats as individual-level traits cashed out in terms of preferences and dispositions. One might claim instead that some traits will be "group-level" traits. Having a certain mating ritual, for instance, seems a trait or property of a *couple*, or more generally a property of an *n-tuple* of conspecifics. This may complicate the notion of conspecific similarity, but not in any way that threatens our use of it. If one wishes to countenance group-level traits that ensure conspecifics tend to share evolutionary fates, then they will likely need to be sympathetic to at least some minimal form of kin- or group-selection. This merely implies that the collective state of conspecific similarity that realizes species cohesion will sometimes be, more precisely, the collective state of conspecific *group* similarity. For individualists, the causes of such group similarity will still have to be *within* and *between* such groups; the operative causal interactions must still be integrative and between all "parts." (Note: geographic distribution is sometimes posited as a species-level and not just group-level trait that helps make the notion of species-selection intelligible, but even in putative cases of species-selection, all conspecifics must be unified by the relevant interactions).

In completing the conceptual spadework of this section we have seen that when searching for the cause of conspecific similarity we are searching for what causes conspecifics to share *evolutionary traits*. These causal interactions will activate the collective state of conspecific similarity that is the tendency for conspecifics of a species to share evolutionary fates. This collective state in turn realizes the distinctive evolutionary fate of the species. That is, it realizes species cohesion.

Moreover, species cohesion seems clearly to correspond to responsive cohesion, so for the individualist the concept of species cohesion must be doing double duty, such that species cohesion as commonly generalized must also correspond to integrative cohesion. For this to be the case and for the MICCI to be satisfied, the causes of conspecific similarity must be integrative causal interactions between conspecifics that "keep the species together." In sum, if the key question in the case of species cohesion concerns the cause or causes of conspecific similarity, then the answer individualists require is that *integrative interactions between conspecifics cause conspecific similarity and activate the collective state of conspecific similarity that realizes species cohesion.*

2. SPECIES CONCEPTS

To determine whether integrative causal interactions between conspecifics cause conspecific similarity we must turn to the species definition problem because competing solutions to that problem derivatively or explicitly imply what it is that causes conspecific similarity. There are many complexities to the species definition problem, however, and to properly consider how the competing species concepts that attempt to solve it might inform our concern with the causes of conspecific similarity, let us get clear on some of these complexities.

In a paper that biologists and philosophers of biology often cite, Mishler and Brandon (1987) argue that defining the species category involves offering two different sorts of criteria: grouping criteria and ranking criteria. *Grouping criteria* concern what distinguishes species from other species. In virtue of what do we group organisms into a distinct unit? *Ranking criteria* concern what ranks a unit at the species level of the evolutionary hierarchy. In virtue of what is a species distinct from a genus or a subspecies? Because competing species concepts purport to solve the species definition problem, they typically contain both grouping and ranking criteria.

There are so many competing species concepts that in our search for the cause of conspecific similarity it is not possible to consider each one in turn, along with the implications of each for the individualist. For our purposes, however, many "competing" species concepts are virtually the same and, moreover, authors recognize more generally that species concepts cluster into groups of similar concepts. By appealing to a plausible categorization of concepts we can more readily draw generalized conclusions about the cause(s) of conspecific similarity.

Unfortunately, however, the typical way of taxonomizing species concepts is, I think, deficient and not likely to provide a short cut to general conclusions about the cause of conspecific similarity. Let me expose this deficiency and propose my own short cut.

The typical taxonomy is three-fold: a species concept either belongs to the *phenetic* family of species concepts, the *reproductive* family of species concepts, or the *genealogical* family of species concepts (see Ereshefsky 2001, ch.2; Dupre 1993, 44-52, R. A. Wilson 2005, 101). Problems arise with this taxonomy because many species concepts will fall into one family with respect to grouping criteria, and into a distinct family with respect to ranking criteria. For example, the BSC is widely and in some sense rightly regarded as a reproductive species concept because it says, "species [are] groups of interbreeding natural populations that are reproductively isolated from other such groups." (Mayr 1999, 17) However, Griffiths (1999, 210) notes that "[i]n most modern species concepts, including modern versions of the BSC, the grouping criteria is [sic] genealogical. Species must be characterized by some version of monophyly—descent from a single population, a single speciation event, or any similar unique point of origin."

One possible reason why the three-fold taxonomy persists is that authors *do* take it to consistently reflect differences in ranking criteria, if not in grouping criteria. This makes sense, given the centrality of ranking criteria. To appreciate the centrality of ranking criteria, consider that Griffiths thinks there is some measure of agreement over the genealogical bases of grouping criteria, but that there is widespread *dis*agreement over ranking criteria and this disagreement is the primary source of the intransigent reputation that "the" species problem now has (211).

However, I think the three-fold taxonomy can be misleading with respect to ranking criteria as well. At least three well known species concepts—Templeton's (1989) *cohesion species concept* (CSC); the *evolutionary species concept* (EvSC) developed by a number of authors; and Van Valen's (1976) *ecological species concept* (EcSC)—tend to resist categorization as either reproductive, genealogical or phenetic views with respect to ranking criteria. Templeton's CSC and the EvSC are pluralistic in the sense that a given population can be ranked as a species, rather than (say) a genus or subspecies, in virtue of a host of "processes," including reproductive processes, homeostatic processes and processes of natural selection (Horvath 1997, 600-661). Van Valen's (1976, 663) EcSC, on the other hand, ranks a population as a species based on a population's occupancy of an "adaptive zone minimally different from that of any other lineage in its range." It is not clear that this view is *either* reproductive *or* genealogical in nature. Indeed, it is ecological, as its name implies.

A perhaps more pressing reason why the three-fold taxonomy of species concepts breaks down even with respect to ranking criteria is because many of the concepts that get lumped into the genealogical family actually appeal to reproductive processes when accounting for the species rank of populations. Most "genealogical views," for instance, go by the name of one or another *phylogenetic species concept* (PSCs; e.g. see discussion in Wheeler and Meier 1999) and a number of these appeal to reproductive notions such as reproductive isolation (e.g. Meier and Willmann 1999, 32) or even interbreeding (e.g. Mishler and Donoghue 1982; Mishler and Brandon 1987).

There are PSCs that do *not* appeal to reproductive ranking criteria and they tend to be the species concepts of "pattern cladists." Even though Ereshefsky claims the species concepts of pattern cladists *do* appeal to hereditary relations as *processes* that can be the bases for ranking criteria, these PSCs certainly do not underscore such processes (e.g. see Cracraft 1983; Wheeler and Platnick 1999). Indeed, the pattern cladists' focus on patterns instead of processes is what distinguishes them from "process cladists" (see Hull 1999, 38-44). For the pattern cladist, patterns of shared traits that are phylogentically significant are central. The number and sorts of such shared traits that are required to rank a species as a species and not, say, a genus, is often left as an open question with only local resolutions.

In short, the diversity of PSCs alone help reveal the inadequacy of the typical taxonomy of species concepts.

Getting the taxonomy of species concepts right with respect to ranking criteria in particular is important because it is ranking criteria that derivatively suggest what causes conspecific similarity. They do so by first explicitly suggesting what it is for a species to share an evolutionary fate, for this explicit suggestion derivatively implies what will be the causes of the *trait sharing* that underlies such *fate sharing*. Indeed, sometimes species concepts even make the implied cause of conspecific similarity explicit as well. For example, the BSC and Paterson's (1985) mate recognition concept (RSC) both aver that conspecifics' tendency to share an evolutionary fate consists in their being genetically isolated from other populations (see Ereshefsky 2001, 85 for how Paterson holds his view while yet criticizing the *way* in which the BSC appeals to it). The BSC then *explicitly* implies that the cause of such fate sharing is interbreeding interactions. Interbreeding causes conspecifics to share traits and thus share an evolutionary fate. The RSC, however, takes issue with the BSC's account of the cause of conspecific similarity, as it says that the sharing of "a common fertilization system" among conspecifics causes conspecific similarity (85).

Given the importance of ranking criteria (for our purposes at least), and given the deficiency of the typical three-fold taxonomy of species concepts, I propose a different taxonomy. There are *pattern-based* species concepts and *process-based* species concepts. This is not an entirely original suggestion, for as I mentioned above, some authors already divide at least the different PSCs into process-based or pattern-based species concepts. My suggestion is that we apply this distinction across all species concepts. As such, the PSCs of pattern cladists would be pattern-based species concepts that do not directly appeal to processes that define the rank of species. The entire family of phenetic species concepts from the typical three-fold taxonomy of concepts would be pattern-based as well, since they define species in terms of overall phenotypic similarity.

Granted, the pattern-based PSCs are distinct in a sense from the phenetic species concepts that have largely fallen out of favour, since the former only appeal to "derived" evolutionary character states called synapomorphies when measuring similarity, while the latter do not have an evolutionary basis as such. However, an empiricist undercurrent flows beneath both sorts of concepts, as neither wishes to appeal to processes when ranking populations as species. As Ereshefsky (2001, 92) puts it, "[p]attern cladists, as pheneticists, believe that theoretical assumptions about processes cause biologists to misrepresent nature's patterns. Accordingly, they advocate species concepts that depend only on theory-neutral observations of pattern." Therefore, not only is it reasonable to categorize pattern-based species concepts as such, but we also see that these pattern-based concepts will not be of much concern to us. In attempting to depend only on theory-neutral observations of pattern, pattern-based species concepts do not posit causes of conspecific similarity.

Process-based species concepts *do* posit causes of conspecific similarity. The seemingly reproductive-based BSC and RSC clearly do so, as we saw. But so do all the process-based PSCs falling under the genealogical family in the typical three-fold taxonomy. And so do the previously-difficult-to-categorize concepts, such as the pluralistic CSC and EvSC, and the niche-based EcSC. As such, my taxonomy of species concepts seems to capture all the species concepts commonly thought of as the leading species concepts (see Ereshefsky 2001, ch.2).

This would seem to leave us with a remarkably lumpy "process" category of species concepts though. But conveniently, we can reduce the definitions that each process-based concept enshrines, and we can do so in a way that shows there are only three basic processes that biologists claim are causes of conspecific similarity. The processes are *developmental homeostasis, natural selection* and *interbreeding*. While operating within his own distinct taxonomy of species concepts, Chris Horvath (1997, 660) similarly reduces the "[t]he mechanisms that have been advanced as explanations for the cohesive behaviour of species" to these three processes. Ereshefsky (2001, 114) does so as well, while also noting that "Hull (1976, 1978), Wiley (1981), and Williams (1985) [also] suggest that these three process...are important in making species individuals." Let us now turn to examine these three causes over the next three sections. Doing so will show how they are indeed the three causes to which process-based species concepts appeal, and will allow us to see that developmental homeostasis and natural selection do not cause conspecific similarity in the manner that individualists and the MICCI require.

3. DEVELOPMENTAL HOMEOSTASIS

Developmental homeostasis and natural selection are the least frequently appealed to of the three causes of conspecific similarity. Developmental homeostasis refers, as Eldredge and Gould (1972, 114) put it, to species "as homeostatic systems—as amazingly well buffered to resist change and maintain stability in the face of disturbing influences." Ereshefsky (2001, 115) draws from sources such as Lerner (1954; the original source of the term "genetic homeostasis"), Waddington (1957) and Mayr (1970, 181-182, 300), when he elaborates upon developmental homeostasis as follows.

> Within a species there is a constant influx of new genetic material. This influx can destabilize the well-adapted phenotypes of a species. Given the influx of potentially destabilizing genetic material, selection occurs for genotypes that produce favored phenotypes despite the reconstruction of a species' genotypes. Furthermore, there is selection for genotypes that produce phenotypes that do well in changing environments. Genotypes that continue to produce well adapted phenotypes, despite the reconstruction of genotypes and variation in the environment, are homeostatic. Due to their homeostatic nature, such genotypes work to preserve species unity.

Ereshefsky casts his summary restrictively in terms of homeostatic *genotypes*. Certain genotypes seem simply to have a resiliency to genome reconstruction; and certain genotypes also tend to produce resilient phenotypes that persist through environmental change.

Though instructive, I think Ereshefsky's is a poor summary of developmental homeostasis because it privileges a *gene-centred view* of development—a view that authors such as Griffiths (2001) and R. A. Wilson (2005, ch.6-7) have criticized for leaving out important developmental details. One basis for such critique is *developmental systems theory* (DST), which argues that tightly integrated systems of developmental resources extend beyond the genome and even beyond cells and organisms more generally.

For example, there is a microorganism that forms a symbiotic relationship with some castes of the aphid *Colophina arma*, such that the aphid's life cycle requires a growth spurt that depends on a chemical that only the microorganism (symbiont) can produce. The microorganism can only survive in the aphid, and the aphid *will not develop* without the microorganism and the chemicals it produces (Morgan and Baumann 1994). As such, the microorganism is a non-genetic developmental resource for the aphid.

Extra-organismic developmental resources, meanwhile, might include various features of a population's habitat that, over generations, have come to play reliable roles in the developmental cycle of the organisms that are replicated within the population. But whether a developmental resource is organismic or extra-organismic, the point is that non-genetic developmental resources also seem able to buffer organisms against evolutionary pressures in a homeostatic manner similar to homeostatic genotypes.

No species concepts appeal *only* to developmental homeostatic causes of conspecific similarity in either a derivative or explicit manner. But pluralistic concepts such as the CSC, EvSC and a number of PSCs do appeal in part to developmental homeostasis, depending upon the species (Horvath 1997, 660-661). When they do, it seems clear that species cohesion will *not* satisfy the MICCI, as individualists require. For species cohesion to correspond to integrative cohesion, integrative causal interaction between conspecifics must cause conspecific similarity. But in the case of developmental homeostasis the operative interactions are primarily of an aggregative sort and are not between conspecifics in any way that would integrate them.

For example, when homeostatic genotypes cause conspecific similarity, the causal interactions that activate the collective state of conspecific similarity that realizes species cohesion are actually *within* each conspecific, *not* between each. In one of his earliest papers (1988, 430) and without an analysis of cohesion to aid him, Ereshefsky briefly raises just this point when he claims that individualists who think homeostatic genotypes can "make species individuals" are mistaken because "the uniformity caused by such genotypes is merely the additive result of those genotypes working independently and not the result of any interactive process between the isolated subpopulations of species." This conclusion holds for those homeostatic developmental resources that are outside the genome but nonetheless *within* organisms. Another way to highlight the conclusion is to say that, as causes of conspecific similarity, homeostatic genotypes ensure species cohesion is an aggregative property, which in the previous chapter we noted would be a sure sign that the cohesion in question does not satisfy the MICCI. How about homeostatic developmental resources that lay *beyond* the organism within tightly integrated but *wide* developmental systems?

These developmental resources will not help the individualist either. When these resources offer conspecifics a homeostatic buffer that causes conspecific similarity, the relevant causal interactions will typically be between conspecifics and the putative developmental resources, e.g., between conspecifics and the greater habitats within which they are reliably replicated and which they come to depend upon for their particular developmental life cycles. This implies that species cohesion will have a wide realization—another sure sign that the MICCI is not satisfied. If relevant causal interactions and entities extend beyond the bounds of the species (where we have said a species' bounds are constituted by the conspecifics and interactions between them), species cohesion has a wide realization and does not correspond to integrative cohesion. More specifically, in the case of extra-organismic homeostatic developmental resources, the causal interactions that activate the collective state of conspecific similarity are activating the state of a *wide system* that realizes species cohesion; indeed, DST theorists have articulated their views precisely in terms of *wide developmental systems*; yet, integrative cohesion is realized only by the state of systems within the bounds of the cohesive entity, since integrative causal interactions between conspecifics are entity-bound. Furthermore, if in the case of extra-organismic homeostatic developmental resources the relevant causal interactions are between the conspecifics and the resources, then, again, the aggregative property sure sign will surface and we see that the MICCI is not satisfied.

An individualist wise to the ways of DST may point out that sometimes the relevant extra-organismic resources are actually other conspecifics, in which cases integrative causal interactions between conspecifics may in part be responsible for conspecific similarity. For example, down through the generations, mothers may cosset their young to the extent that the developmental cycles include a dependency on these interactions, ensuring that mothers are developmental resources that may even be homeostatic developmental resources that help cause conspecific similarity. However, here we must keep in mind the integrative-interactions-are-no-guarantee-of-integrative-cohesion point from chapter three. Within the golf example there, we saw that components of an entity can seem to interact causally in an integrative, say, functional way, and yet unless all the components of the entity are similarly integrated, the entity will not demonstrate integrative cohesion. This would be the case when only certain conspecifics act as homeostatic developmental resources for developing young across generations: yes, certain interactions between conspecifics may help cause conspecific similarity, but these interactions are far too limited to hold species together in the way individuals are held together.

4. NATURAL SELECTION

Let us move on, then, to the second process that can cause conspecific similarity: natural selection. This cause of conspecific similarity is, I think, remarkably under-appreciated within evolutionary theory, perhaps because of the attention that the processes of interbreeding steal, or because selection is more commonly thought of as a cause of change rather than of similarity. The causal power of selection will be a central theme in the next chapter, but for now, we need only note that a few authors and species concepts do appeal to selection as a cause of conspecific similarity and that this does not help the individualist.

Ehrlich and Raven (1969, 1231) claim that conspecific similarity can be caused by subpopulations being exposed to similar selection regimes. Typically, fans of this view of conspecific similarity note two ways in which this causal process can work. First, stabilizing selection can wipe out new traits as they emerge, which can forestall population divergence. Second and conversely, selection "may favor....new variants, fostering their spread throughout the species." (Horvath 1997, 661; and see Ereshefsky 2001, 115; Ruse 1987) Notice that each of these two accounts of selection causing conspecific similarity cash out that similarity, as I did in section 1, in terms of shared traits that need not be morphological traits.

As in the case of developmental homeostasis, pluralistic species concepts such as the CSC, EvSC and a number of PSCs appeal to selection as a cause of conspecific similarity (Horvath 1997, 662). However, Horvath (1997, 660) rightly notes that putative ecological causes of conspecific similarity "such as niche availability and resource allocation probably fit best in this category as well." Ecological causes fit best within the category of selection-based conspecific similarity because ecological factors cause conspecific similarity insofar as they are parts of selection regimes. As a result, aside from the pluralistic concepts just mentioned, ecologically based species concepts such as Van Valen's EcSC seem to appeal *wholly* to selection as a cause of conspecific similarity. For example, when Van Valen (1976, 663) defines a species as "a lineage...which occupies an adaptive zone minimally different from that of any other lineage in its range," the notion of an adaptive zone is appealing to a *niche*, and Ereshefsky (2001, 88) correctly notes that on this concept it is "the selection forces of that niche [that] preserve the species as a distinct taxon."

Of course, we must keep in mind here that the notion of a shared niche and thus set of selection pressures is not a sufficient definition of species; members of *different* species can share a set of selection pressures, for example. Concepts like Van Valen's must and do appeal to genealogical grouping criteria in addition to ecological ranking criteria. In the definition above for example, mention of "a lineage" implies that populations must, in addition to sharing an "adaptive zone," be of the same monophyletic, evolutionary lineage if they are to be of the same species.

For individualists, the problems with selection as a cause of conspecific similarity are similar to those they confront with developmental homeostasis. When selection is a cause of conspecific similarity, species cohesion turns out to be an aggregative property with a wide realization. If species cohesion corresponds to responsive cohesion as we saw it seems to, then that responsive cohesion must be coinstantiated with integrative cohesion such that species cohesion owes to and so also ambiguously corresponds with integrative cohesion. For this to be the case, the causal interactions that activate the collective state of conspecific similarity that realizes species cohesion must be integrative causal interactions between conspecifics. But when selection regime pressures are not only evolutionary pressures against which species remain cohesive, but are also pressures that are *responsible* for that species cohesion because they cause conspecific similarity, then the operative causal interactions are between conspecifics and their *greater environments* (Ereshefsky 2001, 87). As a result, the operative interactions are again of an aggregative sort and are not integrative causal interactions between conspecifics. Or, as Ereshefsky (1988, 431) puts it while also appreciating that operative interactions "work independently on the organisms or subpopulations of a species."

To see this more clearly, let me characterize selection regimes in terms of systems, similar to the manner in which some developmental biologists talk of developmental systems. Doing this in some detail will be of use to us in the next chapter and will help to sharpen the notion of natural selection more generally—a welcome consequence since in the literature authors confusingly refer to selection as a number of things, ranging from "force" to "metaphor" (Skipper and Millstein 2005).

Thinking in terms of *selection regimes systems* is to think of natural selection in terms of processes that occur within causally integrated, objectively demarcated systems that contain entities involved in the selection process, along with the properties, relations, and states of those entities and the interactions between them. More formally,

Selection regime system: a causally integrated network of entities that also includes the properties, relations and states of those entities, the interactions between the entities, and wherein objectively demarcated selection processes occur.

My construal of selection regime systems not only has the advantage of thinking of natural selection in terms of processes as biologists generally do (Futuyma 1998, 26), but by implying that these processes occur within defined spatiotemporal regions, we can help to make intelligible ideas that are currently gaining in popularity. These ideas include, for example, the seeming facts that spatiotemporal factors such as ecological and population structures are relevant to population changes. Let me explain.

Until recent reexamination of the idea that speciation almost always occurs when populations become geographically isolated such that they face distinct selection

Until recent reexamination of the idea that speciation almost always occurs when populations become geographically isolated such that they face distinct selection regimes, authors often overlooked the significance of the structure of a single geographically continuous population. Authors also overlooked the fact that ecological structures involving populations and the entities/processes with which its members interact are often significant to evolutionary processes. Now that models of divergent selection among sympatric (i.e. coextensive) populations are more robust and credible, researchers rightly deem such structures to be important, since variation in those structures across space can help explain population divergence (Baker 2005, 320). My appeal to selection regime systems provides a natural way of including such structure within defined spaces where particular selection processes occur. However, as a system, the selection regime system is more than just a spatiotemporal region. It is a causally integrated network of entities, their properties, relations and states, and interactions between them.

Allusion to selection regime systems also ensures that selection processes occur within spatiotemporal locations within which the three Darwinian conditions of selection—that we covered in chapter one—can obtain. On my conception of natural selection, then, the relevant entities in a given selection regime system wherein a causal selection process is occurring may be a population of goldenrod gall flies, the plants they inhabit, the wasps that parasitize their galls, and the ecological and population structures these entities together instantiate (recall the gall fly example from chapter one). The most relevant states within this system may be the *phenotypic variation* (condition 1) in the fly population, the corresponding *differential fitness* (condition 2) that arises from the interaction between the flies and wasps, and the *heritable nature* (condition 3) of the relevant fly traits. The selection regime system is thus the causally integrated network of flies, wasps and plants that interact, stand in certain relations, and bear the relevant states and properties. Over time, these properties, states, relations and interactions constitute a process that produces change in the fly population, or produces conspecific similarity, whereby we describe these causal processes as natural selection. As always, we explain "selection for" traits in terms of the fitness of the traits and the individuals bearing those traits, but the cause of population changes and of conspecific similarity are processes that constitute natural selection.

Now appreciating the notion of a selection regime system, we can pinpoint the problem that arises for individualists when selection causes conspecific similarity. In such cases, causal interactions within the wide selection regime system activate the collective state of conspecific similarity in the species, and this state in turn realizes species cohesion. Therefore, there is a sense in which species cohesion has a wide realization, not an intrinsic one, and so seems not to correspond to integrative cohesion, as the individualist requires. The kind of intrinsic causal interactions that hold paradigm individuals together do not hold species together.

It will pay to also couch this conclusion within the context of the coinstantiation of the two cohesions. I explained in section 1 how observed responsive cohesion of a species must be indicative of integrative cohesion if species are to satisfy the MICCI. But in the case of selection-based conspecific similarity, when interactions within wide selection regime systems activate collective conspecific similarity, which in turn forms the core realization for species cohesion, we do not appeal to operative interactions that are species-bound. The interactions are wide. If the species' responsive cohesion were to be indicative of integrative cohesion, its core realization must be activated by interactions between conspecifics because only this will leave open the possibility that there are operative entity-bound interactions that would be suggestive of species having integrative cohesion with an entity-bound total realization. When operative interactions stretch beyond the bounds of the species, there is no basis for thinking there could be the entity-bound total realization that integrative cohesion and the MICCI require. Put another way, when selection causes conspecific similarity what keeps conspecifics together is not integrative causal interactions between them, but instead aggregative interactions occurring independently between each of them and the environment, and these are not the interactions that characterize individuality.

As a final note before moving on from selection as a cause of conspecific similarity, notice that one of our points about the complex details of cohesion from the last chapter gains traction here. The point was that responsive cohesion can have a componential feature and that this does not require integrative cohesion. Now, if selection causes conspecifics to share traits, these may be group-level traits that allow *groups* of conspecifics to share fates. But when selection "selects for" certain group level traits in a way that causes groups of conspecifics to be similar, such that "collective group conspecific similarity" realizes species cohesion, there need not be integrative causal interactions between similar groups or even between members within those groups, as the individualist requires.

Take dam building in beavers, for example. Rather than saying that conspecific beavers tend to share fates, in the case of dam building it may be more accurate to say that families of beavers share fates, because it seems selection may "see" dam building as a group-level trait (Sterelny 1996, 566). Some dams are better than others, are built in a componential way by all members of the family, and may thus advantage the group, not just individuals. Selecting for a certain dam style that is a group-level trait may cause "group conspecific similarity." But there need not be even a chain of similar integrative causal interactions between one such group and another (say, a chain of interactions that follows a river system), nor need there even be interactions between the beavers within a family. Family members may contribute to dam building via their different, independent contributions that nonetheless bring about the dam in a componential way, so that certain dam types are selected for and, in turn, conspecific group similarity obtains over generations. This is quite similar to the tweaked golf example in the previous chapter, where Andy and Mandy contributed to a combined score that helped realize responsive cohesion, even though Andy and Mandy contributed in different ways and without interacting. Componential instances of responsive cohesion in species may be frequent and yet are not indicative of individuality unless we find, between all conspecifics, the requisite interactions causing conspecific similarity.

Up to this point we have seen that two of the three processes that cause conspecific similarity will not imply that species cohesion satisfies the MICCI. With only interbreeding processes left to consider, we can also note how my taxonomy of species concepts seems to include all leading species concepts and thus ensures there are not any we have missed that might help the individualist. For instance, aside from the species concepts we are about to discuss, there is the CSC, EvSC, EcSC and process-based PCSs, and I have shown how all of these can be thought to correspond to either developmental homeostasis process-based views, or natural selection process-based views, neither of which indicate that species are individuals. Species concepts based on interbreeding and reproductive processes remain the individualists' only hope.

5. INTERBREEDING AND THE BSC

The two best-known species concepts that propagate the idea that interbreeding causes conspecific similarity are Paterson's RSC and Mayr's BSC. These two species concepts are distinct in technical ways, but we can subsume the RSC under the more familiar BSC for our purposes, since "[f]or both [the BSC and RSC], a species is a group of interbreeding organisms, and the organisms of a species cannot successfully interbreed with organisms of other species." (Ereshefsky 2001, 85) In short, both species concepts posit that interbreeding enables gene flow and causes conspecific similarity. Call this the *interbreeding view of conspecific similarity.*

Unfortunately, because this view of conspecific similarity is so closely woven with the BSC, and because I think conceptual confusions hamper the BSC, confusions also surround what it means for interbreeding to cause conspecific similarity. Individualists do think there is a tight conceptual connection between the species as individuals view, on the one hand, and the interbreeding view of conspecific similarity, on the other (e.g. Ereshefsky 1988, 97; 2001, 112-119; Hull 1976; 1978; Ghiselin 1974; 1997; Holsinger 1984; Horvath 1997; Mishler and Donoghue 1982; Mishler and Brandon 1987; Crane 2004). However, to clarify this connection and the interbreeding view of conspecific similarity more generally, it will pay to first consider closely the BSC and its confusions. Doing so will inspire revisions of the BSC and thus revisions of the interbreeding view of conspecific similarity that help the individualists' case.

As mentioned above, the most current articulation of the BSC says, "species [are] groups of interbreeding natural populations that are reproductively isolated from other such groups" (Mayr 1999, 17). The history of this species concept is complex and dynamic. Historians think that at least some biologists employed something like the BSC well before Darwin, going back perhaps to Buffon (Stamos 1998; also see Mayr 1982, 270). I have followed convention and called the BSC Mayr's species concept, but it is well known that Mayr's peers explicitly articulated versions of the BSC before Mayr. Through his impressive career, Mayr was simply the BSC's most vocal champion.

Even Mayr's articulations of the BSC were fluid though. In the above version, the two central concepts are *interbreeding* and *reproductive isolation*, but some of Mayr's previous versions also suggested that species could be groups of "potentially" interbreeding organisms, and that a species' niche helps to define it. Recently, Mayr explained why he thought it wise to drop these elements from the BSC (1999). But to even understand the concepts of interbreeding and reproductive isolation, and how they relate to conspecific similarity and the individuality thesis, we must appreciate why those two concepts came to have importance.

The primary source of motivation and argument for the BSC is the modern synthesis, i.e., the conjoining of Mendelian genetics and Darwinian evolutionary theory that occurred through the 1930s and 1940s via the mathematical tools of population genetics (Futuyma 1998, 24). A key notion within the three principles of Darwinian evolution is, as we have seen, inheritance. In order for populations to "change," such as when an advantageous saliva type in goldenrod gall flies becomes more prominent within successive generations, traits must be heritable. Darwin, however, was unsure how inheritance worked. In 1900, Mendel's 34-year-old paper on plant hybridization was "discovered" and shed light on the "hereditary elements" within living forms. These elements were christened as "genes" in 1909, though the new field of "genetics" was already growing and through the early part of the 20th century, population geneticists were studying the genetic systems of inheritance that seemed to make Darwinian evolution possible. This gave rise to the following line of reasoning.

On the standard view of evolution, traits (whether genotypic or phenotypic) are what vary in fitness, such that their bearers (typically, organisms) have variable fitness with respect to evolutionary processes (Walsh et al. 2002). Trait fitness and organism fitness are the currency of evolutionary change—the things that have evolutionary fates. The genetic revolution of the modern synthesis attempted to show that genes were the locus of traits. Thus, without *interbreeding* within a population, there is no sharing of traits, and without the sharing of traits, there is no sharing of fates. Put another way, the rise of genetic theory allowed us to think of the all-important trait sharing in terms of *gene flow*. So, without the sharing of traits in the form of gene flow that interbreeding enables, there is no sharing of fates, and without the sharing of fates there is no species cohesion, for species cohesion just is the tendency for conspecifics to be similar so as to share an evolutionary fate. Therefore, as *the* prime method of trait sharing or gene flow, it seems interbreeding is the primary cause of conspecific similarity. In our terms, interbreeding interactions intrinsic to species are primarily responsible for activating the states that realize species cohesion.

This line of reasoning helps explain how the *interbreeding* view of conspecific similarity gained its prominence during the modern synthesis. But what about *reproductive isolation*? Authors often consider reproductive isolation to be just the flip side of one conceptual coin, where the other side is interbreeding.

To see this, consider that the modern synthesis posited that if interbreeding ensures conspecifics share evolutionary fates and are thus "kept together," then just the absence of interbreeding explains the differentiation of evolutionary fates. Thus, as Mayr (1970, 13) puts it, "the basic biological meaning of a species" is that "[a] species is a protected gene pool." For a species to be a protected or *closed gene pool* is just for it to be, at one and the same time, a population in which interbreeding enables gene flow, and a population that is reproductively isolated from other gene pools.

It has thus seemed to some that by defining species as interbreeding populations that are reproductively isolated from other such groups, the BSC contains a redundancy. To be an interbreeding population just is to be a reproductively isolated one. More recently though, others (e.g. Meier and Willmann 1999, 31; Mayr 1970, 13) have at least noted that unlike interbreeding, reproductive isolation makes species "a relational term." (13) For a population to be an interbreeding population it need *not* stand in any particular relation to another population; for a population to be reproductively isolated though, it *must* stand in a certain (i.e. isolated) relationship with at least one other population. Interbreeding and reproductive isolation are still thought of as "virtual" flip sides of each other, but the recognized minor difference does entail the BSC is not redundant. As Ereshefsky (2001, 82) summarizes, "[o]n the biological species concept, species are genetic fortresses, protected by isolating mechanisms *and* held together by interbreeding." (my emphasis) In short, the BSC contains an internal and an external component.

Given this, it seems that *with respect to conspecific similarity* it is indeed the concept of interbreeding that is salient, not reproductive isolation. The concept of reproductive isolation and the "isolating mechanisms" to which it corresponds may capture reasons why conspecifics of one species travel upon an evolutionary trajectory that is distinct from the trajectories of conspecifics of other species. However, it is the interbreeding interactions "internal" to species and between conspecifics that causes conspecifics to be similar.

We are beginning to get a sense, then, of the prima facie support that the interbreeding view of conspecific similarity offers to the species as individuals view. The developmental homeostasis and natural selection views of conspecific similarity seemed like non-starters for the individualist because they did not even place the causal interactions involved in species cohesion between conspecifics, as individuality requires. The interbreeding view at least gives one the sense that interactions between conspecifics may hold conspecifics together along an evolutionary trajectory such that the conspecifics are integrated into a sort of functional unit that resists or succumbs to selection pressures as a whole, perhaps even as an individual. On the interbreeding view that the BSC enshrines, it seems the responsive cohesion of species may be indicative of their integrative cohesion.

Before chalking out this tight conceptual link between the interbreeding view and the individuality thesis more formally though, consider the confusions surrounding the BSC that emerge from our above consideration of it.

The BSC underscores interbreeding as the cause of conspecific similarity, but within the modern synthesis it seems interbreeding came to matter just because it enables gene flow, i.e. enables the sharing of traits and so the holding together of species. And interbreeding and gene flow are distinct concepts. Authors debating species ontology often overlook the difference between these concepts and this, I think, owes to confusions surrounding both concepts. Let us consider the confusions that surround gene flow and then how it is distinct from interbreeding, before looking at the confusions that surround interbreeding.

Numerous authors borrow the term "gene flow" form current population genetics without realizing that within population genetics the term has come to be a technical one with limitations. Technically, gene flow refers to "the movement of genes from *one population* into the gene pool of *another*." (Futuyma 1998, 315; my emphasis) As such, population geneticists construct elaborate "models of gene flow" and measure, among other things, the rates at which genes "flow" from one distinct population to another. Yet, when used by systematics and philosophers of biology within the debate over species ontology, gene flow is envisaged as the movement of single genes from one organism to the next, across generations and down through them, *within* populations. Perhaps we can admit this loosening of the meaning of gene flow, since it is how the term got its start in the modern synthesis, but if we do, we must recognize the limitations of even the loosened concept.

For example, genes do not flow. They are replicated. Granted, they are replicated reliably, and so there is a metaphorical sense in which they flow, but gene flow is nonetheless a metaphor. Appreciating this also signals the fact that "gene pool," too, is a metaphor. Hull (1976, 175) pointed this out, though without further developing the implications of his thought, when he said, "[s]pace-time can no more exist in the absence of material bodies than a person can take a swim in a gene pool." The failure to appreciate the metaphorical nature of gene flow and gene pools seems to have misled many into thinking the flow of genes occurs within some physically continuous space, which it does not, and that the flow of genes is more permeating than it is. For example, population geneticists know that panmixia (random mating among all members of a population) is a seldom-approximated ideal; there are a great many genes that do not "flow" within sexually reproducing populations simply because a minority of males account for a majority of the male contribution to gene distribution.

Even if we are careful with our loosened conception of gene flow though, it is important to realize *how* it is distinct from interbreeding and to ask *why* it is not explicitly enshrined in the BSC. The two concepts must be distinct, since asexual organisms cannot interbreed and yet there are some forms of gene flow within asexual species and clone lines. Horizontal gene transfer and genetic exchange via bacteriophages and plasmids are examples (Nanney 1999, 110). These "methods" of gene flow are certainly distinct from interbreeding, and so are the results. As Nanney (110) says, "[c]onjugation—the mechanism most similar to synkaryon formation in eukaryotes [i.e. recombination in sexually reproducing populations]—only rarely involves whole genomes." Nonetheless, if we have agreed to loosen our notion of gene flow so that it explicitly breaks from the usage of that term in current population genetics, and if our loosened notion also is sensitive to the fact that gene flow is limited within sexually reproducing species unlike the ideals of panmixis suggest, then it is not clear why the notion of gene flow is *a priori* inapplicable to asexual species.

For the sake of argument, though, let us allow that gene flow within sexually reproducing species is sufficiently distinct from that within asexual species to grant that there is something special about gene flow among interbreeding organisms. If we do, it still seems that interbreeding is distinct from gene flow. Interbreeding is a process involving two or more organisms and gene flow is a set of causal interactions resulting from this process. This realization might lead us to question why gene flow is not explicitly mentioned in the BSC. Being charitable, we might offer the answer that interbreeding is merely the most practical and concrete means of capturing what everyone realizes is a slippery metaphor (i.e. gene flow). Even so, there is a real question as to whether the BSC should countenance interbreeding interactions as the cause of conspecific similarity, or whether it should instead countenance gene flow as the more salient cause that interbreeding merely enables.

These points betray the need to bring philosophical clarification to interbreeding. For instance, a moment ago I said that interbreeding is a process involving two or more organisms. This would make interbreeding a property of, say, n-tuples of organisms. However, by saying that "species are interbreeding populations..." the BSC implies that interbreeding is a property of populations. This seems false. Populations do not interbreed, unless we go back to the technical sense of gene flow that attempts to capture the importance of genetic exchange between distinct *populations*. But this still does not seem right, for the BSC is attempting to define *species*, not populations. Interbreeding is not a property of species; species do not interbreed.

Saying that interbreeding is a property of pairs or n-tuples of organisms helps sharpen the suspicions of those authors who point out that interbreeding and reproductive isolation are not merely (or virtually) conceptual flip sides of each other. Unlike interbreeding, reproductive isolation can clearly be a group-level property. It may be a property of pairs of organisms, e.g., if a population contains only two conspecifics. But it can be a property of whole populations in a way interbreeding cannot. Moreover, it is strained to conceive of individual sexually reproducing organisms as having the property "reproductively isolated," since the very possibility of reproduction as such minimally requires two organisms, thus making it unintelligible to say that one organism can or cannot have the property. With interbreeding and reproductive isolation being distinct kinds of properties, they are separable in nature and not dependent upon each other as proponents of the BSC suggest when they imply that reproductive isolation is little more than the absence of interbreeding. We need not envision such a strict *necessary* conceptual connection. Populations can be reproductively isolated from others regardless of whether interbreeding is the preferred method of reproduction.

If we reflect upon the notion of reproductive isolation within the context of my taxonomy of species concepts, it becomes clear that the BSC as traditionally articulated is actually both a process-based view and a pattern-based view, and that the standard way of conceiving of the BSC strictly in terms of processes actually straightjackets the BSC. Interbreeding is clearly a process and serves as the BSC's primary ranking criterion. It is standard to view reproductive isolation *also* as a ranking criterion that is based on processes constituted by pre- and post-zygotic isolating mechanisms. It is also standard (outside of discussions of speciation) for the reproductive isolation component, when construed in terms of processes, to be pushed to the background in favour of a focus on interbreeding (Meier and Willmann 1999, 30).

However, as a relational property of populations, we can also view reproductive isolation in terms of patterns, not just processes that are less significant than or derivative of interbreeding. Viewing each species against the backdrop of other species, as the relational nature of reproductive isolation insists we do, allows us to see species as forming a pattern of reproductively isolated units that extends out from a common point of origin where and when life (even if only sexually reproducing life) began. Combining this pattern-based interpretation of reproductive isolation with the fact that reproductive isolation is a separable property from interbreeding, it is thus possible for us to view interbreeding as just one sort of process that has led to a particular pattern of reproductively isolated species. Singling out reproductive isolation in this way underscores a broader role for reproductive isolation that is often overlooked in favour of the focus on interbreeding, and makes the BSC a more versatile species concept with clear criteria for grouping (i.e. species are grouped into monophyletic reproductively isolated units) and ranking (i.e. populations are ranked as species when their members interbreed, or perhaps when there is gene flow between members).

Taking stock of the confusions that surround the BSC, we can make three general points that do not so much criticize the BSC as suggest ways of revising it.

First, the BSC seems to trade on three distinct, key concepts, not just two. Interbreeding is important *because* it results in gene flow, which is a distinct concept. The notion of gene flow is complex and we must handle it with care, but *given the proclivities of BSC supporters*, something like it seems to deserve a more explicit place within the BSC (the next chapter shall betray that I do not share these proclivities, nor a preference for even a BSC refocused on gene flow).

Second, and related to the first point, there is reason to suspect that the BSC unwittingly implies gene flow is a direct cause of conspecific similarity, rather than, as its most famous articulations suggest, interbreeding.

Third, reproductive isolation is conceptually distinct from both interbreeding and gene flow and we could exploit this as a way of making the BSC a more versatile species concept that appeals to both patterns and processes.

Following our critique of the species as individuals view in the next chapter, I shall come back to these suggested revisions in chapter six, since our critique will afford further recommendations for a more integrative BSC that does not rely upon either interbreeding or gene flow. For now, however, these suggested revisions are only meant to make the BSC as plausible a resource as possible for individualists, in a way that shows individualists how they should sharpen their view of the tight conceptual connection between the species as individuals view and the interbreeding view of conspecific similarity. Specifically, the first and second suggested revisions are most relevant here and suggest that the tight conceptual connection individualists see between the species as individuals view and the interbreeding view of conspecific similarity is more likely a connection between the species as individuals view and the *gene flow view of conspecific similarity.*

Many authors within the species ontology debate already do speak as though gene flow is the important thing. Hence, John Dupre (1993, 46): "if species are considered as...individuals, gene flow is what quite literally holds the parts of the individuals together." But it seems generally *un*appreciated in the species ontology literature that this claim is *not* straightforwardly interchangeable with the claim that interbreeding holds species together or causes conspecific similarity. Interbreeding and gene flow are taken to be synonyms; yet we have seen there is reason to think this is a mistake; and so the gene flow view of conspecific similarity makes a different and more plausible claim than the interbreeding view.

What the gene flow view says more specifically about the cause of conspecific similarity is that genetic exchange can bring about the sharing of evolutionary fates that holds species together, in one or both of two ways: "by spreading adaptive genes" or "by damping the occurrence of change in local populations." (Ereshefsky 2001, 114; and see Mayr 1970, 297-301, 168-169). It is these causal powers to which the individualist must refer when plausibly drawing the tight conceptual connection between gene flow view and the species as individuals view. Identifying them rounds out our articulation of "the three" processes that cause conspecific similarity—three processes that exhaust the possible causes of conspecific similarity enshrined in all extant leading species concepts. With the gene flow view of conspecific similarity clarified, let us now turn to spell out more formally the connection between it and the species as individuals view.

6. THE ARGUMENT FROM GENE FLOW

We are now in position to state more precisely, or at least more charitably, why individualists take the gene flow view of conspecific similarity to form a powerful conceptual basis for the individuality thesis. Here is how individualists might summarize the *argument from gene flow.*

> The argument from gene flow attempts to build on the basic idea that gene flow between conspecifics constitutes causal interactions between conspecifics that integrate and hold them together as an evolutionary unit that functions in the processes of evolution. Gene flow holds conspecifics together in the following sense: conspecifics evolve together in virtue of conspecific similarity, and gene flow is what causes conspecific similarity. Held together by gene flow, conspecifics will respond to evolutionary pressures as a unit, such that the species as a whole

demonstrates species cohesion. Therefore, a species' cohesive response to pressures owes to the integrating effects of gene flow. Paradigm individuals seem constituted in large part by the same kind of integrating effects of causal interactions between their parts. And those parts sometimes respond to pressures as a unit just because they are so integrated. In short then, gene flow gives rise to cohesion in species in the same way that integrative causal interactions between parts give rise to the cohesion of paradigm individuals. (for arguments very similar to this one or closely related to it, see Ereshefsky 1988, 97; 2001, 112-119; Hull 1976; 1978; Ghiselin 1974; 1997; Holsinger 1984; Horvath 1997; Mishler and Donoghue 1982; Mishler and Brandon 1987; Crane 2004).

Appreciating the nature of cohesion as we do, we can restate this argument in terms of realization. What gives the argument from gene flow initial plausibility is that it does not appeal to aggregative sorts of causal interactions when attempting to explain species cohesion. Recall that species cohesion seems at least to correspond to responsive cohesion. Therefore, if the species as individuals view is to hold, species cohesion must be doing double duty. The seeming responsive cohesion of species must be of a nature that indicates it is coinstantiated with integrative cohesion. For this to be the case the core realization of a species' responsive cohesion must be activated by integrative causal interactions between its conspecifics; only then do we have reason to think that the causal interactions that individuality requires might obtain in the species. And the argument from gene flow purports to give us just such reasons. Unlike the appeals to developmental homeostasis and natural selection, the appeal to gene flow is the claim that integrative causal interactions between conspecifics activate the core realization of a species' responsive cohesion, which is collective conspecific similarity. Gene flow causes conspecific similarity. Therefore, it seems a species' responsive cohesion may owe to its integrative cohesion.

This betrays that there is at least one crux of the argument from gene flow, and one further important presumption.

The crux, clearly, is the gene flow view of conspecific similarity. If gene flow is the cause of conspecific similarity, then a species' responsive cohesion seems to involve the causal interactions that could support the intrinsic total realization of integrative cohesion. In other words, the appeal to gene flow does not immediately raise the aggregative property sure sign that the MICCI goes unsatisfied.

Furthermore, this crux of the argument from gene flow bears much burden because we have seen from our look at the leading species concepts that if gene flow is not the cause of conspecific similarity then no other promising options await individualists. The other options do raise the aggregative property sure sign, and the wide realization sure sign as well, and they generally give us no reason to think species have the cohesion of individuals. Individualists who advance something like the argument from gene flow seem to appreciate the burden the gene flow view of conspecific similarity consequently bears. The idea, in short, is that "gene flow, between populations, is a key factor in keeping the *organisms* of a species *alike*" (Ruse 1987, 352; my emphasis), and from this is drawn the conclusion that "[s]pecies consisting of populations that are bound by gene flow satisfy the strong notion of individuality." (Ereshefsky 2001, 116)

Now, the one important presumption. If the individualist were to agree with how I have analyzed the kinds of causal interactions that suffice for integrative cohesion, they would also admit that an important presumption of the argument from gene flow is that gene flow processes consist in causal interactions that integrate conspecifics in a *structural* and/or *functional* sense(s). In fact, individualists do implicitly advance arguments for at least the function portion of such a presumption. For example, Mayr (1970, 12) claims that gene flow allows any given conspecific to "function" as "a temporary vessel holding a small portion of the contents of the gene pool for a short period of time." By functioning as such, the conspecific "serves" the species by later being able distribute its advantageous traits, or instead by distributing traits that dilute the potential dangerous effects of traits flowing into the population from without (e.g. when genetic variants immigrate to a new population in which the spread of their genes may be deleterious). In either case, the argument goes, gene flow interactions ensure conspecifics serve functions and that the species as a whole is a sort of functional unit. This helps support the idea that a species' responsive cohesion owes to integrative cohesion. For not only is the core realization of responsive cohesion activated by causal interactions that are between conspecifics, but also those interactions do seem of the minimally integrative sort we have said individuality requires.

To better appreciate how an individualist could adopt our terminology and claims of chapter 3, and to urge that gene flow interactions do indeed qualify as just the right integrative and intrinsic causal interactions for integrative cohesion, we can think in terms of the *systems* whose states are realizing the two kinds of cohesion that the individualists would say are coinstantiated in species.

If the coinstantiation owes to causal interactions manifest in gene flow, then with respect to responsive cohesion, those interactions activate a core realization (i.e.

collective conspecific similarity) that in turn is a part of a wide total realization that stretches beyond the bounds of the species. The total realization is the state of a system that includes the evolutionary pressures beyond the species bounds, to which the conspecifics respond as a unity.

On the other hand, with respect to integrative cohesion, the gene flow interactions activate a total realization that is within the bounds of the species, as the MICCI requires. We might say the total realization is the state of the *species' replicative system*. If a species' replicative system is entirely within the bounds of the species (and here the bounds of species and the notion of a replicative system would need closer attention were the individualist to construe the argument in such terms), this feature of it may ensure the intrinsic realization of the species' integrative cohesion. In short, through appeal to gene flow, there do seem options for spelling out the coinstantiation of the two kinds of cohesion in terms of activating causal interactions, states, systems, and realization, in the way we said we must at the end of chapter three.

In this section I have tried to show how, via the argument from gene flow, we can articulate the tight conceptual connection between the gene flow view of conspecific similarity and the species as individuals view. Furthermore, I have summarized how individualists have sketched something like this argument in their terms and how we can express it in the terms I have introduced. The intuitive idea is that causal interactions between conspecifics hold those conspecifics together just as the parts of cohesive individuals are held together. Now let me briefly mention how the next chapter shall object to this idea and challenge the individuality thesis.

7. TWO OBJECTIONS

One could question the argument from gene flow from numerous angles. One option would be to grant that gene flow interactions form the basis for species' satisfaction of the MICCI, but upon pointing out that the MICCI is only a minimal criterion, one could then attempt to flesh out a more complete account of the cohesion of individuals that species clearly do not satisfy even in virtue of gene flow. I am sympathetic to this option but I have hinted that it is beyond our grasp here.

A second option would be to press individualists on what I have called their *important presumption*. Does gene flow ensure conspecifics are integrated in the required functional or structural sense(s)? This is also, I think, a promising way to go, but it clearly leaves room for a particular individualist reply. Part of the strategy of chapter three was to suggest that individualists actually agree with something like the MICCI. After establishing this, I offered my own analyses of the ideas contained in the MICCI. So, upon challenging the argument from gene flow through appeal to the notions of structural or functional integration, the individualist could take issue with the analysis that raised those notions as crucial. Perhaps those notions need further fleshing out, or set the bar of individuality too high. I doubt the bar is set too high, but in any case, there is a more straightforward way to challenge the argument from gene flow.

I shall target the crux of the argument from gene flow. If we can show that gene flow does not cause conspecific similarity, then the argument will not go through. Other accounts of conspecific similarity, we know, will not help the individualist: species cohesion would correspond only to responsive cohesion and species would not be individuals. Moreover, this conclusion would depend in large part upon what my textual evidence suggested is an *un*controversial idea, i.e. Ereshefsky's modest clarification that says the operative causal interactions with respect to individuality are interactions that are between parts.

To argue for this conclusion, I will advance two objections to the gene flow view of conspecific similarity and the argument from gene flow that it anchors. In different ways, each objection casts doubt on the causal powers of gene flow. My underlying suspicion here is that many biologists and philosophers of biology have simply misappropriated the causal powers responsible for conspecific similarity. The gene flow view of conspecific similarity is certainly the most widely accepted view of conspecific similarity, but there is something very puzzling with the idea that groups of organisms are held together over time by the distribution of a certain substance between them. We should find this no less puzzling than if someone were to suggest that persons in a group who shared blood through transfusions were held together over time in virtue of that blood flow. Blood flow does not make a group an individual. Neither does gene flow.

5

The Scope Envy and Causal Poverty of Gene Flow

Our suspicion is that, eventually, we will find that, in some species, gene flow is an important factor in keeping populations of the species relatively undifferentiated, but that in most it is not.

Selection itself is the primary cohesive and disruptive force in evolution; the selective regime determines what influence gene flow has on observed patterns of differentiation.

Paul Ehrlich and Peter Raven 1969

1. THE SCOPE ENVY OBJECTION

There are two general bases for claiming that gene flow is not a cause, or at least not a significant cause of conspecific similarity. In turn, then, these are two bases for objecting to what seems the individualists' only argument for the conclusion that species demonstrate the cohesion of individuals. One basis is empirical, the other conceptual. In the first half of this chapter I advance the *scope envy objection* to the argument from gene flow, which exploits *both* the empirical and conceptual bases. In the second half of this chapter, I advance the *causal poverty objection,* which primarily exploits the conceptual basis. Let me introduce the first of these objections.

Parties to the species ontology debate certainly envision and exploit a tight connection between the species as individuals view and the BSC. One may thus presume that the species as individuals view gains much of its plausibility from empirical evidence, since the BSC is the most widely entrenched species concept among practicing biologists, and biologists are not likely to employ a species concept that has little empirical support. However, this might misidentify the support the BSC offers the species as individuals view. It is not so much the empirical success of the BSC that supports the species as individuals view, but rather the tight conceptual connection that I have shown to exist between the species as individuals view and the gene flow view of conspecific similarity that seems (or should seem) central to the BSC. The empirical success of the BSC thus acts to reinforce an already impressive conceptual connection.

This does not mean, however, that challenging the empirical adequacy of the BSC and the gene flow view of conspecific similarity it embodies would not challenge the argument from gene flow. If there is strong empirical evidence that the BSC and the gene flow view are wanting, we have reason to doubt that the conceptual connection between these and the species as individuals view is very relevant.

In the first instance, the scope envy argument is an empirically based objection. Despite the BSC being entrenched among empirically minded biologists, I go against the grain and suggest there is overwhelming empirical evidence that the BSC and the gene flow view of conspecific similarity are inadequate views of species. The BSC and gene flow view of conspecific similarity simply do not have the scope of applicability that authors and researchers typically presume. These views simply do not account for the conspecific similarity within a majority of species.

My method for demonstrating the scope envy of the BSC and gene flow view, however, also ensures that the scope envy objection has conceptual force. For after briefly suggesting that dogmatic views of speciation have illegitimately reinforced the BSC and gene flow views (section 2), my tack will be to draw from empirical evidence and show that gene flow is neither necessary (section 3) nor sufficient (section 4) for "holding species together." Demonstrating that a phenomenon is neither necessary nor sufficient for bringing about another phenomenon casts doubt on the conceptual adequacy of the putatively determining phenomenon. The causal poverty objection of the latter half of this chapter (sections 5 through 10) will attempt to extend this conceptual objection to the BSC and gene flow. Throughout the chapter, the causal powers responsible for conspecific similarity shift to selection regime systems—a result that, in the previous chapter, we already began to appreciate is no boon for individualists.

2. GENE FLOW THEN AND NOW

Let me offer some sense of the entrenchment that I have repeatedly said the BSC enjoys. One sure sign of this entrenchment is the fact that even a biologist critical of it may have no choice but to employ it. When I raised concerns about it with Allan Dibb, a wildlife specialist with the Canadian government, he explained that he is a

> biologist who is obliged to work within the definitions of species...as provided for under existing legislation and programs, such as our national Species at Risk program, because that's how the money for research and recovery programs is distributed. In other words, I haven't really given too much thought in the last few years to the questions you raise about species definitions—not because they aren't important questions, but because I have to work within the current definitions (pers. comm. February 14th, 2005)

The current definition he speaks of is Mayr's BSC. But government legislation and wildlife management plans are not the only reflections of entrenchment. Texts are another good example. Introductions to evolutionary biology often at least survey alternatives to the gene flow view and the BSC, but the debates here are technical and controversial, and after the survey the reader is almost always brought back to the reassuring and clear conclusion that "[a]t this time, the biological species concept is more widely used than any alternative definition. Moreover, it plays a key role in evolutionary theory...For these reasons, this book uses the biological species concept." (Futuyma 1998, 453; note: Futuyma's survey of "alternative" views is, I think, especially good for an introductory text).

Being widely entrenched, however, is different from being widely accepted. In the past twenty years, the BSC and the modern synthesis have flagged in many quarters (Nanney 1999). This is especially true within the research that traffics most heavily in the notions of species cohesion and species concepts, such as systematics research and speciation studies. I want to consider the latter of these now because an appreciation of recent trends in speciation studies quickly brings out an unstable source of support for the BSC and the gene flow view of conspecific similarity.

In a thoughtful discussion of the history and philosophy of speciation debates, Jason Baker (2005, 316) notes, "[u]ntil the mid-1980s, the consensus among evolutionary biologists was that speciation was almost always geographic and isolating mechanisms were almost always adaptational by-products." Further, he documents that more recently, non-geographic speciation models (e.g. "clinal speciation" and "ecological speciation"; see 317 and further discussion below) have contributed to the decline of the past consensus. By "past consensus on geographic speciation," Baker refers to allopatric speciation models that argue that populations typically only diverge when they become geographically isolated, i.e. when interbreeding and gene flow are disrupted.

The allopatric models and the gene flow view seem to go together, thanks (in part) to a certain conditional claim: *if* interbreeding and gene flow hold populations together, *then* populations will diverge when there is no interbreeding or gene flow. Allopatric speciation models are attractive just because it seems many populations have diverged when they became isolated and unable to exchange genes. This phenomenon seems to have ensured, for example, that there was an unusually diverse and strange assortment of life for Darwin to observe while visiting the Galapagos. Thus, with the seeming soundness of allopatric speciation models and the putative support they offer the gene flow view of conspecific similarity through a certain conditional claim, the natural inference is that interbreeding and/or gene flow hold species together.

The conditional reasoning is of course unstable. When isolated populations diverge from each other it is not *necessarily* because the isolation prohibits gene flow. Because of this unstable reasoning, the success of allopatric models does not guarantee any causal powers for gene flow. Ehrlich and Raven (1969, 1230) appreciate the limits of the above conditional reasoning when they blatantly contradict it through the evidence they marshal for showing that when one population becomes isolated from a "parent" population and begins to diverge from the parent population, we should see this not as owing to an "interruption in gene flow," but instead see it as "a function of a very different selective regime." We shall consider this evidence in more detail below, but now I simply make the point that, indeed, geographic isolation may often facilitate species evolution, but this does not imply that an interruption in gene flow is the cause of divergence in the populations of a species.

Authors have not only begun to question the link between allopatric speciation and the gene flow view of conspecific similarity, but as Baker suggested in the passages above, allopatric models themselves have also come under scrutiny, and some authors admit that they should have been scrutinized long ago. For example, on the one hand, alternative and competing models once thought to consists in mere conjecture because they posit that populations can diverge *in spite* of widespread gene flow (i.e. models that diminish the causal scope of gene flow), are now gaining converts because recent research actually has made them more plausible. We shall see this below. On the other hand, however, some past models were indeed plausible *in the past* and the illegitimate privileging of gene flow caused readers to miss this. Thus, even the staunchest allies of the allopatric-gene flow connection, such as Ernst Mayr, have admitted that their past criticisms of evidence (such as Guy Bush's 1969) of the inadequacy of gene flow for species cohesion were poorly motivated (Baker 2005, 316-317).

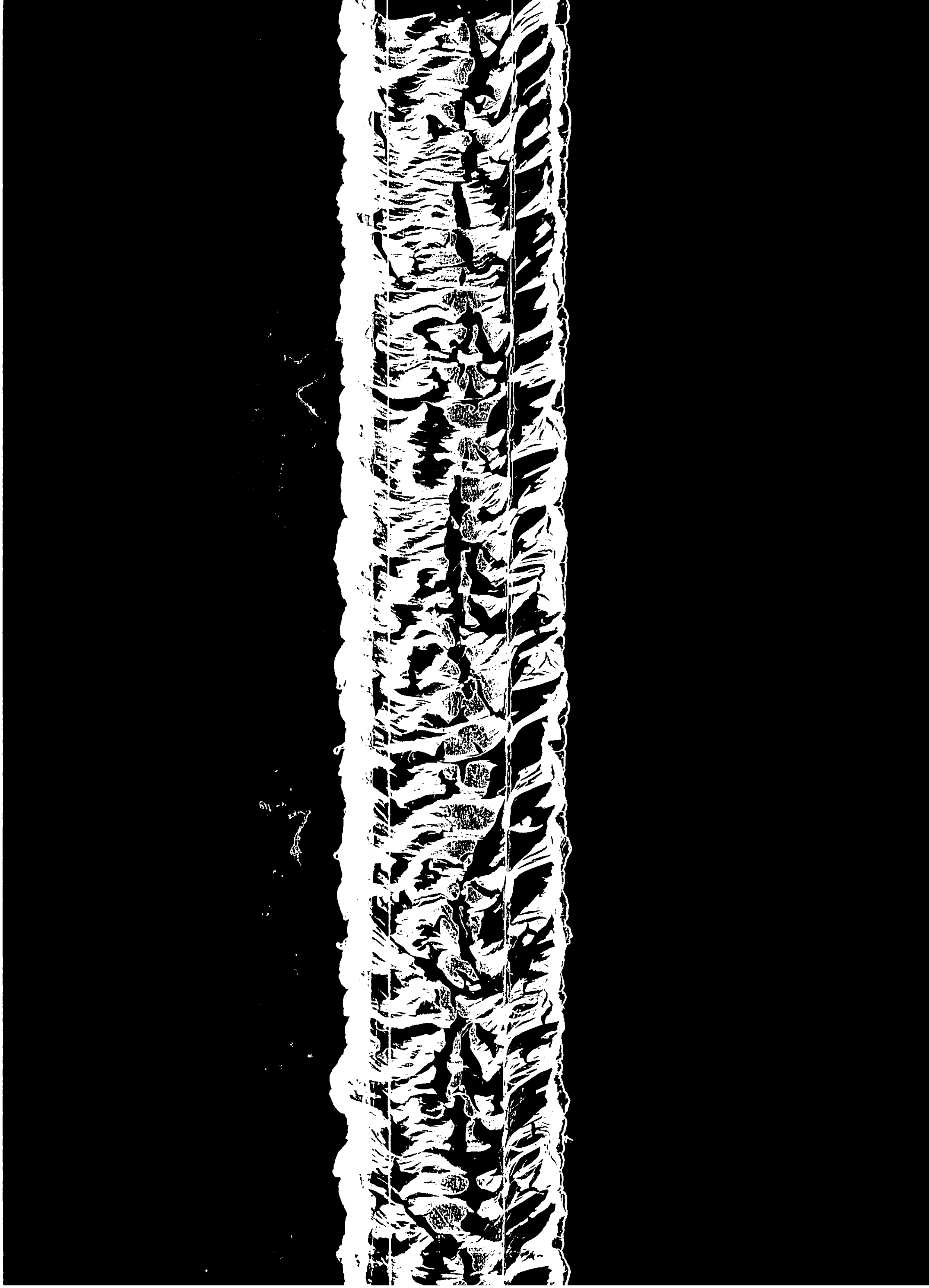
What I hope to have done in this section is acknowledged the entrenchment of the BSC and gene flow views, but to have offered some *prima facie* reasons for thinking this entrenchment had dubious beginnings in the first place. This helps explain why many biologists and philosophers have not appreciated until recently, if at all, the arguments I am about to levy against the BSC and gene flow views while drawing on the empirical evidence of a minority of authors who have considered the BSC and the gene flow view under a critical eye.

3. GENE FLOW NOT NECESSARY

Interbreeding and gene flow are clearly not necessary for conspecific similarity or the maintenance of a population's cohesion because numerous populations and species display evolutionary cohesion without interbreeding or gene flow between conspecifics of populations or between populations of species.

An immediate example here is asexual species, long recognized as the Achilles heel of the BSC. In the previous chapter I mentioned that there are under-appreciated grounds for presuming that asexual species do enjoy gene flow, even if such flow is not nearly as impressive as, or is of a different kinds than, that within sexually reproducing forms. But the consensus within the species ontology literature has been that "species consisting of asexual organisms lack the process of gene flow." (Ereshefsky 2001, 117) So let us presume for the moment that this is true. It is certainly true of interbreeding. Asexual organisms do not interbreed. And if neither gene flow nor interbreeding cause conspecific similarity within asexual forms, there is certainly trouble for the fan of the gene flow view of conspecific similarity. For despite the inability of asexual organisms to interbreed or exchange genes with other members of the species that are not also members of the same clonal lineage, "asexual organisms evolved just as surely as did sexual species." (7) Having evolved implies that they demonstrate species cohesion and to recognize this, taxonomists do, of course, group asexual organisms into species, including bacteria and other prokaryotes (i.e. organisms lacking a membrane bound nucleus). What seems responsible for species cohesion in asexual species is "individuals being kept similar by their continued existence under similar selective regimes" (Ehrlich and Raven 1969, 1230). In this way, shared exposure to selection pressures ensures conspecifics "retain a degree of molecular similarity and hence [represent] a group of presumed 'recent' common origin." (Nanney 1999, 110) As a result, microbiologists especially have had no truck with the gene flow view. While associating that view of conspecific similarity and species cohesion with the modern synthesis as I have suggested we do, ciliate expert David Nanney (1999, 110) comments on those studying prokaryote organisms, as he writes, "[m]icrobiologists have essentially bypassed the Modern Synthesis, considering it irrelevant within their territories. Modern microbiology textbooks abound with Latin binomials, but the 'species' associated with the Linnaean terms make no claims of association with closed gene pools."

To deflate the fact that gene flow and interbreeding are not necessary for the species cohesion of asexual forms, some individualists flatly deny that asexual organisms form species and thus in such cases there is no species cohesion that needs explaining in non-gene flow terms (e.g. Ghiselin 1997, 119). In his (1999, 7), John Dupre



notes that a distinct but no less radical "attempt to save the BSC is suggested by David Hull (1989): in asexual organisms, the species are simply organism lineages—that is, an organism and its descendants (p. 107)."

These radical manoeuvres are not satisfying. As for Hull's manoeuvre, Dupre (1999, 7) points out shortly after introducing it that it would "divorce the identification of species in these cases from any practical utility in classification...such an identification would imply the existence of countless species, many lasting only a few minutes or even seconds." This is bullet biting indeed. On the other hand, rejecting outright the idea that asexual organisms form species would demonstrate a suspicious bias in favour of the sexually reproducing life forms that clearly constitute a minority of the life we know. As Ereshefsky (2001, 117) makes clear, "the occurrence of asexual reproduction is not a biological oddity but the predominant form of reproduction in life on this planet. For the first three quarters of life on Earth, asexuality was the only form of reproduction (Hull 1988, 429)." Evolutionary theory would certainly require adjustment if for the majority of time that evolution has occurred, the entities we take to evolve did not yet exist. But the troubles lie not only in the past. Ereshefsky continues: "Furthermore, if one looks at current biota, most organisms reproduce asexually; most plants and insects, not to mention fungi and microorganisms, reproduce asexually." It thus seems that gene flow is not a factor in conspecific similarity or species cohesion for many living forms. Not only individualists are guilty of overlooking such facts. As Nanney (1999, 11) laments while discussing his colleagues, "[n]either the age of the earth nor the time course of life has permeated fully the thinking of biologists."

Asexual organisms are a relatively obvious, if underappreciated example of the inadequacy of the gene flow view. Within sexually reproducing populations the problems are less visible but similarly deep. *Gene flow is not a necessary condition of conspecific similarity for sexually reproducing life forms either.* The *locus classicus* of this thesis, which parties to debate over species ontology have only just begun to give due attention (e.g. see Ereshefsky 2001) is Ehrlich's and Raven's (1969) paper in *Science* that surveyed evidence of ineffectual or absent gene flow in species ranging across the plant and animal kingdoms. It will pay to reveal some of the variety they draw on, and highlight their major points.

For many animal species, gene flow is not necessary for their cohesion because they consist of populations that seldom if ever exchange genes, but which remain undifferentiated nonetheless. This is even true of aerial species that we often presume are able to overcome geographic separation. Colonies of the butterfly *Euphydryas editha*, for example, are scattered through California with distances of up to 200 km separating them, and studies show that gene flow is nearly zero when the gaps between them are as little as 100 meters (1229). Yet conspecific similarity persists across populations of the species. Thus, "there seems no possibility that gene flow 'holds together' its widely scattered population" (1229). Studies of lizards, newts, and numerous bird species also show that gene flow often is contained within subpopulations that *no* significant geographic barriers seem to divide (1228). Gene flow naturally occurs in pockets, yet conspecific similarity persists.

Even when "travelers" do transgress pocket boundaries, or transgress actual, significant geographic separation, this often does *not* correspond to interbreeding reconnections and gene flow, as studies of *Mus musculus* individuals (mice with tremendous reproductive potential) wandering between granary populations, and studies of emigrating members of *Euphydryas editha* indicate (1229). Moreover, when gene flow does happen to connect populations, it is unclear what causal affects are typical, since often no novel genes are shared, and if they are, their fates are "governed by the same gloomy odds facing mutant genes." (1229)

Compared with the animal kingdom, there have been more studies of the effects of interbreeding and gene flow within plant species. Here again, gene flow seems unnecessary for conspecific similarity and species cohesion.

Typically, either wind or insect pollination facilitates sexual reproduction in plants. One would expect wind to facilitate reproduction over vast distances, "but his assumption is not borne out by the available data." (1229) For example, outcrossing in *Zea mays* and *Beta vulgaris* is only *one percent* at distances greater than 18 m. Likewise among Coulter pines, successful dispersal does not reach far beyond 10 m. Insect pollinated interbreeding fairs no better. In numerous studies of plants, "there is considerable evidence that distances of from 50 feet (15 m) to a few miles (several kilometers) may effectively isolate populations, and there is no evidence of longer-range gene flow." (1229)

In all of these studies, dispersed populations spanning over thousands of kilometres, even across continents, retain conspecific similarity. Based on the illumination of geographic separation alone, Ehrlich and Raven (1229) proclaim that

> [t]here seems no possibility that gene flow "holds together" its widely scattered population. The cave-dwelling collembolan *Pseudosinella hirsuta* occurs in a series of populations in the southeastern United States. There is no gene flow between them (28), yet they resemble one another. *Clarika rhomboidea* occurs in the Great Basin of the western United States as a disjunct series of similar populations in widely separated mountain ranges. These are separated by gaps of scores of hundreds of kilometers and they are genetically highly differentiated. Gene flow can have no bearing on their evolution under present conditions, and we suggest that these...*examples are representative of the vast majority of plants and animal distributions.* (my emphasis)

What Ehrlich and Raven are suggesting as they proceed to diminish the scope of gene flow processes and interactions is that others often remark at how species are "held together," but often seem to do so only because gene flow is an easily envisaged process with a concrete nature. As a result, they (1229) claim "[i]t may be that in certain continuously distributed species—if there are such—the regular exchange of genes between populations prevents differentiation in the face of different kinds of selection pressures at different places. But such a situation has never, to our knowledge, been demonstrated convincingly in either plants or animals." Since publishing this thesis, Ereshefsky (2001, 117, references included) notes that numerous biologists have corroborated it.

What is particularly striking about Ehrlich's and Raven's paper is that many of the examples to which they point are species for which gene flow *had been* considered the operative cause of conspecific similarity. Not only do studies indicate this was mistaken, but many studies provide evidence that where we would have presumed gene flow was the primary cause, *selection* is instead causing conspecific similarity and activating the realization of species cohesion. For instance, "[t]his is clearly what is happening in *Euphydryas editha* in California as well as in many other butterflies with populations that are totally isolated from one another."

Ehrlich and Raven proceed to show that this is what is happening within many of the species for which they found gene flow to be inert, such as "plants on the numerous low atolls scattered through the Pacifics," where separation prevents gene flow across the atolls, but where "[t]he plants which occur on them are identical everywhere, as contrasted with the plants on the high islands which present *different selection regimes*." (1230; my emphasis) In concluding their paper, the authors note that other authors have been willing to see selection as a cause of conspecific similarity, but *only* when it *seems* there is no isolation preventing gene flow. Ehrlich and Raven aver (1230) that "[t]his assumption seems untenable in the light of our knowledge of how rapidly differentiation can occur, gene flow or no, when selection promotes it." One of my presumptions while demonstrating the scope envy of gene flow, then, is that it is selection that gene flow and interbreeding are envious of. This implies that the conclusions against the individualist in the last chapter seem to hold much of the time. I shall have more to say about this as the chapter unfolds.

In sum, the evidence against gene flow as necessary for conspecific similarity and species cohesion is impressive. Not only is it *a priori* unnecessary for the asexual species that constitute a majority of the world's living forms, but it seems unnecessary for sexually reproducing species as well. This begins to cast serious empirical and conceptual doubt on the gene flow view of conspecific similarity that is the crux of the individualists' argument from gene flow.

4. GENE FLOW NOT SUFFICIENT

Moving on from necessity, let us now consider whether gene flow is sufficient for conspecific similarity and species cohesion. Biologists have been accumulating evidence, especially over the past two decades that suggests a negative answer here.

Take, for example, Allan Templeton's (1989) work (and his review of similar work by other researchers) showing that in the plant kingdom, members of distinct species frequently find ways to overcome *pre*-zygotic reproductive barriers, i.e. they successfully interbreed and exchange genes, and yet this is insufficient to unify species. Repetitive genetic exchange fails to cause similarity or bring, say, two distinct species into a larger one that displays species cohesion. Often, more than just two distinct species exchange genes within a shared geographic location. Templeton and others call such gene exchanging clusters of multiple species *syngameons.* What ensures that a single species within a syngameon retains its species cohesion is natural selection (11). This inspires biologists to acknowledge such cohesion and refrain from referring to the whole syngameon as a single species, despite the prevalence of inter-species genetic exchange. Templeton (1989, 10-11) puts it thus:

> The species within a syngameon are often real units in terms of morphology, ecology, genetics, and evolution. For example, the fossil record indicates that balsam poplars and

> cottonwoods...have been distinct for at least 12 million years and have generated hybrids throughout this period (Eckenwalder 1984). Even though the hybrids are widespread, fertile, and ancient, these tree species have and are maintaining genetic, phenotypic, and ecological cohesion within and distinction between and have maintained themselves as distinct evolutionary lineages for at least 12 million years (Eckenwalder 1984)...cottonwoods and poplars are real biological units that should not be ignored.

Templeton points here to cottonwoods and poplars but the ability to remain distinct in the face of interbreeding is not unique to plant species as many zoologists once believed. Recombinant DNA techniques show the same phenomenon is common in all four major mammalian groups (i.e., primates, ungulates, carnivores and rodents) and even in the fruit fly *Drosophila*, a model genus used in genetic studies of animals more generally (11).

Syngameons are examples of genetic exchange being insufficient to *bring* conspecifics of different species together. Yet it seems gene flow is also often insufficient to *keep* the cohesion of single species intact. This casts doubt on the presumption that gene flow was a primary cause of conspecific similarity in the first place. To support the idea that interbreeding fails to keep species together, authors have suggested numerous mathematical models of speciation showing that selection can overwhelm even very high rates of gene flow and cause a population to diverge (e.g. Kondrashov 1986). I mentioned above that such models of speciation were once thought to be interesting, but perhaps not often instantiated in nature. Increasingly sophisticated models now turn back this worry. Models of *clinal speciation*, for example (see Baker 2005, 317ff.), show that a population can diverge when there is "a steep environmental gradient across a series of semi-isolated populations, such that selective pressures at one end differ significantly from those at the other end." (317) Using my terminology we might say that in clinal speciation, multiple (at least two) selection regime systems are instantiated within a continuous geographic space across which conspecifics interbreed. Gene flow is insufficient to keep the whole population together and, at the same time, natural selection is keeping each of the two daughter populations together.

Models of *ecological speciation* are similar to clinal speciation models, yet instead of steep environmental gradients allowing for multiple selection regime systems, distinct niches play the key role, where "a population comprises multiple niches over a spatially homogeneous range." (317)

Whereas models of such forms of speciation once proposed unrealistic "start up conditions," Baker (318-319) notes that they now model real instances of speciation more closely. More specifically, traditional standards of "single-locus models" made it difficult to show how genetic divergence would be initiated during clinal or ecological speciation. But with the increasing suspicion that traditional standards do not respect the complications of nature, wherein no single factor contributes to genetic divergence but instead several individually insufficient ones do, clinal and ecological models now seem more compelling. As Baker (319) puts it,

> divergent habitat preferences, low hybrid fitness, modifier genes, and other specific genetic and population structures are each unlikely either to emerge in a population or to produce reproductive isolation [but] the likelihood that some combination will do so is much higher—particularly with the assumption of stochastic genetics.

Moreover, several studies now purport to show such speciation actually occurring, and old studies are being reinterpreted under the light of newer models (e.g. Kawecki 2004; Dieckmann and Doebeli 2004; Johnson et al. 1996; Howard and Berlocher 1998; Endler

1973; Lande 1980, 467; Grant 1980, 167). Thus, it now seems doubtless that the insufficiency of gene flow in keeping a species together explains, for example, how studies of gene-sharing house mice that were bound within single barns, "showed 'microgeographic' variation" within the populations (Ehrlich and Raven 1969, 1230). Marc Ereshefsky (2001, 117) similarly takes stock of recent criticisms of the insufficiency of gene flow and concludes, "such considerations imply that the unity of many, *perhaps most*, species is the result of genetic homeostasis or exposure to common selective regimes rather than gene flow." (my emphasis)

Interestingly, Ehrlich and Raven (1969, 1231) drew a similar conclusion long before Ereshefsky, saying that evidence of gene flow not being sufficient for conspecific similarity does indeed "undermine arguments about 'gene flow' as a cohesive force binding together all the populations of some widespread species into a genetic entity, even if such binding were not patently impossible for most organisms on purely distributional grounds." (1231) Perhaps it is just that Ereshefsky is able to look back upon more recent work and buttress what were for Ehrlich and Raven more predictive remarks, the strongest of which being that gene flow "eventually might be discovered to play a rather insignificant role in evolution as a whole." (1231)

In this section we have seen that many biologists think gene flow is insufficient for keeping conspecifics similar within most species. Coupled with the previous section, wherein we saw that gene flow is generally not necessary either, we have chiseled away at a longstanding presumption of many individualists. Although individualists sometimes concede that the gene flow or interbreeding views of conspecific similarity have their shortcomings, they tend not to appreciate the depths of these shortcomings, and tend then to presume that for many species, interbreeding and gene flow are causes of conspecific similarity that activate the realization states of species cohesion. But if we look more closely at what many biologists who work on species cohesion and species concepts are saying, rather than at those quarters within which the gene flow view and BSC are merely entrenched, it seems that gene flow is neither a necessary nor a sufficient cause of conspecific similarity. The gene flow view of conspecific similarity does not enjoy the scope of applicability often presumed.

It is worth noting here that some of the biologists I have quoted over the past three sections suggest their critiques of the gene flow view of conspecific similarity are general assessments of the causal status of gene flow. They generalize from the fact that gene flow seems insufficient and unnecessary for species cohesion in many species, to the claim that there seems little conceptual, let alone empirical basis for thinking that gene flow or interbreeding have ever had the causal force we once thought. I want to follow the lead here and now turn to focus more explicitly on the conceptual inadequacy of thinking that gene flow is a cause of conspecific similarity.

5. THE CAUSAL POVERTY OBJECTION

While seeing how a number of authors have recently gone against the grain and doubted the significance of gene flow, we also saw that some of these authors suggest that conspecifics tend to be similar so as to share evolutionary fates because they are exposed to common selection regimes. Selection causes conspecific similarity.

This is certainly a minority view. However, I noted above that its minority status is not due to there being anything impossible about the idea that selection causes conspecific similarity. Indeed, Ehrlich and Raven (1969, 1230) pointed out that many authors are happy to countenance selection as a cause of conspecific similarity *when* gene flow is clearly not an option. Ehrlich and Raven then supported the claim that it seems gene flow is not an option much of the time.

But further conceptual argument is required here. Sometimes (even if rarely due to the pervasiveness of isolation), gene flow is at least an option in the sense that conspecifics may enjoy high rates of gene flow. In such cases, why should we think gene flow is casually impoverished with respect to conspecific similarity? Evidence suggesting that gene flow is neither a necessary nor sufficient condition for conspecific similarity gives us a start on answering this question, for, as I mentioned, this evidence casts a measure of conceptual doubt on the claim that gene flow has causal powers. But this conceptual doubt cries out for further development. Yes, gene flow seems neither necessary nor sufficient for conspecific similarity. However, to further establish the causal poverty of gene flow, we need to ask *why* this is the case. If gene flow is neither necessary nor sufficient for conspecific similarity, then there is good reason for thinking we suffered conceptual confusion in attributing the causal powers to it that we have. But what *explains* the causal poverty of gene flow and thus our conceptual confusion?

Over the rest of the chapter I offer an answer to this question that, I think, secures the point that gene flow is causally impoverished and unable to serve as the crux in the individualists' argument from gene flow: every time gene flow seems to have a causal role in making conspecifics similar, this is only so *in virtue of* selection being a primary cause of conspecific similarity. Gene flow appears to hold populations together in the face of selection pressures that can cause changes in the population. But if selection is a primary cause of *population change*, as is widely thought, then, I shall argue, selection is necessarily a primary cause of *conspecific similarity* as well. Thus, when gene flow *seems* of causal import in the holding together of a population in the face of selection pressures, to the discerning mind this will be indicative of selection having *been* a more crucial cause of conspecific similarity. Gene flow's significance *depends upon* the causal primacy of selection. If selection is the primary cause of conspecific similarity every time gene flow seems of significance, it is conceptually confused to say gene flow is a primary cause of conspecific similarity.

There is a way to make this explanation of gene flow's causal poverty even more vivid. It may be that the causal primacy of selection lends gene flow any significance it has by way of gene flow being a mere background condition for selection's causal primacy. In its various manifestations (e.g. interbreeding, horizontal gene transfer, etc.) gene flow is merely a *method* of distributing trait-building tools (e.g. genes) and this offers little basis for thinking gene flow is even a lesser *cause* of traits being similar. Selection causes the similarity that gene flow merely distributes.

Though this may be one way of making my objection more vivid, the claim that gene flow is a mere background condition of conspecific similarity because it is a mere distributor of similarity is a strong one that I shall make more cautiously. The important and more easily reached claim is that gene flow's significance *depends upon* the causal primacy of selection such that the causal interactions that active the core realization of a species' responsive cohesion are those within a wide selection regime system and thus those interactions are of a wide and aggregative sort. Therefore, we have no reason to think that an instance of species cohesion corresponds to integrative cohesion as well. In short, the gene flow view of conspecific similarity is empirically problematic *and* conceptually confused and so fails as the crux in the individualists' argument from gene flow.

My causal poverty objection turns on the claim that selection is a cause of conspecific similarity if it is a cause of population change. Of course, this claim only gains traction if selection is, in the first instance, a cause of population change. Thus, to advance the causal poverty objection, in the next section I clarify the nature of selection and suggest it is relatively uncontroversial to claim selection is a cause of population change. In section 7, I discuss the nature of causation and background conditions more generally in order to make the substantive claims of my objection more plausible. I argue for those substantive claims in sections 8 through 10, showing how selection is a cause of conspecific similarity if it is a cause of population change, and showing how this entails the causal poverty of gene flow.

6. THE CAUSAL NATURE OF NATURAL SELECTION

I want to suggest that despite complexities surrounding the nature of natural selection, it is relatively uncontroversial to say that natural selection is a cause of change in populations. In chapter one I discussed such change in some detail while alluding to goldenrod gall flies and the three Darwinian principles of evolution by natural selection. Selection pressures may cause allele or trait frequency changes in a population, where such changes may constitute microevolutionary change (change within a species) or macroevolutionary change (speciation), when there is *phenotypic variation* (principle 1) among organism's of a population, such that variation corresponds to *differential fitness* (principle 2) that is *heritable* (principle 3).

Although this view of selection as a cause of population change is the received view, complexities surrounding the nature of natural selection have recently led a few authors to challenge that view. Rather than a cause of change, these authors argue, selection is merely an inert property of sequences of "individual-level" events involving, for example, organisms and "selective pressures" such as predation and sunlight (e.g. Walsh et al. 2002; Matthen and Ariew 2002). On this view, natural selection theory is not a "dynamical theory" but rather a "statistical theory" akin to bookkeeping and it explains changes in populations in terms of means and variances in trait fitness, rather than in terms of the "force" of selection.

Other authors (e.g. Reisman and Forber 2004; Stevens 2004) have pointed out, however, that these non-causal interpretations of natural selection mistakenly generalize from a critique of the way in which selection is described to a more substantive claim about the causal efficacy of selection. More specifically, *critics* of selection-as-a-cause take their argument against the somewhat controversial idea that "selection should be described as forces in a Newtonian sense" to also serve as an argument against the more general and widely accepted idea that selection processes are "population-level causes of evolutionary change." (Reisman and Forber 2004, 1-2) However, as the *defenders* of selection-as-a-cause point out, "[w]hether evolutionary processes should count as 'forces,' and what this entails about the nature of causation are interesting but peripheral questions," while the general thesis that selection is causal seems secure (2). I shall substantiate this further below.

Failing to distinguish between Newtonian forces and causation more generally may represent a more widespread ambiguity that infects natural selection talk. For example, in a recent paper analyzing natural selection as a mechanism, Robert Skipper and Roberta Millstein (2005, 328-329) note that,

> Evolutionary biologists call natural selection, and the other evolutionary mechanisms, many things, which they all seem to think amount to the same thing. Natural selection is a "cause," a "force," a "process," a "mechanism," a "factor." Sometimes, natural selection is called a "principle" or a "concept," but when the explication continues, cause, force or mechanism talk is apparent. We think there is no question that contemporary evolutionary biology exemplifies the view that natural selection is a mechanism.

Despite observing such ambiguity, Skipper and Millstein claim that the basic idea is plain: natural selection produces change in populations. Skipper and Millstein prefer to think of selection as a *causal mechanism* of change and think evolutionary biologists have established this to an extent that it is recent philosophical accounts of mechanisms that need adjusting, rather than our conception of selection as a mechanism, if the philosophical accounts do not capture natural selection theory. Indeed, though Skipper and Millstein recognize (as is standard) natural selection, genetic drift, mutation and gene flow as "the four evolutionary mechanisms," they agree with the consensus that selection is the "apotheosis" of these (328).

Likely, the ambiguity and metaphorical language characteristic of natural selection talk are unfortunate sources of the complexities surrounding selection theory and I think such ambiguity and the corresponding complexities have illegitimately motivated the anti-causal interpretation of selection that authors such as Walsh et al. promote. One of the central concerns of Walsh et al. is that talk of forces tends to reify natural selection as a thing that exudes Newtonian vector-type force, while yet natural selection clearly is not a thing.

But even authors who do claim selection is a "force" do not thereby "reify" natural selection. For example, in his (1984, 100) book *The Nature of Selection*, Elliot Sober explicitly characterizes natural selection as a *causal process* and not the sort of entity that reification would suggest, when he says that

> "[s]election for" is the causal concept *par excellence*. Selection for properties causes differences in survival and reproductive success, even though...overall fitness is causally inert. An organism's overall fitness does not cause it to live or die, but the fact that there is selection against vulnerability to predators may do so. Overall fitness gives a summary picture of an organism's vulnerability to possible selection forces. There being selection for a particular property, on the other hand, means that a certain causal process is *actually* in motion.

It thus seems that if Walsh et al. (2002, 467) are willing to admit "[p]redation, sunlight, and competition" as "selective forces," then it is ad-hoc to subsequently claim that these are not causes of change falling under the rubric of natural selection (see their section 4.3). Authors who see selection as a cause do not envision predation as a thing, but rather as a process or set of interactions occurring between predator and prey that may cause change in population trait frequencies in a componential way, over time. And as Sober explains, we might also see selection not just as a cause of population-level properties such as trait frequencies, but also as contributing to the living or dieing of a single organism, if that organism is caught up in the "motion" of a selection process. When Sober asserts that selection refers to causal processes in motion, he is admitting as causal the same things that Walsh et al. do, that is, the interactions associated with predation, etc. Talk of forces is important for Sober's overall theory, but it is extra to the idea that natural selection at least refers to causal processes.

The argument of Walsh et al. (and others like it) deserves more time than I shall give it here, but I hope to have made a *prima facie* case for setting it aside in favour of the standard causal interpretation of natural selection. To avert any further difficulty with the standard view, we might recall from the previous chapter the way in which I suggested we conceive of natural selection as processes operating within *selection regime systems*. A selection regime system is a causally integrated network of entities that also includes the properties, relations and states of those entities, and the interactions between the entities. Conceiving of selection processes as occurring with the objectively delineated boundaries of such systems allows us to see the concreteness of selection processes without reifying selection as a "thing" external to organisms that exudes forces. The three Darwinian conditions of evolution by natural selection obtain in the forms of processes within selection regime systems and these processes cause population change.

For further support of the notion of selection regime systems, I shall direct the reader to the relevant arguments of chapter four. Let me now exploit the notion of *manipulation* to lend some final justification to the causal interpretation of selection.

My appeal to manipulation in this context follows the lead of Reisman and Forber (2004). They similarly invoke the notion of manipulation that is at the heart of the scientific method, while arguing that experimental and natural manipulations of what seem to be selection processes can show that selection is at least a cause of change, even if appeal to Newtonian forces gets things wrong. The basic argument is that if the dynamics of a population change in virtue of manipulation of the "character of selection" in a population, then selection is a cause of population-level change. This argument does not depend "upon any specific account of the necessary and sufficient conditions for causation, but only on a single sufficient condition." (2) That single condition is "the manipulation condition (MC)" and though it is inherently limited by the potential for confounding between variables, within controlled environments it can be a reliable indicator of causal relationships. The MC says, for example, "if you can systematically manipulate the position of a switch to bring about a change in brightness of a light bulb, then the position of the switch is a cause of the brightness of the bulb." (2-3)

Here, Reisman and Forber would do well to clarify that in the case of the light bulb, the MC helps us to see *not* that the position of the switch is a cause of brightness, but that the manipulation of the switch is a cause of a certain level of electricity that is a cause of brightness. But the utility of the MC is still apparent, keeping in mind that to be ideally reliable we must control—or have sufficient reason for thinking such controls exist—factors that potentially confound variables and vitiate ascriptions of a causal connection for which the MC purports to be sufficient. Note that one need not give assent to manipulation theories of the causal relation in order to make use of the MC. As merely a sufficient condition, "MC fits naturally with most philosophical accounts of causation." (4)

Biologists have (at least implicitly) exploited the MC to establish the efficacy of selection in countless experiments and studies, both in the lab and in the wild. Our gall flies from chapter one are the subject of one famous study in the wild, by Weis et al. (1992). Weis and his colleagues observed manipulations in 16 populations of the fly, *Eurosta solidaginis,* across 64 population-year combinations. The "manipulations" in this case were variations in local bird and wasp populations, each of which interacted differently with the gall flies. The wasps tended to target *small* galls, into which they deposited eggs whose hatchlings would eat the "protected" gall fly larvae (recall, the galls are the protective growths in the plant which protect the gall flies and whose size depends on fly saliva types). The birds targeted *larger* galls while preying upon gall fly larvae. The researchers were able to measure manipulations because it was clear whether galls had been attacked by birds or instead by wasps and thus they could reliably infer the effective population sizes of the birds and wasps. The MC insists that if selection is causal, then the variations in effective wasp and bird populations that constitute manipulations should correspond to changes in the population dynamics of the gall plants and gall flies that, in part, direct plant growth. The researchers did observe such corresponding changes. In 64 sample population-years, the frequencies of gall diameter types (and thus saliva types in the flies) stabilized when birds were more predominant than wasps, and the frequencies showed directional change to large galls when wasps predominated. This was largely because the affects of birds were not nearly as statistically significant as those of wasps. Of course, we may be able to account for these changes in population dynamics in terms other than the manipulations observed. But the study makes a compelling case for manipulation substantiating the causal nature of selection (Futuyma 1998, 424).

By appealing to the MC in more overtly controlled lab experiments, researchers have repeatedly found selection to be causal. For example, Dobzhansky and Pavlovsky (1957) studied polymorphic traits in fruit flies to document the efficacy of and relation between natural selection and another cause of population change, genetic drift. Drift is random change in frequencies of two or more alleles or genotypes in a population, which becomes especially significant in small populations where "chance" occurrences of otherwise rare allele combinations can spread quickly through the population (Futuyma 1998, 297). While comparing sets of populations of 4000 flies with sets of populations of 20 flies, and holding selection pressures constant among them, Dobzhansky and Pavlovsky observed that in both large and small populations, the heterozygote types PP and AR for polymorphic traits each fell from initial frequencies of 50% to stable polymorphic equilibrium. Because the population sizes were different, they reasoned that drift would not play an equal role in each and so would not be responsible for such similar equilibrium states; therefore the cause of "going to equilibrium" seems to have been selection. Or, as the researchers put it, "Heterozygotes that carry a PP and an AR...are superior in adaptive value to the PP and AR homozygotes. Therefore, the frequencies of PP and AR chromosomes in the experimental populations are *controlled by* natural selection" (318; quoted in Reisman and Forber 2004, 9, my emphasis; note: though selection caused trait frequencies to go equilibrium, drift was a cause of a different effect: of there being comparatively more variation among equilibrium values within the small populations). Saying that trait frequencies were "controlled by natural selection" is to say that selection caused certain trait frequencies in the population. No decomposition of selection (or drift) into component forces was attempted in this study. Instead, manipulations simply leant strong evidence to the more general causal nature of selection (10).

This wraps up our consideration of the nature of selection and its causal status. However, before moving on to show that selection is also a cause of conspecific similarity and that, therefore, wide selection regime systems are the realizers of species cohesion when gene flow seems significant, we can first make use of the two above examples of the MC at work to make more elementary but important comments on the concepts of *causation* and *background conditions*. These comments will assist with the adjudication of gene flow's causal status.

7. CAUSES AND CONDITIONS

The application of the MC corresponds roughly to John Stuart Mill's "Method of Difference" (1967) for detecting causes and testing causal claims. In his recent and illuminating discussion of the distinction between *causes* and *background conditions*, Peter Menzies (2002, 13) describes the relevant part of Mill's Method here to be the "*difference observation* between a positive instance in which some effect *E* is present and a negative instance in which *E* is absent. If some condition C is present in the positive instance and absent in the negative instance, it is, at least, part of what makes the difference to *E*." The difference observation as such marks a long tradition of thinking of causes as difference-makers, and that is how I like to think of them. The "difference observation" is one way of detecting causes as difference-makers and it should be clear that the difference observation is, essentially, the notion that application of the MC exploits.

Conveniently, our two examples of the MC in action correspond to two different forms of the difference observation that John Mackie (1974, 71-71) distinguished. Roughly, the gall fly application corresponds to what we might call the *before-and-after* approach, and the fruit fly application corresponds to the *experimental-and-control* approach (see Menzies' discussion of Mackie).

The before-and-after approach is one "in which some change C is introduced, either *naturally* or by deliberate human action...The state of affairs just after the introduction is the positive instance and the state of affairs just before it is the negative instance." (Menzies 2002, 13, my emphasis) In the gall fly scenario, the "introduced" changes manifest in the variation in wasp and bird populations were "natural" rather than human-induced, and the *difference between gall fly saliva type frequencies in the* "negative instance" and those in the "positive instance" suggest that the introduced changes were the difference-makers. This before-and-after approach may work reasonably well for detecting causes, but Mackie notes it certainly is not a sufficient account of causation.

The experimental-and-control approach exemplified in the fruit fly example is a different form of difference observation, where "the experimental case is compared with what happens in a deliberately controlled case which is made to match the experimental case" in the perceived relevant ways. Dobzhansky and Pavlovsky made use of this approach when testing the efficacy of genetic drift. Drift, they suspected, was constituted in the "founder effect," i.e., when population size shrinks substantially. Thus, the researchers held all selection pressures constant in both the experimental cases (small populations) and control cases (larger populations) and found that the founder effect (i.e. genetic drift) was a difference-maker in the variation among equilibrium values of genotype frequencies within small fruit fly populations (recall that selection was the cause of a distinct effect: of the frequencies going to equilibrium at all). Mackie thinks this approach is more useful in offering an actual *account* of causes as difference-makers because an account based upon it would incorporate both counterfactual *and* factual conditionals, which helps to rule out problems that typically plague counterfactual analyses of causation, such as preemption.

This is not the place to delve into competing accounts of the causal relation, but I do wish to use the discussion thus far to say something about how we can roughly conceive of causes. Causes are indeed difference-makers, however that gets formally spelled out, and as such there are, as we have seen, methods for detecting them. Of the two methods cited, the experimental-and-control approach is more reliable, though conditions in nature often force us to make do with the before-and-after approach.

A further issue is how to distinguish causes from background conditions. Aside from the methodology he proposed, Mill also documented the difficulties in justifying this distinction, as well as those surrounding the notion of "the" cause more generally. David Lewis (1986, 215-216) knows the difficulties well, saying that "[t]he multiplicity of causes and the complexity of causal histories are obscured when we speak, as we sometimes do, of *the* cause of something." If oxygen is necessary for the ignition of a forest fire, why are we uncomfortable calling it the cause, instead of the dropped cigarette? Given the necessity of oxygen for the fire, there seems little reason to rule it out as *a* cause, which at least denies the cigarette is *the* cause.

Menzies (2002, 9) points out that despite these difficulties, there is "a long tradition" of trying to base objective distinctions between causes and background conditions upon "pragmatic considerations of context." Both Mill and Lewis have attempted this, though without much success (9). The impetus for trying comes from the overwhelming commonsensical urge to say, for example, that oxygen and the dropped cigarette do not have equivalent causal status. I will be suggesting that natural selection is more like the cigarette and gene flow may be more like the oxygen, and so I want briefly to put in place two bases for this distinction.

First, instead of treading the risky cause/background condition divide, we could appeal to primary causes and lesser causes. The above Mill-inspired applications of the MC represent ways of distinguishing between the significance of one difference-maker vs. another, over repeated trials. Of course, picking out the salient difference-makers will never be a completely objective affair, for what we find of greatest causal significance even in tightly controlled experiments will depend in part on our epistemic abilities, the questions we ask and our prior knowledge. Nonetheless, our ability to pick out primary causes is evidenced routinely by the success of predictions we base upon them, and from the empirical investigations that lend such success, we should be able to draw further conceptual inferences regarding causal primacy. I suggest this is the case with selection. Empirically, we believe it is a primary cause of population change and even in some senses a cause of individual-level events (see above passage from Sober). After showing that selection-as-a-cause-of-change is necessarily selection-as-a-cause-of-similarity, the implication should then be that selection is also a primary cause of conspecific similarity. This is a deflationary approach to the problem of distinguishing between background conditions and one that we shall see still produces a conclusion that rejects the individualists' argument from gene flow.

Second and more controversially, we may try claiming there is an objective basis for the cause/background condition distinction. While introducing the language of realization in chapter three I said that background conditions are objectively distinct from systems, the states of which constitute total realizations of properties. The justification for this lay in the fact that systems as such seem to have observer-independent (if fuzzy) boundaries in time and space. The states of such systems work as total realizations of certain properties (e.g. the circulatory system as a total realizer for the property of having a blood pressure of 120/80) (R. A. Wilson 2004, 110). Making an analogy between realization and causation here is not entirely straightforward, but may be helpful. As R. A. Wilson (133) suggests, perhaps causes form systems of a sort as well, or causal chains, that we can delineate reasonably well from background conditions. Authors have tried spelling out how this might work. Menzies (2002), for example, thinks there are different *natural kinds* of systems, the intrinsic properties of which determine the nature of the causal chains found within them. The distinctions between such systems ensure that investigations of causes in a given system will be context sensitive, though still objective given the system's status as a natural kind. Repeated investigations of such systems and causal chains afford causal models for each kind of system. The models give an account of what counts as cause and what counts as background condition.

Menzies substantiates his view with rich discussions of the nature of intrinsic properties, laws and possible worlds. I shall not rehearse those here. Rather, I want to note that there are *prima facie* promising ways of distinguishing between causes and background conditions on naturalistic grounds. My ensuing comments on background conditions, then, will be less committal than those surrounding the more secure notion of a primary cause.

8. A CAUSE OF CHANGE IS A CAUSE OF SIMILARITY

After the preparatory detour of the last two sections, it will pay to review exactly what it is we are about to object to. We have seen that the individualists' argument from gene flow apportions causal powers in the following way: gene flow causes conspecific similarity and is responsible for species cohesion, while natural selection causes population change and destroys species cohesion. If a species "stays together" it is because "the regular exchange of genes between populations prevents differentiation in the face of different kinds of selection pressures at different places." (Ehrlich and Raven 1969, 1229)

In light of the above discussion of causes and conditions, we can say that many individualists see gene flow interactions as operative *given* the background conditions of a selection regime. This is implied through such statements as Mary Williams' (1970, 357), who says a species is a lineage that "is held together by cohesive forces so that it acts as a unit with respect to selection" (see also Wiley 1981, 25; Simpson 1961, 53; Hull 1976, 183; Holsinger 1984, 293). This clearly gives selection a role to play in the idea that species are held together, but the role is as a background condition. It would nary make sense, the reasoning goes, to talk of anything holding a species together such that it has a distinct evolutionary fate if there were not processes such as selection with respect to which it makes sense to speak of distinct evolutionary fates.

When advancing the argument from gene flow the closest individualists usually get to giving selection a more substantial role to play is when they point out that selection may direct the fate of single organisms, in the sense that "selection against vulnerability to predators" may "cause an organism to live or die." (Sober 1984, 100) However, gene flow between conspecifics is still responsible for fates tending to be shared and thus for species cohesion.

Pace the relegation of selection to background status, if selection processes cause population change then they necessarily cause conspecific similarity too. Change and similarity are two sides of the same conceptual coin. As a more specific example, we can say that within selection regime systems interactions between organisms and features of their environment cause conspecifics to be similar and thus they activate the states that realize cohesion. To see this, consider an idealized example involving the salamander, *Ensatina eschscholtzii*, where selection causes change and thus similarity as well.

Unseasonably dry conditions in California ensure that those salamanders that tend to survive through the mating season to reproduce successfully are those that retain body moisture more efficiently. Thus, in the next generation of salamanders the frequency of genes that are (in part) responsible for moisture retention efficiency will increase (for brevity it will pay to speak of gene frequencies here). Therefore, climatic conditions (and perhaps other process and features of the selection regime system) constitute a selection pressure that causes a change in gene frequencies.

In this case of selection causing change, selection selects some genes or traits over others. But selecting "for" genes that are responsible (in part) for moisture retention is also to select "against" those genes that are responsible (in part) for a salamander not being able to retain moisture efficiently. Selecting against genes as such culls variety from the gene pool and a reduction in variety entails a corresponding increase in the similarity of genes and traits that remain. But this is just to insist that natural selection is a cause of conspecific similarity. And if we agreed that selection had a primary causal role in changing gene frequencies, we should agree that its role here in causing conspecific similarity is likewise as a primary cause. Therefore, natural selection is not just a background condition of similarity among conspecifics. Had the weather not been unusually dry and posed a selection pressure, there would not have been a tendency for conspecifics of the next generation to be similar with respect to water-retention traits. There *was* unusually dry weather and the next generation of salamanders *did* have similar such traits. Natural selection seems a crucial, that is, a primary difference-maker of conspecific similarity.

Moreover, we should think selection is crucial whenever it *seems* gene flow has any role to play at all. Recall that many individualists take the causal power of gene flow to obtain "with respect to" selection, that is, during episodes of selection pressures working to cause change within a population. But we have just seen that whenever selection is a primary cause of population change in this way, it is also a primary cause of conspecific similarity. Therefore we should aver that every time it seems the causal power of gene flow obtains "with respect to selection," then selection is not only causing change but is trumping gene flow as the primary cause of conspecific similarity too. Any time gene flow seems of import to species cohesion, selection ensures that the traits that conspecifics have are indeed *shared* traits and, therefore, that selection is instead the primary cause of conspecific similarity and the basis for species cohesion.

Since this point is important to my causal poverty objection, let me also come at it in another way. When the flow of shared traits seems important, the cause of the similarity that those shared traits instantiate actually owes to selection. The flow of that similarity is merely the distribution, not the cause of that similarity. So, when the flow or distribution of similarity seems important at all, it is only because natural selection is a cause rendering it intelligible to speak of similarity at all. Gene flow is at best a lesser cause of conspecific similarity here, and at worst a mere background condition of it. Perhaps it is a mere background condition just as air is a background condition of my circulatory system's functioning in virtue of it distributing needed oxygen to the lungs. Below I shall discuss the implications of deciding whether gene flow as a mere distributor of conspecific similarity is either a lesser cause or a background condition. For now, I submit the conclusion that *every time gene flow seems to have a causal role to play in making conspecifics similar, it has at most a lesser role, and it has this role in virtue of selection being a primary cause of conspecific similarity.*

This conclusion entails that every time individualists hope that gene flow integrates conspecifics into a cohesive entity as the individuality thesis demands, they shall be disappointed. It is selection that "holds" species together when it seems gene flow has integrating force. This also adds conceptual force to Paul Ehrlich's and Peter Raven's (1969, 1228) argument for the claims that "selection is both the primary cohesive and disruptive force in evolution, and that the *selective regime itself determines* what influence gene flow (or isolation) will have." (my emphasis)

I have no reason to suspect that the results of the salamander scenario will not generalize to most instances of evolution by natural selection. If selection causes change, then necessarily it causes similarity. Here, similarity is not just tightly linked with change; it is logical closure of the phenomenon. In some ways, this is not a completely new point, but rather a clearer and much stronger way of putting the idea that lurks in numerous "species concepts," such as the majority of phylogenetic species concepts that appeal not just to natural selection but also to "historical constraints." Chris Horvath summarizes this idea briefly in his (1997, 662) paper on the "external factors" that individuate species, when he says the following while introducing a host of species concepts that appeal to historical and developmental constraints:

> The future of a lineage is determined, in part, by the evolutionary changes that have taken place in its past. One adaptation in the past can make a second adaptation in the future more likely and, thus, reinforce the cohesiveness of the particular lineage which shares this developmental history.

We can get a clearer sense of how the past determines the future if over the next two sections we draw out the further conclusions that the argument above affords.

In the next section I explain that when selection is a cause of conspecific similarity, it is aggregative, wide causal interactions that activate the collective state of conspecific similarity in a species, and that, in turn, this state realizes species cohesion. This will simply amount to a more detailed and vivid presentation of the argument offered in the previous chapter that showed species cohesion corresponds to responsive cohesion when selection causes conspecific similarity, and that in such cases nothing indicates that this responsive cohesion might owe to the integrative cohesion that individualists seek in species. Then, in section 10, we can couple the argument of this section (i.e. 8), with that of the next (i.e. 9) to confirm that integrative cohesion seems *always* elusive to the individualist.

9. NO INDICATION OF INTEGRATIVE COHESION

When interactions between conspecific salamanders and environmental selection pressures cause conspecific similarity we should expect that those interactions activate the core realizer state of species cohesion. For the core realizer state of species cohesion is *collective conspecific similarity*, i.e., the tendency for conspecifics to share evolutionary traits that, in turn, realizes the tendency for them to share evolutionary fates. The collective state of conspecific similarity and thus the core realizer state cannot be activated without conspecifics being similar in evolutionary terms, and selection causes conspecifics to be similar as such. Therefore, when the causal interactions that constitute selection processes cause conspecific similarity, they are what activate the core realizer state of species cohesion.

Moreover, the operative causal interactions of selection here are both *aggregative* and *wide*: they are between (say) individual salamanders (which have certain relevant properties, relations and states) and features of the environment (which have certain relevant properties etc.) such that in independent but additive fashions the conspecifics come to be similar in evolutionary terms. These wide and aggregative *interactions*, then, not only constitute a selection process that causes conspecific similarity but *that* constituted process occurs within the wide selection regime system that is instantiated by all of the relevant causally integrated entities, properties, states, interactions, etc. Therefore, the interactions that cause salamander similarity are interactions that are "parts" of a wide selection regime *system* that stretches beyond the bounds of the species (i.e. just the organisms and interactions between them) to include environmental features, etc.

With the operative interactions helping constitute a wide selection regime system, the state of this wide selection regime system is the *total realization* of species cohesion. Species cohesion *as such* thus amounts to the components (i.e. conspecifics) of an entity (i.e. species) responding to pressures as a unit such that this cohesion approximates an *aggregative property* with a *wide realization*. This is virtually the definition of responsive cohesion and we observe two sure signs (the aggregative property sure sign and the wide realization sure sign) that cohesion *as such* is not integrative cohesion. Given this, let us clarify our argument by saying such species cohesion is *Selection Based Responsive Cohesion*. When selection causes conspecific similarity and those conspecific tend to share evolutionary fates, at least this cohesion is realized.

Now, for further clarity, let us spell out the realization of Selection Based Responsive Cohesion in the case of the salamanders. In this case, the selection process and the interactions constituting it cause conspecific similarity with respect to water-retention efficiency traits in the generation of salamanders that followed a drought. These interactions thus activate the core realizer of species cohesion simply in virtue of the conspecifics being similar in at least one respect. With each of the salamanders in the new generation tending to be adapted to conserve water, in the future they will tend to face any selection pressures relevant to water retention as a cohesive unit.

For example, if in the new generation's *second* year of life the climatic conditions actually become wetter than they, as dry-adapted salamanders, are suited for, a result might be that the "moisture-retention genes" that each conspecific tends to have will actually become a hindrance to survival. If this results in selection *against* moisture retention genes, then the population will change in a cohesive fashion, as a sort of evolutionary unit. It will change from "most members have moisture retention genes" to "most members do not have moisture retention genes" (over time, of course). *If* there is a sense in which members of the salamander population cohere as an evolutionary unit because one set of genes was typical among members, and because that set became "no longer typical," *then* the core realizer of the evolutionary unity of species cohesion in this case is members typically having a certain set of genes, i.e. collective conspecific similarity. The cause of members typically having those genes, we have seen, are wide, aggregative causal interactions constituting the selection process within a wide selection regime system. This reveals exactly how selection causes conspecific similarity and thereby activates the collective conspecific similarity that is the core realizer of a cohesive species. Such a cohesive salamander species demonstrates Selection Based Responsive Cohesion.

Appreciating, as we do, how aggregative and wide causal interactions are the basis for Selection Based Responsive Cohesion, we can also appreciate more precisely why we have no reason to think Selection Based Responsive Cohesion owes to integrative cohesion.

Although the *core realization* of Selection Based Responsive Cohesion is the collective conspecific similarity that may be intrinsic to the species, if that realization is activated by wide, aggregative properties and interactions, then no integrative causal interactions between conspecifics seem to be holding those conspecifics together. Yet for responsive cohesion to be indicative of integrative cohesion, the core realization of that responsive cohesion must be activated by intrinsic, integrative causal interactions that are between components of the cohesive entity. We saw that in the case of the brook trout, for example, that the fish's parts were responding as a cohesive unit to the pressures of osmosis that the fish's environment presented, and that the fish was demonstrating responsive cohesion as such *just because* those parts were causally integrated in either or both a structural and/or a functional sense.

What was indicative of the fish's responsive cohesion owing to integrative cohesion was the fact that the core realization of its responsive cohesion was activated by integrative causal interactions between the fish's parts. Complex integrative interactions between cells, membranes and extra cellular fluid, as well as those between parts of the water balancing system more generally, activated the saline states of the parts of the fish. And jointly, these saline states were the core realization of the fish's cohesive response to osmotic pressure. Granted, the total realization of the fish's responsive cohesion still stretched beyond the fish to include features of the environment posing the osmotic pressure. But the core realization of the responsive

cohesion was at least intrinsic and, critically, was activated by the sorts of intrinsic interactions that integrative cohesion requires. In this way the fish's responsive cohesion was indicative of integrative cohesion.

Unfortunately for the species as individuals view, we have seen in the previous chapter, and spelled out in more detail here, that what we now call Selection Based Responsive Cohesion does not have a core realization that is activated by intrinsic integrative causal interactions between conspecifics. There is thus no indication that Selection Based Responsive Cohesion is coinstantiated with integrative cohesion such that it owes to integrative cohesion. When species display Selection Based Responsive Cohesion, they do not satisfy the MICCI.

As we began to appreciate in the last chapter, this conclusion will hold across species even when, as in the case of dam-building beavers, there may be group level traits and thus group level similarity involved. When cohesion has these componential group origins, there still need be no integrative causal interactions between conspecifics, and if there are such interactions, they do not form a chain-like system of interactions that would unite all conspecifics as parts of an individual.

10. THE INADEQUACY OF SPECIES COHESION

We are now in position to draw out the conclusions of the causal poverty objection to the argument from gene flow. We do so by coupling the previous section's detailed explanation of why species cohesion does not refer to integrative cohesion when selection causes conspecific similarity, with section 8's argument for selection being a primary cause of conspecific similarity whenever gene flow seems significant to species cohesion. Insights from previous chapters will help as well.

Upon considering the leading species concepts we found that there are three putative causes of conspecific similarity. The developmental homeostasis view of conspecific similarity does not help the individualist because it does not form a basis for saying that species cohesion is indicative of individuality. In the section directly above we have seen in detail why the same is true for the natural selection view of conspecific similarity. Selection Based Responsive Cohesion gives no reason for thinking species display integrative cohesion as the individuality thesis requires.

This leaves only the gene flow view of conspecific similarity as an option for the individualist. That view serves as a crux in the individualist's argument from gene flow because it suggests that when gene flow causes conspecific similarity, species cohesion will have a core realization that is activated by the sorts of integrative causal interactions that hold paradigm individuals together and activate the realization of their integrative cohesion.

However, in section 8 above we saw that the gene flow view of conspecific similarity is faulty. Gene flow is causally impoverished in that any time gene flow seems a cause of conspecific similarity we should admit that this is conceptually confused and that selection is a primary cause of conspecific similarity. Ironically, this entails that any time the individualist advances the argument from gene flow, it is actually an "argument from natural selection" that goes through instead, whilst blocking the argument from gene flow. This is disastrous for the individualist because it denies their only option for showing that species are cohesive individuals.

I want to entertain one worry here that will raise the issue of whether, with respect to conspecific similarity, selection relegates gene flow to the status of a lesser cause, or instead to the status of a background condition. The individualist may point out that if gene flow still has some, even if little causal role to play in keeping conspecifics similar, then there may be *some indication* that the integrative causal interactions required for integrative cohesion do obtain in the species and that, therefore, we might have *some* reason for thinking that a Selection Based Responsive Cohesion owes to integrative cohesion.

My reply here is to say that if gene flow as a *lesser* cause of conspecific similarity gives the individualist "some" reason for thinking integrative cohesion obtains, it is negligible reason indeed. Upon completing our analyses of cohesion at the end of chapter three I explained that if responsive cohesion is to owe to integrative cohesion, then the intrinsic, integrative causal interactions that are thought to activate the core realizer state of the responsive cohesion in question must indeed have the causal force to activate that state. If those integrative causal interactions that *may* be indicative of integrative cohesion do not have the causal power to activate the core realizer of responsive cohesion, then we cannot presume they *are* indicative of integrative cohesion. While lacking causal adequacy as such, it must instead be that wide and/or aggregative causal interactions—that are not indicative of integrative cohesion—are pulling the causal load and activating the core realizer of responsive cohesion. We thus seem forced to conclude that responsive cohesion does not *owe to* integrative cohesion. Therefore, even if gene flow has some lesser causal role to play in the specific case of conspecific similarity, we must conclude that Selection Based Responsive Cohesion does not owe to integrative cohesion, because selection pulls the causal load and is constituted by causal interactions that are not indicative of individuality. At the end of chapter three I mentioned that the "causal poverty sure sign" could help us detect such instances of integrative causal interactions not being up to the task and that this sure sign would allow us to conclude that a putative cohesive entity did not satisfy the MICCI. The causal poverty sure sign gains traction here.

Notice: because there are only three widely recognized possible causes of conspecific similarity, and because of the way in which gene flow can do no better than a lesser cause, even when conspecific similarity has *multiple* causes, the resulting responsive cohesion of species will still not be indicative of integrative cohesion. Gene flow is always trumped by a cause (i.e. selection) that entails we have no reason to countenance the integrative cohesion of species even when there is more than one cause of conspecific similarity. And when gene flow seems not a cause at all, we likewise have no reason to countenance the integrative cohesion of species.

It thus appears that nothing hangs on our decision to treat gene flow as a lesser cause of conspecific similarity, or instead as a mere background condition. If gene flow is a lesser cause, Selection Based Responsive Cohesion is not indicative of integrative cohesion, and if gene flow is a mere background condition, the same conclusion holds.

Now, this may prove wrong. For example, the philosophical study of realization is still young and authors are still working through the relation between realization and causation. Upon further reflection, one might want to adjust the way in which I have conceived of the relation between responsive cohesion and integrative cohesion in terms of causal interactions activating core realizations and being indicative of one kind of cohesion or the other. Perhaps future adjustments will result in gene flow being indicative of integrative cohesion even if it is a lesser cause.

I want to head off this speculative possibility with another possibility that seems no more speculative, i.e. the possibility that gene flow is indeed a mere background condition. Though there is not space here to reach closure on whether gene flow is a mere background condition or not, let me offer further support for thinking it is.

Gene flow only comes to matter insofar as it is a method of distributing the similarity that selection creates. As we saw with the salamanders, selection culls variety from the gene pool and thus necessarily brings uniformity to conspecifics. In the next generation, gene flow may have ensured that such uniformity was passed on, but it nonetheless resulted from selection and will be shaped into a new type of uniformity in the future (e.g. as when wet weather returns) in virtue of selection pressures again. This is the sense in which I have said gene flow merely distributes similarity, while selection causes it.

If gene flow merely distributes similarity as such, and we enlist the promising work of Peter Menzies that I mentioned above, there seem good if inconclusive reasons for treating gene flow as a background condition. Menzies (2002) thinks there are different natural kinds of systems, the intrinsic properties of which determine the nature of the causal chains found within them. Upon study of the relevant natural kinds of causal chains here, we may have naturalistic ground for saying that as a distributor of similarity, gene flow does not have a place in the actual causal chain responsible for conspecifics being similar. Consider that, on the one hand, it is at least clear from the above argument that if gene flow is in the causal chain that produces conspecific similarity, it is more distal from the effect than is selection. But this implies, on the other hand, that if gene flow has a place in the causal chain it is in some sense a cause not just of conspecific similarity, but a cause of "natural selection *qua* cause." I am not sure one can make sense of gene flow as a cause of selection. Perhaps gene flow ensures conspecifics are involved in similar selection processes, but it could only do so through its affect on the genes or traits that are involved in interactions with selective pressures, and I have already shown that the central affect on traits—what ensures they tend to be similar across conspecifics—is selection. There seems, therefore, no clear place for gene flow in the causal chain.

In this section I have drawn from our work in this chapter and the previous two to offer and defend the following conclusion. Gene flow must be a primary cause of conspecific similarity for the individualist, but any time its seems to be, selection is the actual primary cause, and so any time the individualist appeals to gene flow as she must, species cohesion actually corresponds to Selection Based Responsive Cohesion, which in turn entails that species do not satisfy the MICCI. Individualists have no option but to conclude that species are not cohesive individuals.

To see how this conclusion surfaces in the broader context of a summary of this chapter, we can recall that the gene flow view of conspecific similarity is the crux of the individualist's argument from gene flow, but because we have shown in this chapter that the crux is faulty, the argument from gene flow does not go through. In the first half of this chapter, I argued against the crux of the individualist's argument through appeal to empirical evidence, where that empirical evidence was also suggestive of conceptual confusion surrounding the idea that gene flow causes conspecific similarity. Gene flow seemed neither necessary nor sufficient for conspecific similarity and so the crux of the argument from gene flow seemed conceptually confused.

In the second half of this chapter I attempted to explain this conceptual confusion, showing why gene flow is not a primary cause of conspecific similarity. To do this, I first established the causal nature of selection and then offered preparatory discussion surrounding the distinction between primary causes, lesser causes and background conditions. This laid the groundwork for showing that selection is a primary cause of conspecific similarity any time that gene flow seems significant to conspecific similarity, and that furthermore, there are reasons for thinking gene flow is a mere background condition of conspecific similarity. This causal poverty objection leant support to Ehrlich's and Raven's claim that "selection is both the primary cohesive and disruptive force in evolution, and that the selective regime itself determines what influence gene flow (or isolation) will have." However, as neatly as the causal poverty objection and the scope envy objection go together, they can work independently too.

The gene flow view's inability to work for a vast majority of species is enough to cast serious doubt on the argument from gene flow, as is gene flow's seeming causal poverty.

Finally, we appreciate that the objections of this chapter block the individualist's *only* argument for showing that species are cohesive individuals. Chapter four demonstrated that the argument from gene flow seems the individualist's only hope for claiming that integrative cohesion obtains in species, but we saw that the natural selection view of conspecific similarity always trumps the gene flow view, and that this trumping natural selection view of conspecific similarity always entails there is no good reason for thinking species cohesion corresponds to integrative cohesion.

In short then, there are no extant species concepts or views of conspecific similarity that give the individualist reason for thinking species display integrative cohesion. If individualists were to get past their cohesion complacency and appreciate the distinctions between kinds of cohesion, and if they were to continue to invest in something like the MICCI that I have articulated, then they would see that current biology entails that species are not individuals.

6

Beyond Individuals and Back Again

...the individual needs to be transcended
in order to do justice to the richness of the corresponding domain.

...genes exist and thus reproduce only in individuals, and groups do so only through the reproductive efforts of the individuals they comprise.

Robert A. Wilson 2005

1. CONCLUSION

In chapter one I introduced the species ontology problem as one that seeks the ontological status of species. Generally, two views have competed to solve this problem. The species as kinds view says that each species taxon belongs to the ontological category "natural kind." The species as individuals view says that each species taxon belongs to the ontological category "individual." The currently popular individuality thesis takes a stand on both of these views, arguing against the former and in favour of the latter.

I hope to have cast doubt on the individuality thesis' negative half and rejected its positive half.

Even though the second chapter focused on the negative half of the individuality thesis and its four arguments against the species as kinds view, it also helped demonstrate just how radical the species as individuals view is. For the individualist to entirely abandon the kinds view on account of evolutionary insights is, in effect, to claim that traditionally we have made a stark *category mistake* when it comes to species taxa. We saw, however, that natural kinds theory has impressive revision potential and need not commit to the traditional essentialist doctrine with which individualists saddle it. As a result, abandoning the kinds view seems hasty at best and mistaken at worst. Individualists have not engaged alternative kinds views and, in any case, have not targeted the proper kinds views when mounting their negative arguments. With the option to adjust our view of natural kinds in light of scientific advance, we have an option for solving the species ontology problem that makes the species as individuals view seem radical indeed. The species as individuals view thus took on added burden as we turned to investigate it and the nature of cohesion.

In chapters three through five I hope to have made clear why the positive half of the individuality thesis cannot carry the burden that revised kinds notions ensure it must. The species as individuals view not only seems strained beside the kinds view, but given that species do not display the cohesion of individuals, the species as individuals view is untenable. This should motivate us to further flesh out the revised species as kinds views that already have such promising though neglected starts in the works of Richard Boyd, Hilary Kornblith, Robert A. Wilson, Paul Griffiths, Joseph LaPorte and Anjan Chakravartty.

Now, it is true that my conclusion that species do not display the cohesion of individuals grew out of a distinction between kinds of cohesion that we crystallized through the technical language of realization. However, analyzing cohesion in terms of realization only brought the problems for the individualist into focus; the individualist's problems do not depend upon the language of realization. To see this, it may pay to summarize the conclusion I presented at the end of the last chapter in terms more familiar to those who debate species ontology.

Many individualists realize that causal forces internal to species must keep conspecifics together if species are to be individuals. Because many individualists think gene flow binds conspecifics, they think that forces internal to species bind species in the same *kind* of way that forces internal to paradigm individuals bind those individuals. But we have seen that gene flow is causally impoverished with respect to ensuring conspecifics share traits, share fates and are thus bound. Instead, when gene flow seems significant, external forces actually keep species together. External forces do not keep paradigm individuals together in this way. Even if external forces did have important roles to play in the causal unity that paradigm individuals display, a majority of authors still think that individuality consists (at least in part) in internal causal unity as well. Necessarily, there are integrative causal interactions between an individual's parts. However, when selection is what keeps species together, the idea of species cohesion give us no reason for thinking that the internal integrative causal interactions required for individuality exist within species. Moreover, empirical evidence suggests an absolute minority of species may be kept together by the internal forces of gene flow, and conceptual considerations help explain these findings by suggesting that gene flow is never what keeps species together. The external forces of selection keep species together when it seems that gene flow does. Finally, no other accounts of species cohesion, buried in competing species concepts, offer consolation to the individualist here. Species cohesion has external origins that preclude them from being individuals.

Thus, even without the language of realization, we see that the individuality thesis fails to solve the species ontology problem. The species as kinds view seems the most promising alternative.

With the substantive claims of this work now concluded, over the next few sections, let me enter more speculative territory while considering the implications of my conclusions and the further work to which they point.

2. HOMEOSTASIS AND THE BSC

Having concluded in favour of the kinds view, we might recall one specific option for developing the kinds view and how it can shed light upon the discussions that followed our consideration of kindhood.

The idea that many of the natural kinds that scientists investigate may be homeostatic property cluster (HPC) kinds reflects a certain disdain for the traditional approach to scientific definition. Traditionally, definitions of dispositions, kinds, and so on, have often appealed to one essential criterion. But dispositions and kinds do not seem individuated by single, essential properties. Rather, it seems that many kinds in nature are individuated by clusters that comprise multiple "essential" properties, where homeostatic mechanisms or causal links between those properties ensure that the properties tend to be coinstantiated and even help explain each other. Recall that the *structural properties* definitive of life are possible in part because of definitive *functional properties*, and vice versa. Viewing properties as homeostatically linked as such provides a powerful way of doubting single criterion scientific definitions. Biological phenomena tend not to have single causes but rather owe to a compendium of states, properties and relations that are causally entwined and which ensure each instance of each phenomenon tends to have complex realizations or multiple causes. It thus seems that scientific definitions will often have to incorporate these facts of the world and thus include multiple criteria that do not form a "mere" disjunctive list, but a list unified by the regular, causal ways in which properties tend to go together.

Bringing these insights to our discussion of species concepts and to our critique of the gene flow view of conspecific similarity might help point the way to developing species concepts and accounts of species that more accurately reflect the homeostasis inherent in nature.

In chapter four we saw that authors think we can subsume the distinct possible causes of conspecific similarity under three general causes, those being developmental homeostasis, natural selection and gene flow. While arguing against the gene flow view in chapter five it may have seemed that I preferred a single criterion account of conspecific similarity that favoured natural selection as "the" cause. At the end of that chapter, however, I mentioned that my conclusions would still hold even if conspecific similarity had multiple causes, and in light of the insights that the HPC view lends, I think authors should pursue such a multiple cause account. If gene flow and interbreeding do have some minor causal roles to play, along with the more substantive factors of selection, developmental homeostasis and the historical constraints that selection and developmental histories impose on the evolution of conspecifics, there are reasons for thinking these factors may reinforce one another. For example, selection may cause certain similarities in conspecifics in part because of the way the homeostatic developmental cycles of conspecifics buffer them against change and predispose them to certain shared paths through shared selection regimes.

If there are homeostatic mechanisms linking the causes of conspecific similarity, we have a basis for further pursuing the revisions of the BSC that I bruited at the end of chapter four. My critique of the gene flow view of conspecific similarity may give one the impression that I see little hope for the BSC, but recall that we brought philosophical clarification to the notion of reproductive isolation and that this made the BSC seem more versatile. Rather than corresponding only to *processes* of pre- and post-zygotic isolation mechanisms, conceiving of reproductive isolation as a property of species that is separable from the property of interbreeding allowed us to appreciate that reproductive isolation offers a basis for thinking of the BSC as not only a process-based species concept, but as a pattern-based concept as well. Species are reproductively isolated from *each other* and this serves as the basis for envisioning reproductively isolated lineages as layering out in a pattern of evolutionary trajectories. Rather than there being a single cause of each species' isolation (e.g. gene flow), there are multiple causes that homeostatically reinforce one another. Thus, a BSC that gives reproductive isolation a key role to play and admits that conspecific similarity and the reproductive isolation of the conspecific collective owe to multiple causal processes, will be a BSC that integrates the intuitions at the hearts of numerous species concepts that are now supposedly "competing" with one another.

Certainly much conceptual and empirical work is needed if we are to countenance an integrative species concept. But the discussions in this work at least lend hope to those attempting such integration. And let it be clear that by recognizing multiple causal processes, a species concept can indeed be integrative rather than strictly pluralistic. The homeostatic mechanisms that relate causal processes form the basis for conceiving of those processes as integrated and not independent components in a pluralistic view. Indeed, they form the basis for thinking of the species category (and not just each species taxon) as an HPC kind itself.

In this section we have seen that our discussion of revised notions of kinds in chapter two may indirectly bear upon our later discussions of species concepts and causes of conspecific similarity in a helpful way. But rejecting the species as individuals view and investing in the species as kinds view may have more direct consequences as well, especially for biological taxonomy. Let me now say something brief about this possibility.

3. TAXONOMY AND SPECIES AS KINDS

I noted in chapter one that some authors have thought the individuality thesis serves as a basis for their recommended overhauling of biological taxonomy. For example, authors who urge that we abandon the Linnaean classification hierarchy that biologists have used for over 200 years, in favour of rank-free taxonomic systems such as *PhyloCode*, have claimed that their calls for abandonment "rest on the philosophy of individualism" (Keller et al. 2003, 106; and see de Queiroz 1992). Having claimed the individuality thesis does not go through, there may be *prima facie* reason for doubting the suggested taxonomic revisions that rest on the individuality thesis. However, this would be a hasty conclusion. Although authors think the individuality thesis bears most directly upon biological taxonomy, nobody has clearly spelled out this relationship. At best, we can say what work needs doing and suggest what shape that work may take in light of our critique of the individuality thesis.

Criticisms of Linnaean classification have had numerous motivations, but their philosophical motivations have rested at least in part on both the case *against* the kinds view and the case *for* the species as individuals view.

On the one hand, some authors think Linnaean classification emerged from a strict essentialist view of the world; that it is thus infected with that view; and that, therefore, it is unable to capture the dynamic nature of the living world that we came to appreciate after Darwin (e.g. Ereshefsky 2001, de Queiroz 1992).

On the other hand, if species are individuals, authors have presumed that we can only define them via ostensive means. Essential or "intentional" properties do not characterize individuals and so to define an individual we cannot appeal to properties that all of its parts share. Instead, we must merely "point at" the individual and christen it with a name (Ghiselin 1974; Keller et al. 2003, 98). Since on the Linnaean scheme we attach binomial names to species in virtue of intrinsic or relation properties that conspecifics (at least tend) to share, we are not using the ostensive method that the individuality thesis seems to imply we must. Therefore, rather than arrange species into classes within a classification scheme, we are instead to arrange them as individuals within a system that is based upon the natural relations of systematic inclusion between them (98).

One problem with the simple idea that the individuality thesis supports ostensive definition and, therefore, the abandonment of the Linnaean hierarchy, is that authors have realized that even alternative and "individualistic" taxonomic schemes like the PhyloCode actually do appeal to intentional, not just ostensive definitions. When ordering an individualistic taxonomic system we do not merely "point at" a common ancestor and all its descendants in order to define species. For while pointing as such, we are claiming that all members of a species share the essential property of descending from a certain ancestor. In short, even within the PhyloCode, a taxon is "defined by the necessary and sufficient property of common origin, rather than as an individual." (99)

Thus, authors need to further specify just how the species as individuals view motivates a break from taxonomic tradition. In one of the few papers that criticizes the individuality thesis while also exploring the corresponding consequences for taxonomy, it seems to me that Keller et al. (2003) fail to make up much ground here. Keller et al. *note* the putative relation between individualism and alternative taxonomies, and then argue for the HPC view and against the individuality thesis, but they fail to *specify* the relation between individualism and alternative taxonomies, and fail to show why the

HPC view might help save Linnaean taxonomy. As a result, it is not clear why alternative taxonomies fail along with the individuality thesis, nor clear why Linnaean classification succeeds on the back of HPC kinds.

In light of the intentional definitions to which taxonomic individualists now realize they appeal, my hunch is that suggested taxonomic alternatives do not rest upon the positive half of the individuality thesis as much as they rest upon its negative half. It is arguments against the kinds view that allow criticisms of Linnaean taxonomy to get a philosophical toehold. To further develop the defence of the Linnaean taxonomy that Keller et al. initiate, our work from chapter two shows that authors may need to enlist the work of historians. Let me explain.

It may well be that alternative taxonomies rest upon dubious arguments against the species as kinds view. We have seen these arguments make a sort of strawman of the species as kinds view. Species may indeed be HPC kinds. But to offer philosophical vindication to Linnaean taxonomy (pragmatic vindication is another matter), we must show how Linnaean classification can tolerate the looser brand of essentialism upon which the HPC view is based. This will involve analyzing the methods of practicing taxonomists as well as the nomenclatural codes to which they adhere, in order to see if such "taxonomic epistemology" comports with "HPC metaphysics." Do taxonomists tend to group species by clusters of properties? Are type specimens flexible such that descriptions are malleable and reflect underlying homeostatic mechanisms of the species?

I suspect the answer to such questions is 'yes' and one way to begin to support that answer is to refer to the work of Mary P. Winsor, Staffan Muller-Wille and Lorraine Daston—historians whom I discussed at the outset of chapter two. I shall not restate that discussion in full here, but recall that Winsor (2003) shows that pre-Darwinian taxonomists who employed Linnaean classification seemed, through their methods, to unwittingly treat species as HPC kinds, not kinds in the traditional, strict-essentialism sense. Muller-Wille (2003) is a Linnaean scholar whose work strongly suggests that Linnaeus' own methods and the taxonomic revolution he led in colonial times comported with the HPC view. Finally, Daston (2004) focuses on taxonomic type specimens in botany and her work suggests that non-taxonomists misunderstand modern type specimens. No part of such specimens are representative of a typological species essence that all members of that species must share. Type specimens are rigid designators only in the sense that they afford a permanent record of species names, while their descriptions are malleable, multifaceted and integrated as one would expect if a type specimen represented a species that was an HPC kind.

In sum, further work needs doing on the relation between species ontology and taxonomy, but coupling our critique of the individuality thesis with the work of historians helps show how our conclusions may gain salience within debate over biological taxonomy. There are viable projects for those wishing to defend the philosophical support for a Linnaean taxonomy tweaked in accord with revised species as kinds views.

4. THE MYSTIQUE OF AGENCY

Over the rest of the chapter I want to turn from discussing the implications of rejecting the species as individuals view in favour of the species as kinds view, to consider possible consequence of our focus on cohesion more specifically. At the start of this chapter I reformulated my conclusions without appeal to realization. But one reason why the appeal to realization is useful is because it brings needed clarity to the concept of cohesion. The appeal to realization was not just a convenient way to attack the species as individuals view. I really do think cohesion is realized, *not* caused, and that this clarifies the role that causal interactions *do* play with respect to cohesion: they activate realizer states in a way that gives wide and "merely" aggregative causal interactions important roles to play. Understanding cohesion in this way helped reveal a distinction in kinds of cohesion and this, in turn, can help shed light on the relationship between agency and individuality. This relationship deserves more attention within the philosophy of biology.

Aside from the arguments of individualists that I have considered, I suspect that one reason why so many authors have approved of the species as individuals view is because the notion of agency is tightly bound with that of individuality, and species seem to be agents of evolution.

Individuals are causally unified, but so too do the components of agents seem to enjoy a causal unity. Indeed, authors often define agents as loci of causal differentiation. Consider that R. A. Wilson (2005, 6-7) says an agent is an "entity that is a locus of causation or action. It is a source of differential action, a thing from which and through which causes operate." This seems to imply that the things constituting agents enjoy some sort of causal connection that ends at the bounds of the agent.

The notion of responsive cohesion and the idea that cohesion can depend on context afford bases for *decoupling* agency and individuality in a way that might reveal mistaken motivations of the individuality thesis, and which may help reframe debates peripheral to the species ontology problem. But these are two specific ways in which cohesion can bring clarity to issues of agency and individuality. More generally, further efforts to liberate the notion of agency from that of individuality could guide the needed development of more integrative scientific methodology. Let me show how we can draw the two more specific upshots from our discussions, and then draw the more general upshot from those.

Consider that an entity need only demonstrate responsive cohesion in order to be an agent or locus of causal differentiation. One might say that a species, for example, is an agent of evolution. It travels along a distinct evolutionary trajectory in virtue of the way its conspecifics or groups of conspecifics respond as a cohesive unit to (say) selection pressures. Species are causally distinct units as such in virtue of their responsive cohesion. This does not draw a tight connection between agency and individuality, for we have seen that responsive cohesion need not be indicative of individuality. Instead it draws a connection between agency and cohesion more generally. I think this is right: an entity is a locus of causal differentiation, and thus an agent, if its components are causally connected, and this is just the idea that cohesion attempts to capture. As we have seen, the relevant causal connections may be of an indirect and aggregative sort and still set off an entity as a cohesive unit, and so these sorts of causal connections may also set entities off as agents.

If entities may be agents in virtue of their responsive cohesion, then their agency, like their cohesion, may have a wide realization. Agency may owe to context. For example, a species' agency depends not just upon its constituents, but upon the higher level aggregative properties those constituents realize, and, as well, upon the relations in which species stand with other species. What I am suggesting then is that we often have to transcend the boundaries of both individuals and agents when seeking to understand agency.

Tying agency to cohesion rather than individuality as I am suggesting we do ensures there will be many agents that are not individuals. An entity need only demonstrate responsive cohesion in order to be an agent and responsive cohesion need not be indicative of individuality. Sometimes, such agency may seem especially like individuality, such as when some components in an entity *do* interact in integrative ways. But we know that not all components need interact as such in order for them to nonetheless constitute an entity that demonstrates responsive cohesion. Beaver families, again, may causally interact and yet *Castor canadensis* does not display integrative cohesion; the species demonstrates cohesion in virtue of its members responding to (say) selection pressures as a unit. Moreover, neither do we need to conceive of the beaver family itself as an individual with integrative cohesion. Rather, it is a group that displays responsive cohesion if family members respond to selection pressures such that the dam they build advantages their family as a whole, in an evolutionary context. In this way, we see that a *group* can be an "agent of selection," in virtue of its responsive cohesion, and so *agentive groups* with responsive cohesion may often *not* demonstrate integrative cohesion nor be individuals.

Since scientists are often interested in determining the causes of things, they are often interested in agents. Agents play causal roles in causal processes. But if agents need not be individuals, then, scientists need not always be concerned with individuals. Certainly individuals are often agents and so we will often appeal to individuals when offering a causal explanation of a phenomenon. But because agents may be non-individuals, such as groups, we need not force individuality upon the things that are of interest to us when we are attempting explanations. Indeed it will often be useful to transcend focus upon individuals and instead investigate non-individual agents. And this means, too, that we shall often want to investigate not just the constituents of agentive entities, but the *contexts* that make their agency possible.

We can now better appreciate the two specific and two more general ways in which I said our focus on cohesion could clarify issues involving agency and individuality. First, we can see how sharpening the concept of cohesion in terms of realization isolates a possible mistaken motivation for the individuality thesis. Authors who are convinced that species are agents of some sort have appealed to this agency when arguing that species are individuals (e.g. Ghiselin 1974). But we now see that species need not be individuals to be agents. Indeed, given that I think entities are often agents in part because of the contexts in which they are situated (e.g. contexts that help ensure they have responsive cohesion), it simply seems a mistake to use species agency as an argument for species individuality. Agency is a concept that is crucial to our attempts to understand the living world. But agency cannot license substantive claims about the ontological status of entities. When authors appeal, in various different ways, to an apparent equation between agency and individuality, they appeal to what I call the *mystique of agency*: "if something is an agent it must surely be an individual, or if something is an individual it has the ontological might required for agency." From what I have said it is clear that I think the mystique of agency leads our ontological claims astray.

The second specific way in which our discussion of cohesion can clarify issues involving agency and individuality is also afforded by the realization that agents need not be individuals. This realization can help reframe debates peripheral to species ontology.

Take, for instance, the "levels of selection debate" that seems to have been the flagship debate within the philosophy of biology over the past 30 years. The presumption is that there are distinct hierarchical levels at which selection *may* operate, such as at the level of the gene, the organism, the group, the species, the clade, and so on (for complications surrounding the presumptions that "hierarchical thinking" embodies, see R. A. Wilson 2005, ch.10). If selection works at one of these levels, then authors consider the entities situated there to be "agents of selection." The disagreement is over which level(s) selection operates at, and so over what the agents of selection are. The literature distinguishes between different kinds of agents of selection though, such as replicators, interactors, beneficiaries and manifestors. These distinctions point to important complications, but generally, a simple Darwinian dogma still pervades the debate. This dogma presumes that selection only "works upon" individuals, so that, for example, to be a manifestor (i.e. a thing that accrues adaptations) or a beneficiary (i.e. a thing that can be differentially represented in future generations), a thing must be an individual.

We can make a first and broad distinction in this debate between two camps.

On the one hand there are group-selectionists who think selection works in equally important ways at many levels of the evolutionary hierarchy so that (say) organisms, kin-groups and species may all be significant agents of selection. Being a group-selectionist, then, normally goes hand in hand with being a "multi-selectionist."

On the other hand, there are those who think selection primarily works at the level of the organism or the gene, and not, in any case, upon groups.

Both of these camps exploit the mystique of agency, though in different ways.

For example, group-selectionists such as the team of David Sloan Wilson and Elliot Sober (1994) often attempt to strengthen the group-agency argument by appeal to the individuality of groups or species, such as when they say, "higher units of the biological hierarchy can be organisms, in exactly the same sense that individuals are organisms, to the extent that they are the vehicles of selection." (605). On this view, species have the individuality "required" to be various kinds of selection-agents, including beneficiaries and manifestors; though it is not entirely clear if the individuality of species lends credibility to their status as agents, or whether their status as agents implies they are individuals. In any case, the equation between agency and individuality is appealed to and group-selectionsists *presume* this appeal has an important role to play in their more general *frame-shifting approach* that ensures they are also multi-selectionists. On the frame-shifting approach, "one can treat any one of genes, individuals, or groups as the kind of thing its neighbour is in this biological hierarchy, and thus [say] that groups can be treated as our paradigmatic *biological individuals*, organisms." (R. A. Wilson 188; my emphasis)

The mystique of agency likewise clouds the critics of group-selection/radical frame-shifting when those critics invest in the dubious equation between agency and individuality but then use this *against* the group-selectionist, claiming that groups cannot be agents of selection because they lack the ontological might of individuals. For example, Kim Sterelny (1996) presumes that something like individuality is required for an entity to be an agent of selection—in this case a "vehicle" more specifically—when he says of individuals, "[t]here is a fairly objective description of their location in design space. Their existence and location in the biological world is stance-independent." In the next sentence he then uses the putative "individuality requirement" against the group selectionists' notion of agentive "trait groups" (e.g. beaver families from our previous discussions) when he says, "Trait groups that are not *cohesive* do not share this objective existence as vehicles." (583, my emphasis).

Of course, I think the mystique of agency has misled the authors who are arguing with each other in the levels of selection debate, such as the team of D. S. Wilson and Sober and Sterelny. Agents need not be individuals and so D. S. Wilson and Sober would do well to lay to rest their ontological claims about groups that "can be organisms." Groups can be agents because they can demonstrate responsive cohesion, and one need not posit a "hierarchy of biological individuals" up and down which we can frame-shift.

At the same time, this demonstrates the problem with the expression of Sterelny's objection to trait groups. Sure, groups are not individuals. But this is no basis for arguing against group selection. Individuality is not required for the cohesion that Sterelny thinks is necessary for being a trait group. Trait groups may demonstrate responsive cohesion and this, along with the differential reproduction of the group that occurs as generations in the group replace each other (R. A. Wilson 2005, 184), may suffice for a group to be an agent of selection. My discussions of the componential feature of responsive cohesion through examples of golf groups and beaver families in previous chapters should make it clear why I think groups can demonstrate responsive cohesion that owes in part to their contexts. Arguments against group selection, then, can and need to be more empirical in nature. In short, all parties to the levels of selection debate need to rethink agency in terms of cohesion and context, and if they do, new and more relevant problems will emerge that require debate, thus steering us from what, in this case, is the red-herring of individuality.

Tying up our second specific point about cohesion and the mystique of agency in the manner we just have helps bring out the third, more general way in which I said our discussion of cohesion may clarify issues surrounding agency and individuality. I suggested that if one were to further pursue the liberation of agency in light of appreciating cohesion in terms of realization (i.e. in terms of wide systems, aggregative causal interactions, and so on), then one could motivate, and guide the development of, more integrative scientific methodology.

What I have in mind here is paying closer attention to authors who have recently cautioned against an exclusive penchant for "constitutive decomposition" (e.g. R. A. Wilson 2004; 2005). If agents and the scientifically interesting cohesion of entities more generally can be determined in part by context (e.g. by the independent interactions between an entity's components and its environment), then scientific investigation must often look beyond the bounds of entities in order to understand those entities and the causal processes in which they partake. Methodology that respects the wide nature of agency and cohesion will better capture the sense in which the world and its entities are causally *integrated*—not integrated in the technical sense of integrative cohesion, but rather integrated within the world's broader causal structure. Certainly we have learned much from decomposing entities into their constituents. But we need to complement this methodology with means of studying the wide systems and environments that entities in turn constitute if we are to have a more complete and accurate understanding of those entities and the world through which they move.

I say that this is a more general point to emerge from our discussion of cohesion because authors are only beginning to flesh out integrative methodologies that are less decompositional. With an appreciation of liberated agency new problems that require attention *will* emerge within the levels of selection debate, but until we explore wide cohesion and liberated agency further, and formalize integrative methodologies, it is difficult to specify the shape these new problems will take. Let me take a stab at just one.

Perhaps once we further liberate agency from individuality we will take more seriously the notion of "multi-species trait groups" that some group-selectionists have claimed are agents of selection. And so when Sterelny (1996, 566, f.n.4) takes up the idea that a beaver family may be *an* agent of selection in virtue of each family member benefiting from an advantageous dam, and then playfully criticizes this idea because it must admit that a beaver's fleas benefit from the dam too and so are free-riding parts of the agentive group, we might take the inclusion of fleas seriously here. The "new" problem then might be investigating the context and cohesion of the agentive group in order to understand how the fleas figure in the causal picture in question. It is the articulation of these sorts of problems that will benefit from the extension of the discussions we have initiated.

Integrative methodologies and research projects are not entirely undeveloped, mind you, and authors have already began articulating new and important problems in light of more integrative approaches. In chapter four I briefly introduced developmental systems theory (DST) and the efforts of some developmental biologists to study wide developmental systems that integrate developing organisms within their greater environments. Primarily, authors have developed DST as a critique of gene-centred views of development and this has required integrative approaches to the study of ontogeny. But DST shows promise as a positive research program as well (R. A. Wilson 2005, ch.7) and one way to speed positive development is to further probe integrative approaches that flesh out the sense in which developmental systems are not merely "the sum of developmental resources" (Griffiths and Gray 1998, 130-131), but are constituted by wide causal interactions (e.g. between developing organisms and their environment) that insist we conceptualize those systems as integrated and the entities within those systems as agents exhibiting (at least) responsive cohesion.

In this section I have shown how our discussions of cohesion might form the basis for decoupling agency and individuality and in a way that reveals a mistaken motivation of the individuality thesis, helps reframe debates within the philosophy of biology, and points the way to integrative scientific methodologies more generally. We must be cautious of the mystique of agency.

But in making these points I have focused on the agency side of the agency-individual relation and suggested that in many senses we need to transcend the individual. To end this chapter and *Species of Biology* I now want to briefly consider the individuality side of the relationship we have been inspired to rethink.

5. BODILY ANCHORS OF AN INTEGRATED LIVING WORLD

Decoupling agency and individuality may liberate agency but it also might rightly return individuality to its inherent bodily nature. Granting that entities such as groups can be agents allows us to recognize the important role such entities play in causal phenomena without ascribing individuality to them. Without needing to liberally ascribe individuality to entities to make sense of the scientific explanations in which they feature, we might be open to shoring up our concept of the individual.

Rather than argue in detail for a stricter conception of individuality here, I merely want to point to three bases for doing so that have emerged during our discussions, and then suggest how such a stricter view may make more plausible the view of the living world to which I have been gesturing more explicitly in this chapter.

Recall that while discussing (at the end of section 5 in chapter three) the sense in which individuals display integrative cohesion that has a functional and/or structural aspect, I mentioned that leading accounts of individuality can be organized neatly under the rubric of integrative cohesion. Wiggins (1980) and van Inwagen (1990) might seem interested in developing the notion of integrative cohesion along functional lines, while Ayers (1974; 1999) would seem to prefer a structural account. I noted that Ayers thinks something called *material cohesion* (or material unity) is ontologically basic for individuality and that this is a sort of precondition of an individual's objective functional unity. This seems to me a powerful reason for preferring Ayers' intuitions over Wiggins' and van Inwagen's. The material cohesiveness of structures seems to add legitimacy to the functions we attribute to those structures and their interactions. The structure formed when the Bowman's capsule envelops the glomerulus within the trout's water balancing system seems to help explain why that structure is able to facilitate the process of filtration in just the way it does. Material cohesion seems a structural property in the sense that the parts of a materially cohesive entity cohere in virtue of causal interactions between them that render them material attached or unified in some sense. At least when compared with the individualists' intuitions about individuality, Ayers' conception of individuality seems "strict," since species clearly do not (at least necessarily) compose conspecifics that interact such that they are materially attached and cohesive.

I mentioned, however, that Ayers' account is deficient in that it lacks an empirical account of material cohesion. Ayers offers persuasive metaphysical arguments for his view, though the details of material cohesion need fleshing out. Granted, claiming that material cohesion is more ontologically basic than functional cohesion may be one way to confront Wiggins; the basicness of material cohesion may represent what makes Wiggins' notion of a "principle of activity/function" intelligible, and may do so in a way that undercuts Wiggins view in favour of material cohesion. But van Inwagen, on the other hand, thinks that only the cohesion of living organisms suffices for individuality; to confront this view, Ayers needs some empirical account of material cohesion that shows living cohesion to just be one special form of the material cohesion in which individuality consists.

Decoupling agency and individuality offers a first, indirect basis for countenancing a "strict" or tighter view of individuality such as Ayers'. If agents were necessarily individuals, it would be implausible to think material cohesion constitutes individuality, because many agents are clearly not characterized by material cohesion. Beaver families seem like candidate agents of selection, for example, and are not materially cohesive. Putative "superorganisms," such as hives of eusocial bees, are less controversial agents of selection, and yet bees in a hive are not bound by material cohesion either. But now that we see how agency is distinct from individuality, we see that beaver families and bee hives can be agents and serve the explanatory role they need to, without being individuals, and so without making implausible the idea that individuality consist in material cohesion.

The second way in which our discussion might support Ayers' view, or help show how to sharpen it, stems from our analysis of cohesion. If material cohesion needs empirical fleshing out as I suggested it does, my analysis of cohesion in terms of realization gets us started in a way that Ayers has not attempted. Much work needs doing of course, but continuing to spell out integrative cohesion in terms of structural, material cohesion, via empirical examples, seems a promising way of attempting to delineate the concept of material cohesion.

Finally, our brief discussion of boundedness and continuity in chapter one further supports the material cohesion conception of individuality. We saw that one trouble facing the individualist when she advances the parity thesis (i.e. attempts to claim species are just like organisms with respect to boundaries, continuity and cohesion), is that species do not seem to have the *continuous physical boundaries*, nor the *spatial continuity* that paradigm individuals do. To stay true to the parity thesis, the individualist seemed forced into claiming that species demonstrate these things. But, on the one hand, claiming that a species has a continuous physical boundary that we could "paint without lifting our brushes" (see section 5 of chapter one) ensured that a species would not be spatially continuous. While, on the other hand, preserving the spatial continuity of species entails giving up the claim that they have continuous physical boundaries. The trouble for the individualist becomes clearer here in light of the view that individuals are materially cohesive. Material cohesion may be a sort of precondition of continuous physical boundaries and spatial continuity, and yet species are not materially cohesive.

For example, if an entity demonstrates material cohesion, its parts causally interact in a way that renders them materially unified and this seems to also entail that its parts will be spatially continuous and that they will cluster so as to form an entity with a continuous physical boundary. Even if we conceive of that boundary in terms of an outer surface, and the outer surface of a particular individual is not smooth but highly irregular, we could still paint the surface without lifting our brushes just because the individual's parts are materially cohesive. Surfaces that are continuous physical boundaries containing spatially continuous parts thus seem to owe to material cohesion. This is perhaps the strongest of the three bases for the material cohesion view of individuality. If we invest in the tripartite conception of individuality as many do, the material cohesion view helps explain the attractiveness of that three-fold conception because material cohesion ensures the three conditions of individuality are reliably coinstantiated or even entwined in a sense. Thus, no wonder that they are, together, the marks of individuality.

In short, if material cohesion is a sort of foundation for the tripartite conception of individuality, then we should not be surprised that it is difficult to ascribe the boundedness and continuity of individuals to species, for species do not demonstrate the required cohesion (i.e. foundation) to begin with.

Moreover our discussions show how the material cohesion view is not held back by commitments to agency and may be developed further along the lines of analysis we have begun. If the material cohesion view is plausible on these bases, it underscores the inherent bodily nature of individuals that is difficult to appreciate without decoupling agency and individuality.

Again, the foregoing has not served so much as an argument for tightening our conception of individuality via the notion of material cohesion as it has pointed to the basis for such arguments. As a final plug for tightening our conception of individuality, consider how such tightening and the material cohesion view in particular might help make more plausible the integrated view of the biological world that in this chapter has begun to emerge from the chapters before it.

The biological world is integrated in that many of its cohesive entities depend upon their environments and context for their cohesion and agency. The biological world is also messy though. Its processes owe to coinstantiated multiple causes and those processes are heterogeneous just like the organisms and agents they feature. This messiness is accentuated when one attempts to pay respect to the world's integration, for in paying respect as such we resist tidying-up the world through the atomization of its agents and constituents more generally. Thus, integrationism implies some measure of order, but perhaps paradoxically, it implies some measure of messiness as well. What affords order within this messiness such that it is intelligible to conceive of the biological world as integrated? A tightened conception of individuality is at least one partial answer. To see this, consider again DST.

One criticism of DST is that by countenancing wide developmental systems it is not clear how or why wide developmental systems are still "attached" to the individual organisms that develop in virtue of them (R. A. Wilson 2005, 155). When the causal nexus of a developmental system stretches beyond the individual, it seems, even if that system is *objectively delineated,* that the boundaries of the individual might become irrelevant, such that it is unclear what is anchoring the developmental system and making it intelligible to say the developmental system is a system of *something* that is developing. But if in virtue of *being individuals,* developing individuals are materially cohesive, and thus are spatially continuous and have continuous physical boundaries, then they have an ontological robustness and independence that allows them to be the central things to which wide developmental systems attach. It is *the individual* brook trout that develops in virtue of the wide developmental resources upon which it may depend; it is not the resources nor the system they constitute that develop. There may be exceptional cases where two organisms are developmental resources for each other, whereby it is difficult to say what develops in virtue of what (e.g. see R. A. Wilson 2005, 157). But this does not diminish the sense in which each individual organism *can be* a sort of integrational anchor of its wide developmental system, in virtue of the ontological independence that its material cohesion and thus bodily nature bestow upon it.

So although integrationism asks us to often transcend individuality in some senses, there is another sense in which the messy biological world is integrated in some orderly, lawful fashion in virtue of the bodily individuals that do populate it. Returning individuals to their inherent bodily nature in this way reflects a more principled view of individuality that ensures organisms have a special role to play in the living world's causal structure. As R. A. Wilson (2005, 237) notes in the quote at the head of this chapter, genes and groups may be biological agents, but "genes exist and thus reproduce only in individuals, and groups do so only through the reproductive efforts of the individuals they comprise."

In this chapter we have appealed to previous chapters to comment on the possibilities for an integrated species concept, a defence of Linnaean taxonomy, and the reconceptualization of debates, methodologies and views in which agency and individuality feature. In doing so we have betrayed how our narrow focus upon species ontology both motivates and is motivated by a deep belief that we can more accurately frame the biological world if we sharpen our focus on kinds, cohesion, agents and individuals. Loosening our conception of biological kinds frees the notions of natural kinds and essentialism from dated views of nature and corresponds to the ideas that kinds can be both historical and co-extensive with agentive groups. This, in turn, helps forestall the liberal ascription of individuality to the biological agents we appeal to in our causal explanations. A picture of nature that includes fewer individuals and more kinds will still allow us to capture nature's heterogeneity in our biological explanations; it will just capture that heterogeneity via an integrative approach that highlights how hierarchical levels of biological organization and the agents they contain are infused and integrated, rather than via a fine-grained atomistic approach that imposes a misleading crispness on the world. Reframing our thinking in this way invests in what I have been calling an integrated picture of biology: a certain *species of biology*, if you will.

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