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Dispositions in Evolutionary Biology: A Metaphysically Realist Account

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DISPOSITIONS IN EVOLUTIONARY BIOLOGY:
A METAPHYSICALLY REALIST ACCOUNT

A Thesis

Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Master of Arts

in

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by
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ABSTRACT

In the last several decades, philosophers of biology have published countless books and articles on the causal mechanisms underlying evolutionary change. There has been scant effort devoted, however, to detailed analysis of what these mechanisms mean for the relationship between our best interpretations of evolutionary change and our metaphysical picture of the world. This thesis addresses some key aspects of that metaphysical picture. I argue for a metaphysically realist interpretation of dispositions as causally active in evolutionary biology. I address *fitness* and *evolvability* in particular, as they present two of the best possible case studies for a metaphysically realist interpretation of dispositions. I claim that dispositional realism is justified in part based on its empirical warrant. That is, as a metaphysics of science, it gives us all the metaphysics we need for making sense of the empirical success of science (especially biology), and no more. I present Ontic Structural Realism as an opposing view. Ontic Structural Realism argues for the dismissal of objects and dispositions on the basis of a certain interpretation of fundamental physics. I present some arguments against this view and in favor of my own.

INTRODUCTION

The philosophy of biology and metaphysics have long seemed like worlds apart. This thesis is my attempt to bring them a bit closer together and to see how they might be profitably related to one another. It is surprising, in a way, that there hasn't been *more* work trying to bring metaphysical clarity to bear on philosophy of biology. The philosophy of biology, after all, is mostly the philosophy of *evolutionary* biology, and one of the central problems for the discipline, historically speaking, is the "tautology problem" (see Sober, 1984). That is, philosophers have long been troubled to explain why evolution by natural selection is not just an empty tautology ("survival of those that survive"), but instead a rich and interesting (and *contingent*) empirical thesis.

In the late 1970s a new view emerged from the philosophical literature, developed independently in papers by John Beatty and Susan Mills (1979) and Robert Brandon (1978). This view is known as the Propensity Interpretation of Fitness, as it takes the fitness of organisms to be a general propensity toward leaving offspring to future generations of its species population. This is just to say that the fitter organism is the one that's *better disposed* to leaving more offspring to future generations than its counterparts. This is part of why I say it's a bit surprising there hasn't been more attention given to the metaphysical treatments of these topics in the philosophy of biology literature. One of the central problems in philosophy of biology is clearly tied up in talk about dispositions, and this seems to suggest that there might be good reasons to develop a clear metaphysical analysis of what dispositions are and what kind of causal work they're doing in evolutionary biology (if any). This thesis is an attempt to do some of that work.

I argue for a metaphysically realist interpretation of dispositions and their causal role in evolutionary biology. It seems to me that most of the defenders of the Propensity Interpretation

are already pretty close to this view. That said, one could reasonably hold to something like the Propensity Interpretation, but defend it on basically instrumental grounds. I develop a causal view of dispositions (like the one that the Propensity Interpretation describes) that's more metaphysically robust than this on the grounds that such a view does the best job of making intelligible that which we actually observe in evolutionary biology. To do this, I take a close look at debates over fitness and its causal role in evolutionary biology, and then extend the realist attitude that I develop with respect to fitness and apply it to emerging conversations over the role of *evolvability* in evolutionary biology. I take it that the addition of evolvability as a case study for dispositional realism strengthens my overall position: that dispositions are real and causally active in the evolutionary biology.

In Chapter 1, I sketch a view of dispositional realism and then relate that view to recent discussions of fitness in evolutionary biology. My views on the metaphysics of dispositions have been heavily influenced by Stephen Mumford, Nancy Cartwright, and Anjan Chakravartty (among others). Cartwright and Chakravartty especially have influenced my views on the task of metaphysics as it relates to our empirical experience of causal change in the natural sciences, and this is a point that features throughout the thesis. I argue for a general view that takes dispositions as causally active and not reducible (metaphysically, anyway) to counterfactual conditionals and the like. I then argue for dispositions as causally active in evolutionary biology more specifically, and in the case of fitness in particular. I argue further for holistic fitness (that is, fitness understood in terms of whole organisms) as opposed to trait fitness as the right treatment for the job. Finally, I argue that biological individuals ground the causal change processes we see in evolution, not populations (or their structure).

In Chapter 2, I extend the case that I've made for dispositional realism in Chapter 1 and apply it to recent work on evolvability. I argue for a conception of evolvability as a real dispositional property of organisms, and further that it is best construed as a functional property that's multiply realizable. I pick up on many of the major themes raised by Rachael Brown (2014), while shoring up what I take as some problems with her treatment.

In Chapter 3, I present an alternative view on the metaphysics of science: Ontic Structural Realism. This view sees objects and dispositions as eliminable from our proper ontology. If this view is correct, then it clearly presents *major* problems for the view I've outlined in chapters 1 and 2. I present counterarguments to the view. My treatment of Ontic Structural Realism mainly focuses on two texts: James Ladyman and Don Ross (2007), and Steven French (2014). I use French (2014) most of all, since he's given the clearest and most convincing version of Ontic Structural Realism to date. The counterarguments I offer partly relate to what I've argued in Chapters 1 and 2, and partly they're self-contained to Chapter 3. With Ontic Structural Realism dispatched, we should not take its arguments as a credible threat to objects and dispositions of the sort described in this thesis.

1. DISPOSITIONAL REALISM AND ORGANISMAL FITNESS

How should we talk about “fitness”? This question has received extensive treatment over the last several decades, with the Propensity Interpretation of Fitness (PIF) emerging as the prevailing orthodoxy (Millstein, forthcoming). At least since Darwin, fitness is usually understood in a broadly ecological sense, which is to say that the fitness of an individual or type has no absolute value, in the sense that its fitness is relative to things like its environment (adaptation), the available resources (including the organism’s ability to compete for them), and its conspecifics (including the organism’s ability to successfully mate with them).¹ As regards the PIF, the basic notion is as follows: the fitness of an organism (or type) is best conceived of as a probabilistic propensity understood in terms of the number of offspring it can be expected to contribute to future generations (see Sober, 2001).² This view was first developed by Robert Brandon (1978) and John Beatty and Susan Mills (now Finsen) (1979), and the motivation for undertaking this project was mainly to do with the conceptual problems that plagued the “actualist” interpretation of fitness (Beatty & Mills, 1979; Sober, 1984; Ramsey, 2006; Millstein, forthcoming).³

As far as developing a theoretically rich (and practically useful) model of fitness as predicated on the PIF goes, much of the focus has been on developing an accurate mathematical representation of fitness in terms of this propensity (Brandon, 1978; Brandon, 1990). At least as often as philosophers of biology have tried to develop and defend such formalizations of the PIF, others have leveled seemingly devastating objections against these models, even causing some

¹ For a paradigmatic example of this manner of thinking, see Robert Brandon (1978).

² Sober’s relationship to the PIF is somewhat problematic. At certain points, Sober (1984) seems broadly sympathetic to the view, but in later publications he seems more skeptical. I note this just in order to emphasize that I don’t want to ascribe a view to anyone that that person may not hold.

³ The actualist interpretation of fitness held that fitness was calculated in terms of the *actual* offspring left by some type. This leads to what Sober (1984) and others call “the tautology problem,” which can be stated sloganistically as the (undesirable) reduction of natural selection to the empty claim “the survival of those that survive.”

former proponents of the PIF to turn critics of varying degrees (see Beatty & Finsen, 1989; Sober, 2001; Sober, 2013). One semi-canonical mathematization of the PIF involves taking fitness as the expected number of offspring, or the arithmetic average of the probabilities for various numbers of offspring multiplied by the number of offspring. That is, if organisms of type O_1 have a 50% chance of leaving 3 offspring and a 50% chance of leaving 7 offspring, then by taking the arithmetic average of the relevant values we determine that organisms of type O_1 are expected to leave 5 offspring (see Sober, 2001; Ariew & Ernst 2009). Following rediscovery of John Gillespie's work (1973; 1974; 1979) by Beatty and Finsen, it came to be seen that the representative models favored by propensity theorists were vulnerable to a number of counterexamples. In the simple example given above, for instance, it turns out that we can specify two types, O_1 and O_2 , and make it such that they have identical fitnesses with respect to the expected values for offspring contribution. The actual offspring contributions of the type with lower *variance* in its distribution of outcomes will be greater (over time) than that of the type with higher variance (Beatty & Finsen, 1989; Walsh, Lewens & Ariew, 2002; Ariew & Ernst, 2009). While this seems counterintuitive at first blush, a simple example helps to illustrate why this is the case. Consider a type with a 50% chance of leaving a very high number of offspring and a 50% chance of leaving just one. For each organism of that type, there's a 1 in 2 chance that it contributes very little to future generations, making the population susceptible to loss via drift and the like. So given enough time (probably not much!) the population size is going to tend to zero.

There are other “higher moments” of distributions that ground further objections to this traditional construal of the PIF (mirroring the problem with variance).⁴ Brandon (1990) attempts

⁴ The skew of the distribution curve, for instance, seems to influence the contributions of genotypes to future generations. For more, see Ariew and Ernst (2009).

to deal with some of these objections, but at the cost of the generality that mathematical models of this sort are meant to aim for.⁵ Charles Pence and Grant Ramsey (2013) offer a new mathematical foundation for the PIF, this time using the toolkit of contemporary mathematical and theoretical biology, the result of which is a very general and mathematically sophisticated model that avoids the objections and counterexamples that plagued the original models.⁶ But Pence and Ramsey (2013; Pence & Ramsey, 2015) are careful to point out that the success of the PIF is not necessarily dependent on there being a successful and generalizable mathematical model as a representation of it.⁷ There have been many interesting debates over what we should understand as the best mathematical representation of evolution's causal structure. My interest, however, is in exploring the conceptual and *metaphysical* foundations of evolution and its causal structure, especially with respect to the question of what fitness is, where the question of what fitness is is understood as a question about basic ontological commitments and not a question about which among the various ways of operationalizing the concept are most beneficial for scientific practice (see Sober, 1984; Sober, 2001; Sober, 2009; Millstein, 2006; Millstein, forthcoming).

I will argue that the causal history that the theory of evolution reveals is *primarily* (although surely not only) due to the fitnesses of individuals, and that individual fitnesses are best interpreted as *dispositions*. I want to make it clear at the outset, however, that “best interpreted as” should not be taken to denote anything like an “operationalist” construal of dispositions. Nor am I claiming that to say individual fitnesses are grounded by the dispositional

⁵ For more on the notion of generality (and how it may be gained or lost by different model types), see Pence and Ramsey (2013). Clearly I'm aiming for some manner of generality here, although not necessarily of a mathematical sort.

⁶ The best recapitulation of these counterexamples that I've seen is in Ariew and Ernst (2009). Also see Ariew and Lewontin (2004), although Ariew and Ernst (2009) is probably a bit less technical, and so more accessible.

⁷ That is, it may be the case that fitness is best understood roughly as something like the PIF, but perhaps no one is capable of modeling it mathematically (perhaps it is too complex, or something).

features of organisms is just to say something about the *concepts* we deploy when talking about the biological world. Rather, I claim that fitnesses are best interpreted as dispositions in a way that regards dispositions as *real*, and that this is so because (1) dispositional realism with respect to individuals and their attendant fitnesses best accounts for the relational nature of populations and population change, and (2) the relational structure revealed by a commitment to dispositional realism tells us how the direction of explanation goes. This is to say that dispositional realism helps us to get a grip on the question of whether the primary causal agent influencing the direction of evolution is something more like *individuals* or something more like *populations*. Clearly, I take the former to be the case.

This chapter will go as follows: In (§1.1), I explain and defend my reasons for bracketing questions concerning philosophical treatments of fitness as they relate to biological practice. Philosophers of biology are often at pains to show how their work bears on contemporary debates in the scientific literature. Further, some question the very usefulness of philosophical work on scientific questions where the philosophical work *doesn't* try to do precisely this (see Mayr, 1982). I think this view is misguided, for reasons I will specify in (§1.1). In (§1.2), I sketch an account of dispositional realism and its relevance to the question of what fitness is and how fitnesses contribute to the causal structure of biological science. The account I offer draws heavily from the work of Nancy Cartwright (1989; Cartwright & Pemberton, 2013) and Stephen Mumford (1998). In (§1.3) I will take the account of dispositions from (§1.2) and spell out its relation to points (1) and (2) above. In (§1.4), I offer some concluding remarks.

1.1 Bracketing the question of “Biological Practice”

As mentioned above, philosophers of biology are typically quite concerned to ensure that their

work makes close contact with real world biological practices. To say what this means requires a bit of unpacking. Peter Godfrey-Smith (2009) describes two basic ways in which biologists are typically inclined to approach scientific problems. They might offer a “standard summary” of phenomena, whereby scientists try to offer “literally true” descriptions of the “important features of all cases of a phenomenon” (or, more likely, a true description of some *class* that can be taken to extend, roughly, to each member of that class) (Godfrey-Smith, 2009). Another approach uses simple cases (and possibly even counterfactual cases) as models that serve to explain some central feature of the phenomenon in question.

1.1.1 The model-based approach

Much of contemporary philosophy of biology focuses on this latter approach that takes models as central. Talk of models is commonplace in philosophy of science. The use of models has proven especially helpful with respect to physical theories (see Contessa, 2006; Frigg, 2006; Giere, 1999).⁸ But the use of models can be helpful in biology, too. There is some precedent for the use of “model organisms,” for instance, where these can either be simple organisms that are taken as models for evolutionary change more broadly, or hypothetical organisms, as in the case of computer simulations of evolutionary change (Godfrey-Smith, 2006).⁹

The benefit of this model-based approach is that it makes scientific questions more tractable, and so scientific research more executable (Giere, 1999). If we take the fitness of a trait, for instance, as the average fitness of organisms having that trait, then we have a clear-cut model for fitness that cuts through much of the extraneous biological noise that might otherwise confuse our findings. The total fitness of individual organisms given their trait complexes, say,

⁸ It is, perhaps, not surprising that physics finds such affinity with model-based analysis, since physics is so well-represented via mathematical tools.

⁹ Dawkins’ algorithm from *The Blind Watchmaker* (1996) is a famous, if simplistic example.

might feature in biological reality somehow, but to measure each of these values one-by-one would be tremendously difficult, and so a model of fitness taken in terms of fitness *averages* is more practically useful. Indeed, according to Sober (2013), “Biologists get along quite well by thinking about the fitnesses of traits that are shared among organisms; the fitness of the total trait complex that a token organism uniquely possesses is typically beyond their ken.

1.1.2 The standard summary approach

It’s worth noting that models of this sort do not seem to constitute the only conceptions of fitness that are of typically interest to biologists. Philosophers of biology have demonstrated that in certain cases, biologists do appear to be interested in the fitness of organisms in a more holistic sense (the sense seemingly rejected as biologically useless by Sober (2013)). This is perhaps most in keeping with the first approach mentioned by Godfrey-Smith (see Millstein, 2009; Brandon & Ramsey, 2007; Pence & Ramsey, 2015). This approach squares especially well with scientific research executed through field observations and the like. For instance, a plant scientist conducting fieldwork on how a particular species of orchid is responding to climate change is going to be more interested in offering a full description of the plant and its response to external conditions than in how her findings model climate change more broadly. Notably, I’m engaged in neither sort of project mentioned by Godfrey-Smith.

1.1.3 Philosophy of science and metaphysics of science

Isn’t there space for an exploration of the central ontological commitments of evolutionary biology that doesn’t take biological practice as an article of central concern? The typical reasons for privileging scientific practice can usually be classified in one of two ways. The first simply

concerns the kinds of research questions that philosophers of science are interested in exploring. Philosophers of science often aren't interested in questions that aren't practically useful to scientific practice, and this, perhaps, generates a certain cognitive bias against philosophy of science that's done without such a strict eye toward the practical benefits of one's philosophical research.¹⁰ But this doesn't do anything at all to show that philosophy of science done with an eye toward ontology and metaphysics should not be done. The second sort of objection one typically finds is broadly historical.

It's risky business, historically, when philosophers of science say that they wish to explore the metaphysical commitments of a theory, and it seems especially so for philosophers of biology. In the history of the natural sciences, it is not uncommon to see opposing views derided as unduly "metaphysical." 18th century Newtonians derided the corpuscularianism of the Cartesians as metaphysical for lack of its predictive capability and its seemingly inflated ontology (to include, it seems, an inflation of causal powers). The Cartesians derided the Newtonians as metaphysical due to their seeming refusal to specify a *mechanism* responsible for gravitational force; thus did they consider the "forces" postulated by the Newtonians as "occult" in character (see Shapin, 1996).

There are similar worries in the historical background of biological science. Worries about "essentialism" in biology, for instance, have occupied many of biology's most prominent figures.¹¹ The "orthodox view," as John Wilkins (2009) holds it, is that thanks to the influence of Platonic and Aristotelian philosophy in the west, most of western thought, scientific or otherwise, has been haunted by the specter of essentialism. But, thanks to Darwin, biology is now framed in terms of "population thinking," which gives conceptual priority to difference and eschews the

¹⁰ For a reasonably good representative of this view, see James McLaurin and Heather Dyke (2012).

¹¹ "Vitalism" presents another prominent example of a concept derided as unduly "metaphysical" by the lights of modern biology. Mayr (1982) discusses this in some detail.

notion that any biological thing is what it is *essentially* or *necessarily* (Mayr, 1982). Rather, the things of biology are what they are *contingently*, and so essences, vital forces, and all other basically “metaphysical” concepts, objects, and qualities have to be left to the side.¹² So it is that this troubled historical relationship between biology and Platonic/Aristotelian metaphysics makes it difficult for philosophers interested in exploring the relationship between biological science and subjects of concern to contemporary metaphysics to justify themselves in doing so.

But of course these traditional concerns from the history of science (and biology in particular) trade on a fairly outmoded conception of what metaphysicians are typically trying to show. Recent work in the philosophy of science (esp. physics) has gone quite some way toward demonstrating the scientific respectability of certain forms of metaphysical inquiry. This work has focused on the nature and existence of certain structural features of scientific theories (in the ontological sense) (Ladyman & Ross, 2007), the extent to which results from “traditional” analytic metaphysics (which is to say, metaphysical work that’s not understood to have been addressing scientific questions directly) can be taken as broadly homologous to the concepts employed by “scientific metaphysics” (French, 2014), and to what extent scientific realism can (or must) be grounded by some manner of metaphysical theory (Chakravartty, 2007; Chakravartty, 2013).¹³ Clearly I disagree with much of the work in this field, as I will no doubt show in Chapter 3 of this thesis. But, in any case, there are clear examples of metaphysical projects regarded as basically methodologically responsible by scientific lights in the domain of physics. No reason has yet been given that would show why philosophy of biology cannot also

¹² I refer the reader back to Wilkins’ (2009) treatment of the history of the species problem. He does a tremendous job of showing why these traditional concerns (to do with essentialism and the like) are really quite misplaced.

¹³ Ladyman, Ross, and Kincaid have a (2015) volume entitled *Scientific Metaphysics* that touches on many of these themes. I refer the reader there. It should be noted that I’m not interested in “scientific metaphysics” as they seem to understand it. That is, I don’t take science as a fundamental constraint on metaphysics. I’m interested instead in how the two may be fruitfully related to one another, however they’re conceived.

target questions of metaphysical concern, so long as the objects of such concern are carefully stated and the subject of investigation is amenable to metaphysical modes of inquiry.¹⁴

I take it that the question of *what fitness is* clearly qualifies under these conditions. There is great merit in investigating which operationalized models of *X* are most useful for scientific practice, but questions of a more explicitly ontological flavor are also important for philosophy of science, even if they're not of interest to practicing scientists as such. Of course, the intersection of metaphysics and science should respect the empirical constraints that the sciences enforce given the evidence on offer. Metaphysical analyses in philosophy of physics and general philosophy of science have helped to clarify the central ontological commitments of scientific theories and the conceptual schemes that typify them, all while respecting the empirical deliverances of the theories they treat. What's sauce for the goose is sauce for the gander, so there's no reason to think that philosophy of biology cannot engage in methodologically responsible metaphysics. This is different, however, than the claim that metaphysical commitments can just be "read-off" of the relevant science. Science constrains the content of metaphysics, but it does not determine it.

1.2 Sketching dispositional realism (and why it matters for biology)

Take any canonical dispositional feature of certain objects—fragility, say—and surely you will find defenders of any one of half a dozen (or more) interpretations of disposition ascriptions like "x is fragile." It is possible, at least, to take the ascription of fragility (or ascriptions rather *like* the ascription of fragility) as denoting some real feature of the object in question. Some form of

¹⁴ It is possible to argue that physics is unique with respect to the applicability of metaphysics because physics is foundational to all other sciences. More of this will be covered in Chapter 3. For now, see Ladyman and Ross (2007, p. 27).

this interpretation is what I aim to defend here. First, however, it is necessary to explain why some of the popular alternative interpretations are inadequate.¹⁵

1.2.1 Some preliminaries and a note on motivations

As something of a preliminary note, I would like to point out that *dispositions*, *propensities*, *capacities*, and *powers* are often treated pretty much interchangeably. Nancy Cartwright (1989; Cartwright & Pemberton, 2013) prefers to speak in terms of capacities. Anjan Chakravartty (2013) and Stephen Mumford (1998) speak in terms of dispositions. E.J. Lowe (2008) prefers to speak in terms of powers. Noting this terminological issue, let it be known that I will be speaking in terms of *dispositions* (although I may slip into one of the alternative vernaculars from time to time), but I understand everything I claim here to apply equally well to talk in terms of *powers*, *propensities* and *capacities*.

The main concern that motivates my realist view of dispositional features is this: the canonical effects issuing from observations of interactions between objects must answer to something *about* the objects under observation.¹⁶ Stated another way, it seems to me implausible that the regularity of certain events (i.e. the “constant conjunction” of specific event types, in Humean terms) can be anything like a brute fact having nothing to do with the properties of the things that stand as participants in the events (see Lowe, 2008). To see why, it helps to use an example. Cartwright and Pemberton (2013) use the simple example of “wind pushing a blowable thing.” Here we witness a change process that takes the starting arrangement of some suitably

¹⁵ One reason, which I don’t mention in detail, is that most of the alternatives lead to some theory of event causation, and E.J. Lowe (2008) has shown that event causal frameworks are flimsier than has been appreciated generally.

¹⁶ Lowe (2008) claims that a robust theory of causation requires a robust account of the causal powers attributable to various substance types. I find that I agree, in general, but I would add that a realist metaphysics of causation requires the further qualification that we need not successfully make such attributions in order to defend the claim that the powers in question do in fact exist.

blowable object (imagine a toy sailboat) and sufficiently gusty conditions, where the sufficiently gusty conditions exercise their power on the blowable object, and the blowable object exercises its power to catch the wind, and so move to another physical arrangement causally downstream from the initial one.

We can imagine more complex examples. While I'm no horologist, I understand that my automatically winding watch is a rather complex mechanical system. Some of the components involved in the successful operation of the watch movement include small jewels that act like ball bearings for the various sliding components; a windable spring mechanism that allows the movement to acquire the necessary energy for starting the movement on its task to tell time reasonably accurately; a separate subroutine of moving parts that accounts for the watch's ability to keep track of the day and date; and a weighted pendulum capable of transferring energy to the spring mechanism so that my body's kinetic movements can wind the watch (saving me the effort of winding it manually each morning). An automatic watch movement is clearly much more intricate than I can adequately communicate here, but this rudimentary description suffices for our purposes here. The success of the watch movement in performing its function answers to the fact that each of the components in the physical arrangement offers the right sort of causal contribution. That is, there are canonical effects issuing from objects, of the relevant types, in the arrangement of parts, and these effects are due to the dispositional properties that we track in and throughout our observations of the world (Cartwright, 1989; Cartwright & Pemberton, 2013).¹⁷

¹⁷ One might hold that it's the categorical rather than the dispositional features of the watch movement that account for its ability to make manifest certain canonical effects. There are, however, good reasons to doubt this. One reason is that dualism with respect to properties (that is, holding that there are *both* dispositional and categorical properties of things) itself seems questionable. For a more thorough discussion of this, see Mumford (1998 pp. 64-91). See also C.B. Martin (1994, pp. 1-8). I take no stance on whether property monism or property dualism is correct. I'm open to the possibility that categorical properties are indispensable to both metaphysics and science. But I am committed to the view that dispositions are importantly causal. For a clear explanation of the central causal role played by dispositional properties in science, see Chakravartty (1998). See also Chakravartty (2007).

So to say that some object (or kind), X , has some disposition, D , is to make a *causal* claim (Mumford, 1998). That is, to say that X is disposed to behave D -ishly is to make a claim about X and its ability to make it such that D -ish things can happen in the world.¹⁸ This is something of a controversial claim, but one that I think is defensible. Alternatively, some have taken a non-causal view of dispositions that treats them as analyzable in terms of conditionals (counterfactual or otherwise). Of course, such treatments need not be non-causal (see Malzkorn, 2000), but for now I focus my attention on non-causal conditional and counterfactual analyses.

1.2.2 Early versions of conditional analysis

Early attempts to conditionalize dispositions deployed the logic of material implication in order to do so.¹⁹ Here, the possession of a disposition D by an object x is just the truth of a conditional of the form “if Fx , then Gx .”²⁰ This approach ran into a number of problems almost immediately.²¹ As an example of such problems, for “if Fx then Gx ,” let F stand for “has been submerged in water,” let G stand for “will dissolve,” let x name some quantity of sugar, and let Dx stand for the truth of the disposition ascription “sugar is soluble,” which is true just in case “if Fx then Gx ” turns out true. Since the falsity of the antecedent entails the truth of the conditional, we have that Dx successfully obtains for any sugar that is never tested (placed in water). The dispositional realist is glad with this half of the claim. The realist, after all, is keen to show that a disposition like solubility is held by its suitable objects even if never manifested. However, in similar fashion to the above, $\sim Dx$ successfully obtains for conditionals of the form “if Fx then $\sim Gx$,” where the sugar is not appropriately tested and $\sim Dx$ obtains just in case the conditional is

¹⁸ I take it that this is broadly consistent with Anjan Chakravartty’s version of dispositional realism in his “Semirealism” paper (1998). See also Cartwright (2015).

¹⁹ See Ryle (1949) for a paradigm example.

²⁰ See Mumford (1998, pp. 45-50).

²¹ Mumford (1998, pp. 50-62) gives a nice historical sketch of these problems.

true. So, according to Carnap (1936), we have it that for objects never tested, both Dx and $\sim Dx$. This is problematic (clearly).

1.2.3 Contemporary treatments: counterfactual and modal

Perhaps some other manner of reducing dispositions to conditionals avoids those objections raised against indicative conditionals. If dispositions are basically causal, as Mumford claims (and as I'm claiming), then perhaps it is possible to embed dispositions in counterfactual conditionals and take this as the proper analysis of disposition ascriptions. This approach would not fall prey to the objections leveled against conditionals cast in the mode of material implication, since counterfactuals state what *would* be the case rather than what *is* the case, placing problems related to untested dispositions safely to the side.²²

So, instead of analyzing solubility as a conditional of the form, "If the sugar cube is placed in water, then it will dissolve," the counterfactual construal of dispositions will give us the form, "Were the sugar cube placed in water, then it would dissolve." Clearly, counterfactuals can't fall prey to the objections leveled against indicative conditionals, since here the analysis already entails that the antecedent is false (that is, we take for granted that the sugar cube has *not* been submerged in water) (Prior, Pargetter & Jackson, 1982). Untested dispositions are no problem for a counterfactual—none of the objects embedded in such subjunctives are supposed to have been tested.

An immediate objection arises. We would not typically want to say that an object no longer possesses its disposition D in the very process of D -ing (Mumford, 1999; Mellor, 1974). Returning to the example of solubility, if we analyze the solubility of sugar in water as the

²² See Loux (2006, pp. 195-203) for a nice overview discussion on conditional accounts of causation and the motivations underlying such accounts.

counterfactual claim, “Were the specified quantity of sugar placed in water, then it would dissolve,” then we’re seemingly forced into a position of endorsing the rather odd claim that the sugar does not have the dispositional feature of solubility in such instances as it is currently manifesting solubility (Mumford, 1998, p. 6).

Consider instead the disposition *x is fragile*. If we say that fragile things break when (suitably) dropped, and then analyze this in terms of the counterfactual claim, “Were *x* dropped, then *x* would break,” the implication is that an object that is *now* breaking is *not* fragile (Mellor, 1974, p. 169). It can be legitimately demanded, however, that the counterfactualist explain just *when* in the process of breaking it is that the disposition is discharged. Is it just when at least 51 percent of the objects matter has broken? Perhaps so, but then it seems that fragility is retained in at least some substantial portion of the total duration of the breakage event.

Perhaps the above objection can be circumvented by recourse to some manner of modal analysis of counterfactuals and causation. If we can grant some version of counterpart theory as the right mode of analysis (whether concrete or abstract), then we can say that for any object *x* and any disposition *D*, *Dx* can be analyzed as the counterfactual claim that “Were *x* to *F*, then *x* would *D*,” and argue further that present manifestations are no problem because for any *x* there will, in some nearby possible world, be some counterpart *x** for which it is not the case that *D* is being presently manifested. An in-depth investigation into the philosophical controversies over counterpart theory is beyond the scope of this project. For our purposes, however, I take it that Jonathan Jacobs (2010) and Alexander Pruss (2002) have argued fairly convincingly that counterpart theory (concrete or otherwise) introduces unnecessary elements into our ontology (see also Merricks, 2003).²³ As Jacobs points out, the piece of counterpart theory that seems

²³ Merricks’ paper is mainly a destructive assault on counterpart theory. Jacobs (2010) offers a nice constructive continuation of Merricks.

relevant for analyzing properties and dispositions is the fact that some object x is in some important sense similar in its properties and capacities to a counterpart x^* .²⁴ But this kind of an analysis doesn't seem to require counterparts at all. We usually have no issue grounding similarity relations in our normal observations of the world, and indeed, much of natural science depends on our ability to do precisely that. This, according to Jacobs and Pruss, seemingly obviates any need for recourse to counterparts and the worlds they inhabit.²⁵

1.2.4 Modalized conditionals as a mode of conceptual analysis

It may be the case that (non counterpart-theoretic) modal treatments of conditionalized dispositions can work as a mode of conceptual analysis. The value of such projects is broadly heuristic. Malzkorn (2000), for instance, argues that there need be no tension between conditional treatments of dispositions and realist attitudes toward dispositions. One can be fully consistent in holding that dispositions are real, causal, and that they aren't fully captured by any formal semantic treatment (whether because of interfering conditions in the world, "finkishness," or whatever), and still hold that conditional analysis is useful for clarifying concepts and building conceptual frameworks. I find myself in agreement with this attitude.

Jacobs (2010, pp. 243-6), for instance, develops a fairly robust modal semantics for dispositions and powers cast as counterfactual conditionals. Clearly Jacobs is a realist about dispositions and their causal roles; that's the entire point of his argument! But still, having a nice

²⁴ Carwright (2015) makes a similar claim, where she argues we should favor a "this worldly" metaphysics where such this worldliness suffices for our explanatory purposes. Her analysis, though, extends even to "Laws of Nature," not just worlds populated by counterparts.

²⁵ Jacobs says, for instance, that my having the property of possibly being a truck driver is grounded in my physiological properties (being tall enough, having enough upper-body strength, sufficiently good eyesight, etc.) and also the fact that individuals relevantly like me *in this world* are able to drive trucks. For Pruss's part, he argues that to possibly have a certain property or to have the property of possibly bringing about (or initiating) some causal process is grounded in the actual world being such that such properties and causal processes are instanced in that world (and, presumably, being the sort of thing such that you can bring about or make manifest such an instancing).

semantic scheme is helpful for clarifying the concepts and sentences we use for talking about this sort of thing. This need not be a problem for dispositional realism.

But we shouldn't let ourselves slide into thinking that conditional analyses of dispositions (modal or otherwise) are anything *other* than conceptual analyses. A conceptual reduction of dispositions to counterfactual conditionals is not a metaphysical reduction of dispositions to counterfactual conditionals. And indeed, I don't think there's much hope for a metaphysical reduction of powers to any sort of conditional treatment, no matter how robust the semantics—and the examples that I draw from the natural sciences should help in demonstrating why that is. In short, nature doesn't operate according to semantic rules.

1.2.5 Quinean Mechanisms

It may be possible, in Quinean fashion, to take disposition talk as just short for mechanistic descriptions that haven't yet been made. For Quine, a mechanistic description is one that explicates natural operations in terms of microphysical parts and their causal interactions. The slogan version of Quine's position is that there's no change "without microphysical change" (Quine, 1981, p. 98). So perhaps disposition talk mainly issues from certain epistemic limitations since some such mechanisms may be difficult to explicate; but we need not lose heart since these limitations will eventually be overcome as science progresses (Quine, 1960). Mumford (1998) recognizes that we are not generally aware of the relevant mechanisms responsible for the dispositional behaviors of each class of objects under investigation, but Quine doesn't take this as a problem. The repeated success of scientific explanation offers a sufficiently good "promissory note" to the effect that we ought to be confident that a precise scientific (read: mechanistic) explanation is in principle possible, and that the relevant mechanisms are present

throughout all disposition manifestations.²⁶ If that's right, then perhaps we ought not be realists about dispositions so much as realists about mechanisms.

1.2.6 Dispositions in physical science

But is it true that scientific explanations are only mechanistic explanations in the Quinean sense? Do explanations in terms of dispositions not figure in scientific explanation? It seems that dispositions do, in fact, figure in scientific explanation, and fairly prominently at that. Ian Thompson (1988) argues that scientific explanation is often concerned not just with predictions and explanations concerning what *will* happen, but also what *may* happen, even if the conditional probability of the realization of certain physically possible scenarios is quite small.²⁷ This is especially true in various engineering contexts (see Mellor, 1974). There's good reason, also, to think that quantum systems display irreducibly dispositional properties. Thompson (1988) explains that position and velocity are "not continuously definite, but only have specific values in suitable situations such as measurement interactions of certain kinds." This suggests a dispositional interpretation of quantum mechanics, and further, if we understand the relational (and statistical) features of quantum mechanics ontologically, then we're committed to a fairly robust realism concerning dispositions (Thompson, 1988; Popper, 1959; Suarez, 2007). If we take seriously the notion of modal and statistical facts as parts of the world's ontology (and so not artifacts of epistemic limitations), then dispositions are part of the world, and feature as chief components of our causal explanations (Cartwright, 1989; Mumford, 1998; Molnar, 2003).

Indeed, the capacities of objects to make manifest certain effects in the world seems to square

²⁶ An argument like the pessimistic meta-induction may apply here, too. That is, it may be that arguments for scientific "promissory notes" may be dismissed on the grounds that future explanatory schemes may overturn our present ones, and we have no real idea of what such explanatory schemes might look like.

²⁷ Even prohibitively small conditional probabilities are non-zero probabilities, and so should count in the dispositional profile of the phenomenon in question.

better as an explanation of the world's operations than any thoroughly mechanistic (or rigidly deterministic) explanation—and this is so even with respect to scientific practice.²⁸ In many cases (as with quantum systems) there is precious little in the way of empirical support for the notion that the fundamental operations of nature are law-like, strictly mechanical, or rigidly deterministic.²⁹ Instead, the world usually displays some calculable measure of regularity, and so we say that when certain conditions obtain, then certain specified effects are probabilified to degree x .³⁰ Dispositions offer only as much metaphysics as is needed to make sense of natural science, and no more (see Cartwright, 2015).

1.2.7 Dispositions in the life sciences

But what about biology? Can the life sciences countenance the same manner of support for dispositions as the physical sciences? It is my position that they can (and must). Richard Boyd (1999) suggests that systematic biology depends, at least in part, on organizing groups based not just on properties as such, but on *causal* properties. That is, at least part of a particular conceptual organization of the biological world is due to the fact that a class of objects under inspection stands as witness to the same set of causal relations.³¹ Boyd's analysis does not make a clear invocation of dispositions, but his argument is still relevant here. Boyd is clearly

²⁸ Cartwright (1989) provides some nice examples of the practical usefulness of capacities and dispositions with respect to econometrics and theoretical treatments of superconductivity. While I don't wish to use the practical success of such analyses as a key justification for my own arguments, I do think the fact that such analyses do feature prominently in real-world scientific contexts should help to allay any fears that might cause someone to think that what I'm doing should be held in suspicion.

²⁹ Cartwright (2015) argues that while we have some support for the notion that certain engineering contexts, and perhaps some subset of natural scientific contexts appear deterministic, it's hardly reasonable to extrapolate from these limited cases and conclude that *all* natural scientific contexts are deterministic. Stuart Kauffman (2013) provides good reasons to think that global determinism is untenable precisely because of what we know about how biological processes work. See also Philip Clayton (2013)

³⁰ Mumford (1998) argues that an interpretation of disposition manifestations as merely probabilified to some degree is perhaps the best candidate interpretation given that (as realists, anyway) we want to be able to say that things can be *D*-ishly disposed even if *D* is never manifested by *X*.

³¹ This is not meant in the same sense as something like a causal theory of semantics (see Sider, 2012).

committed to the notion that causal properties (at least in part) account for our ability to taxonomize biological entities. My argument goes only one step further in claiming that *dispositions* are the causal properties we're after. None of this requires a slide into some objectionable form of essentialism, though. In systematics, for instance, biological properties can, in effect, be organized into equivalence classes insofar as the properties of biological types are sufficiently similar. The best test for sufficiency is to be understood in terms of similar manifestations of effects (which answer to dispositions).

Outside of systematics, dispositional analysis seems to feature fairly prominently in functional morphology. The evolution of dark skin in humans, for instance, is plausibly linked to folic acid loss in populations exposed to more hours of direct sunlight (Jablonski, 2002). Higher UV exposure inhibits the body's ability to properly metabolize folic acid, resulting in increased instances of neural tube deficiency in infants. Darker skin, however, acts as a prophylactic to the effects of UV exposure, lending support to the notion that there was a selective regime favoring darker skin in humans. But biological systems are notoriously noisy, perhaps—much like other natural phenomena—supporting an ontological interpretation of statistically driven dispositions. In that case it should not be understood that increased melanization can be reasonably thought of as a determinate outcome, but instead as a disposition, the manifestation of which generates a selective signal in evolutionary history. Dispositions in evolutionary biology will receive fuller treatment in the next section, but for now I think these examples are sufficient to show that there's little reason to think that dispositions do not feature regularly in the life sciences, just as they appear to do in the physical sciences.

1.2.8 Summing up

Dispositional realism, then, appears well supported and well motivated. Against analyses of disposition ascriptions in terms of conditionals and against explaining away dispositions with successful scientific descriptions (that is, *mechanistic* descriptions), dispositions can be reasonably argued for as real features of the world's ontology. Both the physical and biological sciences seem to appeal to explanations taken in terms of dispositions, and so there's little reason to think them undesirable by scientific lights.

1.3 Dispositions and the causal ground of evolution

I argued in the conclusion of (§1.2) that dispositional realism need not be thought of as undesirable by scientific lights, and that explanation via dispositions is actually quite important to the natural sciences (to include the life sciences). I now wish to focus on evolutionary biology more explicitly, and show how dispositional realism helps to explain change in biological populations over time. I take the somewhat controversial view that it is the fitnesses of whole organisms (not their traits) and so individuals (not populations) that figure in the causal ground of population dynamics.³²

1.3.1 Holistic vs. trait fitness

We saw earlier that Sober (2013) takes the fitnesses of individuals—understood as the fitness values of total trait complexes—as causally impotent. It seems that he means one of two things by this (or perhaps he means both, strictly speaking, but understands that each of the objections

³² I'm not sure how controversial this claim is insofar as the claim is understood in the ontological sense that I defend here. The overwhelming majority of the literature focuses on the practical use of population concepts vs. concepts privileging individuals, so certain in that context my claims are controversial—but I'm not operating in that context.

applies only in certain contexts). On the one hand, he seems to have concerns about how to operationalize a holistic conception of fitness. As he puts it, biologists may be interested in the fitness of dorsal fins, but they're surely not interested in the fitness of "Charlie the Tuna."³³ He seems to mean that measuring or estimating the fitness value of Charlie's total trait complex is too difficult and time consuming to cut any real ice against scientific problems. Focusing on dorsal fins and operationalizing dorsal fin fitness in terms of the average fitness of all the creatures sharing that trait offers the opportunity of a tractable research endeavor, and so this conception of fitness ought to be favored over the other, more holistic conception.

But Sober (1984) also makes it sound as though the claim that holistic fitness is "causally impotent" is to be taken more or less at face value. In explaining the tendency of a population toward an equilibrium frequency, for instance, he claims that it is *trait* fitnesses that explain the frequency of alleles in a population, and not anything else.³⁴ My own view can tolerate the claim of causal impotence if understood in the former, more practical sense (I've already noted that I mean to bracket questions about biological practice, *per se*). But the claim that the total fitnesses of individuals *really don't* figure in the causal foundations of evolutionary change seems too quick.

1.3.2 Holistic fitness as causal

Recall Ernst Mayr's (1982) claim that part of Darwin's genius is that he introduced—or at least settled the conditions of the possibility for—"population thinking." The importance of population thinking is supposed to be that it privileges the notion of differences between

³³ Pence and Ramsey (2015) point out that John Endler's (1986) survey of methods in evolutionary biology casts serious doubt on the claim that biologists don't care about the fitness of "Charlie the Tuna."

³⁴ This claim might be understood in a less literal sense, where traits are taken to *model* fitnesses of genotypes in a population, for instance. Nothing that Sober (1984) says seems to prohibit a more explicitly literal understanding, and so I employ such an understanding here.

organisms not just in interspecific contexts, but also with respect to intraspecific contexts. That is to say, one of the key drivers of change *within* a population is due to the manifest differences between members of the *same* species population.³⁵

But what sort of differences? It helps to revisit R.C. Lewontin's (1970) classic "Units of Selection" paper and the three principles of selection that he lists:

- (1) Different individuals in a population have different morphologies, physiologies and behaviors.
- (2) Different phenotypes have different rates of survival and reproduction in different environments.
- (3) There is a correlation between parents and offspring in the contribution of each to future generations.

Without denying any of the importance of (3), I focus here on (1) and (2). There is a clear sense in which individual fitnesses, taken in the holistic sense, are important for any robust account of why conditions (1) and (2) can be said to hold generally. Take, for instance, the claim in (2) that different phenotypes experience different success rates depending on the environment. A similar claim can be made with respect to the fitness values of individual traits. That is, if organisms are to be properly understood as trait complexes, then it cannot be the case that the measurement of some trait T_1 can be a sufficiently good proxy *in general* for what fitness is and what fitness values can explain more broadly, and this is because the actual benefits that T_1 can confer upon an organism must account for T_1 's broad-based interactions with other traits in the organisms's trait complex.

The facts about an individual's morphology and physiology have deep consequences for how the fitnesses of individual traits can figure in the reproductive success of said individual.

³⁵ This shouldn't be too controversial, since competition between conspecifics, for instance, is quite commonplace.

Consider the effects of pleiotropy.³⁶ If we account for the fitness of some trait T_1 in terms of the average fitness of individuals having that trait, then we might think that we can generate certain predictions concerning a possible optimization process on T_1 , for instance. But it may be the case that some step in the optimization of T_1 requires structural changes in a cis-regulatory region controlling the expression of genes coding the proteins associated with the structures involved in T_1 (see Stern, 2010). But in instances where regulatory changes such as this occur near the beginning of a developmental sequence, the effects will likely be pleiotropic, and likely detrimental overall because changes at early steps in a developmental sequence usually morphologically catastrophic. Similarly, certain traits in the trait complex of an organism may have epistatic³⁷ effects on other traits, resulting in fitness values of that may be less than the sum of the values added together as discrete values (Stern, 2010).

In the same way that Darwinian fitness can only be interpreted against an ecological backdrop, the fitness of individual traits can only really be understood once we've considered the trait's interactions with the rest of the trait complex in which it's embedded. Biological traits, after all, are situated in relational contexts. Organic bodies are relational complexes such that the traits they exemplify can see their typical effects influenced by other biological traits in important ways.³⁸ Since it is the individuals, as trait complexes, that either will or will not contribute progeny to future generations (and since the likelihood of doing so is a function of the conditional probability of survivorship to viability, among other things), it certainly seems as though individuals are (at least part of) the causal basis of population change.

³⁶ Pleiotropy occurs when changes to one genetic element result in more than one change to an organism's phenotype.

³⁷ Epistasis occurs when one gene suppresses the typical effects of another gene. One gene is often said to "modify the gene whose effects it suppresses."

³⁸ See Stern (2010pp. 79-101). See also Alex Rosenberg (2006).

1.3.3 A semi-formal gloss on fitness as a disposition

Individual fitnesses, then, express something like a disposition, where we say that an organism O_1 is fitter than organism O_2 if O_1 is better disposed to leaving offspring for future generations.³⁹ Call this disposition F . F is dependent upon the causal contributions of dispositions associated with individual traits without being reducible to any of the fitness values of any of the traits taken individually. Call each of these dispositions associated with individual traits f_n , where n is some natural number that picks out an individual trait in the total trait complex of some organism O . So, F = the causal potency conferred by the interactions of (f_1, \dots, f_n) in the trait complex of O . The causal potency of F should not be understood as the raw computation of the average fitnesses for each of the traits, added together one-by-one. As mentioned already, pleiotropy and epistasis make it such that the fitness of a trait complex cannot be so computed. Instead, for “sum,” read something like the statement “total account of the causal interactions between each of the little f -es.”

But if F is accounted for in terms of the total causal interactions between (f_1, \dots, f_n) , then why is it not the case that the individual traits, and their fitnesses, f_n , account for the causal basis of population change? The answer is that there are no traits in biology, only trait complexes. Contra Sober (1984; 2001; 2013), it looks to me as though careful analysis (as in the above) puts traits in the backseat, causally speaking. Since traits are always embedded in trait complexes, and the interactions of traits in complexes will determine (or at least influence) the causal contribution of any particular trait, it seems there is no way to interpret the causal contribution of any particular biological trait outside of its participation in a trait complex. Traits may have some causal power, but they don’t do much on their own. The f_1 of T_1 is dependent on the causal power

³⁹ This reflects something like the pairwise comparisons of Bouchard and Rosenberg (2004). It’s certainly true that fitness is comparative, whether or not the comparative nature of fitness is best understood in terms of pairwise comparisons of organisms (see Millstein, 2006; Millstein, forthcoming).

of the big disposition F , but F is not symmetrically dependent the causal influence of any particular f_i .

1.3.4 Connecting back to dispositional realism

Clearly F is relational, just as Darwinian ecological fitnesses are relational. But what does this have to do with realism about dispositions? The main reason dispositional *realism* ought to be brought to bear on questions of fitness (and evolution more broadly) can be illustrated in terms of the nature of F . Recall that F cannot be cast in terms of the arithmetic sum of (f_1, \dots, f_n) , since epistatic effects, for instance, can make it such that the fitness of two traits taken together is less than the arithmetic sum of their fitness values understood discretely (Stern, 2010). It seems to me a live option that the specific causal interactions that would compute a precise specification of F are inaccessible to us, given that biological systems are famously noisy, and given that the relevant causal interactions are web-like and bushy. We may write promissory notes to ourselves to the effect that we will one day understand the full causal profile of some trait complex, but I remain skeptical. At any rate, it seems to me that the issuance of effects from the causal contribution of objects is best accounted for by a realist understanding of dispositions, where disposition ascriptions are true of their objects at all times (Mumford, 1998). It is not the case, for instance, that DNA was only disposed to leave a certain refraction pattern *after* x-rays had been refracted off of DNA molecules. Rather, DNA was always so disposed toward refracting x-rays in a certain pattern, but it wasn't until the 1960s that anyone did so. Likewise, DNA would have been so disposed even if x-ray technology had never been discovered, and so even if no ascription of such a disposition could ever have been made.

Likewise with F . That is, even if it were the case that a full description of the causal interactions encoded by (f_1, \dots, f_n) is not possible even in principle, the causal contributions and the dispositions they issue from are still very real and very present. I take it that dispositions, if actual, should not be so only insofar as some ascription of the disposition has been successfully made. Rather, dispositions, if actual, are actual always, and so the possession of them is not subject to our cognitive limitations.⁴⁰

1.3.5 What about populations?

But even if one grants me that it is fitness in this holistic sense and not trait fitness that accounts for the causal basis of population change, one might still wonder whether it is not features of populations themselves that properly account for the causal basis of population change, and not individuals. Millstein (2006) argues that populations are the right unit of analysis for tracking causal change in evolution. Godfrey-Smith (2009) seems to concur. Millstein (2006) argues that evolution is indeed a causal theory, and that selection (and fitness)⁴¹ count as causal components of the theory of evolution. But, she claims, these are best taken as population-level features; and so it is populations that constitute the real meat of a causal theory of evolution.

She argues that frequency-dependent selection and density-dependent selection are good examples of population-level features that influence the direction of population change.⁴²

Mimicry, for instance, is only a successful strategy for avoiding predation if it is not too widespread in a population. So, if a few butterflies mimic the coloring patterns of another

⁴⁰ Also, clearly, it may be the case that we fail to deliver accurate ascriptions of dispositions. The ascription of a disposition is no guarantee of the truth of the ascription, just as the actuality of the disposition does not depend on anything about our cognitive practices.

⁴¹ Millstein (2006) avoids talking about fitness directly. But much of the contents of her paper are certainly relevant to discussions of fitness.

⁴² See also Stern (2010) for a discussion of the epistatic effects of certain population features.

poisonous type that predators tend to avoid, then the few that have developed that trait will likely avoid predation somewhat successfully. But if too many within the population follow suit, the immunity to predation is eroded, since it is no longer possible for the predators to target certain coloring patterns and avoid the others (they must eat, after all). Thus Millstein claims that we already have good precedent for thinking that population-level features are important in evolutionary biology, so we can't reject *a priori* the notion that it is populations themselves, and not individuals, that undergo selection.

This seems plausible at first blush. If fitness is understood as a disposition toward the successful production of offspring for future generations, then perhaps we should focus our analysis on those features of populations that make it so that individuals are more likely to successfully reproduce. The list of such possible features is difficult to exhaust, but plausibly includes facts about reproduction schedules (see Godfrey-Smith 2009; Ariew & Lewontin, 2004), the degree of overlap between generations, certain behavior dynamics between conspecifics (e.g. whether “communal parenting” is typical of a population), and environmental facts about predation and the like (and specifically how this bears on likelihood of maturing to viability) (see Brandon & Ramsey, 2007). While I don't want argue that the population-level features that Millstein identifies do not offer important causal contributions to the dynamics of population change, I do want to note that populations should not be considered as the causal *ground* of population change. The causal ground, rather, is found in biological individuals.

1.3.6 An analog disposition for populations

To see why this is so, recall that the disposition F of a biological individual is expressed by the causal interactions between (f_1, \dots, f_n) , where each f_i is the fitness value for the traits of an

organism O . F , I argued, does not depend on the value of any particular f_i , since the contributions of each f_i is relative to the rest of the trait complex. The same goes for populations. That is, we can suppose that there exists some disposition, like F , except that it applies to populations instead of individuals. This would cast biological individuals as the rough equivalents of the f -es in the analysis of F .

It might be argued that I've just shot my own argument in the figurative foot. If biological individuals can be cast as broadly analogous to the f -es, then it looks like we have to say that populations *are* causally foundational since we already claimed (with respect to individuals) that the value of F does not depend on (nor is it analyzable in terms of) the f values. But if there's some disposition like F for populations, surely it does some of the work of mapping the trajectory of population change, and so the natural conclusion is that populations *should* be understood as the primary unit of analysis when considering the causal foundations of evolution.

It would be a mistake to suppose this. In the case of biological individuals, the disposition F does not significantly depend on the discrete values of the f -es, but it is still the causal *interactions* of the f -es that give F whatever causal potency it has. F cannot be reduced to particular f values because of the manner in which the traits denoted by f values interact with one another. The interactions are complex, relational, and can render certain causal effects suppressed or promoted, however the case might have it. The case is similar, I argue, with respect to populations and individuals. Serial epistatic effects, for instance, can occur at the level of populations, such that the selective advantage of some trait might score lower than it otherwise would have due to the countervailing force of certain other extant traits in a population (Stern, 2010, pp. 94-95).

But just as in the case with individuals and their traits, the relevant population-level disposition cannot be cashed out in terms of individuals not because it is not the individuals that are doing the causal work, but rather because the relevant interactions are quite complex. Those favoring a population-centric view of the causal process of evolution are perhaps warranted in claiming that populations offer the best epistemic guide to change processes in evolution, since it is plausibly the case that such change processes are more immediately apparent, empirically speaking. Millstein (2006) seems to support something just like this notion that populations constitute an epistemic guide when she discusses Rosenberg and Bouchard's idea that fitness is best analyzed in terms of relative pairwise comparisons of organisms in a population. To actually carry such a procedure is practically impossible to be sure (unless you have a *very* small population). Better to focus on populations, says Millstein, since at the population level we can gain solid empirical and epistemic traction.

But of course I can grant this position and consistently hold that the real causal work (and so the real explanatory work) is being done at the level of biological individuals. Before a population can be disposed toward evolving in some particular way it must be the case that individuals are disposed to occupy the functional roles that make possible the realization of the relevant population-level feature. Even if we can grant that the functional space provided by higher-level biological objects (populations, local ecologies, etc.) is somehow *analytically* prior to the individuals that realize them⁴³—and so make possible the truthful ascriptions of certain attendant higher-level dispositions—it would be odd to claim that the higher-level objects are *causally* prior. This, it seems, would require something of a move toward teleological reasoning

⁴³ See, for instance, Trevor Pearce (2010) Pearce understands Darwin to have seen something like a notion of analytic priority with respect to the “places” in nature's economy, where the places are already fixed elements of the ecological background. While this explains the conceptual space of possibilities for an increasingly diversified ecological system, the economy of nature should not, I take it, be interpreted as having any fundamental causal power. Causal powers are the province of organisms, not the conceptual space they occupy.

in biological domains, and so is probably best rejected. Returning to the example of density-dependent selection, it would be odd to claim that certain density-dependent population features are evolutionarily advantageous, and so cause individuals to evolve in such a way as to make manifest that population feature. We should instead say that certain population-level features are evolutionarily advantageous (perhaps for both populations *and* individuals), and then turn our attention to those lower-level biological processes that make possible their realization. In the case of mimicry, we should not say that facts about population density explain the emergence of a propensity to evolve certain color characteristics. A population-level analysis would at best explain *why* we take it to be the case that the selective advantage conferred by certain mimetic abilities is dependent on facts about population dependency. But this is a non-causal analysis. Biological individuals and their attendant dispositions do the causal work.

1.4 Conclusion

In this chapter I argued for a realist construal of dispositions as causally active in evolutionary biology, specifically with respect to fitness. I first sketched a realist account of dispositions in general, and then showed why such an account is of the right sort for fitness as a causal component of evolutionary change. The account of fitness that I argued for is one that takes the holistic fitnesses of biological individuals as central to the causal framework of evolutionary biology. This is against accounts that would take populations and population structures as the main unit of analysis for a causal account of evolutionary change.

In the next chapter I defend dispositional realism with respect to evolvability. In the same way that I've argued for fitness as a real property of organisms, the close examination of which

assists us in clarifying our understanding of causal processes in population change, I argue that evolvability is a real property of organisms that plays an analogous explanatory role.

2. EVOLVABILITY: A FURTHER CASE STUDY FOR DISPOSITIONAL REALISM

In the previous chapter I argued that a realist stance toward dispositions in biology helps us to make sense of evolutionary change. This is especially the case, I argued, with respect to fitness. But the explanatory power of dispositional realism is not specific to fitness. In this chapter I will argue that a similar case can be made for dispositional realism as regards *evolvability*.⁴⁴ In this chapter I will argue, indeed, that dispositional realism is the best stance to take toward evolvability as a property of organisms, and that this counts as further evidence in support of dispositions as causally central to the life sciences.⁴⁵

The structure of this chapter will be as follows: in (§2.1) I will review some of the scientific and philosophical literature on evolvability. This is mainly in order to gain some descriptive and analytical clarity with respect to what evolvability is, since the literature on it is young and different treatments often appear mutually incompatible. Having gone over some of the literature on evolvability and its various construals in (§2.1), I will spend (§2.2) developing and defending a functionalist conception of evolvability, casting it as a multiply realizable dispositional property of organisms (and possibly populations, too). In (§2.3) I deal with some objections. Some will object to the idea that evolvability can be treated as a distinct phenomenon in its own right, while others might object to the functionalist interpretation that I aim to give. I will treat each sort of objection in turn. In (§2.4) I offer some concluding remarks.

⁴⁴ I'll be referring to Massimo Pigliucci's (2008) review piece quite a bit in this chapter. For a thorough review of competing views on evolvability, I refer the reader to him.

⁴⁵ This is to say that chapters one and two gesture toward a cumulative case in favor of dispositional realism.

2.1 Evolvability: the lay of the land

It is generally agreed that evolvability—however else one might wish to think about it—is concerned with explicating that property of organisms and populations that makes them *more likely* to evolve (Brown 2014; Pigliucci 2008; Wagner & Draghi 2010). It may be comparatively easy for chimpanzees as opposed to macaques, for instance, to evolve the necessary musculature structure for bipedal locomotion, and this will have to do with certain properties that chimpanzees have and macaques lack. That said, there is not, as of now, a widely accepted, general account of what precisely evolvability *is*.⁴⁶ In general, the debate seems to have something to do with what we ought to identify as the right unit of analysis with respect to evolvability, and not so much a debate over the definition of the concept as such (Pigliucci 2008, 76-77). It is worth noting, though, that the relevant property is taken to be largely *independent* of natural selection (and so plausibly independent of fitness and the like). That is, when it is claimed that an organismal type, or population, or species is highly *evolvable*, this is supposed to be a substantially different question than, say, whether a type (etc.) has been acted upon by selective forces.⁴⁷ Usually evolvability is understood as having to do (at least primarily) with the ease with which an organism can move through morphological or phenotypic space, and clearly this need not have anything to do with fitness. Returning to the example above, it may be that chimpanzees are more evolvable than macaques as regards bipedalism, but that can be true even if selection has never worked on chimpanzees in such a way as to make that novelty appear.

⁴⁶ See Pigliucci (2008) and Wagner & Draghi (2010). It's interesting to note that the authors writing on this subject all seem to recognize both some measure of disagreement and some manner of core conceptual similarity.

⁴⁷ There's some disagreement over what, exactly, this should be taken to mean (contrast Brown (2014), Love (2003), and Pigliucci (2008)), but it's generally agreed that evolvability concepts *at least* serve as something like special conditions that selective powers can manipulate. Clearly, then, pretty much everyone agrees that evolvability is different than fitness and the like.

2.1.1 Genetics: the G and M matrices

If it's generally agreed, then, that evolvability is concerned with identifying and analyzing which properties make it more likely that a type should evolve, what exactly is there to disagree about? Pigliucci (2008) identifies a few strains of disagreement. A particularly noteworthy source of disagreement is centered on whether evolvability, as a property, can be related to population genetics in such a way as to make transitions from standard selection-based analysis of populations to analysis of their evolvability more or less seamless. That is, there are some that argue evolvability requires only a relatively minor expansion of scope with respect to the questions that contemporary quantitative methods in population genetics aims to handle. The semi-classical conception of evolvability is due to R.A. Fisher (1930) and his introduction of *standing genetic variation* as an evolution-conducive property of populations. Here the notion is just that more variation within the genetic makeup of a population will make it such that that population is more responsive to selection pressures where they arise. Note that standing variation is understood to make populations more *responsive* to selection, meaning that standing variation in a population is in some sense *independent* of selection effects. Somewhat more recently, biologists have developed more precise methods for modeling the change in standing genetic variation within populations by use of the G-matrix formalization (Roff 2000; Wagner & Draghi 2010). Avoiding as many of the technical details as possible (since the G-matrix formalization is not really the topic of this chapter), the G-matrix is a multivariate regression model that predicts the amount of additive variation that a population can be expected to display given changes in mutation rate, gene recombination rate, migration rate, selection intensity, and the like.⁴⁸

⁴⁸ This will obviously involve some manner of idealization, since it's probably not possible to rule out all of the signal confounding effects that are likely to occur in the complex interactions of biological components. But even

Moving a bit past this semi-classical, neo-Fisherian method for representing evolvability, biologists have tried to use similar methods for modeling *variability* in populations (Wagner & Draghi 2010). Whereas standing variation is a measure of the available genetic variation for selection to work on, variability is understood to express the *ability* to vary. Formally, variability is “the amount of additive genetic variation created for a trait by a mutation [within] a generation” (Lynch & Walsh 1998). That is, regression models (here using the “M-matrix,” which uses different input variables than the G-matrix mentioned above) employed for the prediction of variability values are attempting to correctly predict the amount of variation that a particular mutation will create with respect to the trait associated with the mutated gene. We can imagine, for instance, a particular gene that’s thought to be associated with height. If there were a genetic mutation in a particular generation that caused a duplication of that gene, and later the expression of that duplicate contributed to increased variation in height, we would say that the mutation accounted for a positive contribution to the variability of the population.

2.1.2 Beyond the G and M matrices

Recently, the study of evolvability has grown beyond analysis of variation and variability and the regression models deployed in their representation and analysis. Wagner and Altenberg (1996) provide an analysis of what they call “the representation problem.” The representation problem has to do with the $G \rightarrow P$ (Genotype \rightarrow Phenotype) map.⁴⁹ Here researchers are interested in the evolution of complex systems (often computer-based), and more specifically the dependence of

so, these models are pretty fine-grained, and so warrant some epistemic respect (see Griswold, 2006). For an in-depth critical review of **G** and its uses, see Pigliucci and Kaplan (2010), chapter 4.

⁴⁹ The genotype is just the genetic constitution of an organism. The phenotype is the set of observable traits or characteristics of an organism. The $G \rightarrow P$ map is basically a schematic that attempts to analyze how changes in the genotype map to changes in the phenotype.

evolvability on the way genetic variation maps onto phenotypic variation.⁵⁰ In order for a complex system (like an organism) to be considered properly evolvable, it must be the case (according to Wagner and Altenberg)⁵¹ that genetic variation sometimes results in phenotypic improvement. A proper account of how this occurs requires a deeper understanding of the structural and functional architecture that characterizes the relationship between genotype and phenotype (Wagner & Altenberg 1996; Pigliucci 2008).

Two major research themes in the more recent studies of evolvability have been *robustness* and *modularity*. Robustness is typically linked to neutralist theories of evolution.⁵² The idea is just that mutations can accumulate in certain ‘neutral spaces’ in the G→P map. Mutations occurring in the neutral spaces of the map have no effect on the phenotype, allowing organisms to accumulate mutational variants of genes (often through gene duplication) for selection to work on at a later time, once the neutral region becomes expressed (perhaps as a result of a change to a regulatory section of the genome), and thus making the organism more evolvable (see Stern 2010).⁵³

Modularity is usually linked specifically to developmental pathways in organisms. A modular organism is one that is organized in such a way that different parts of its developmental architecture are (at least partially) independent of one another. A relatively simple example

⁵⁰ For a lucid explanation of this “representation problem,” see Wagner and Altenberg (1996). Pigliucci (2010) is a good source, too, this time in the context of “phenotypic plasticity.” The problem, broadly speaking, is just dealing with questions of how we can make sense of the claim that genetic information “translates” to trait features manifest in the phenotype. The correlation is strong, and we’ve known this at least since the Modern Synthesis. But Wagner and Altenberg (1996) and Pigliucci (2010) show that the mapping is pretty far from one-to-one, even if it’s pretty strong. Studies of plastic phenotypes, for instance, demonstrate that the phenotype can vary somewhat independently of changes in the genotype.

⁵¹ It’s a matter of some controversy whether evolvability must involve *beneficial* phenotypic change or not. Ability to change *simpliciter* may be sufficient (see Brown, 2014).

⁵² See Masatoshi Nei (2013) for a paradigmatic example of such a view.

⁵³ Stern (2008) argues that mutations to cis-regulatory regions are essential to morphological change since they can alter the manner in which genes are expressed without rendering gene products non-functional. See Chapter 7, “Pathworks.”

would involve an organism that's evolved to have developmental tasks segregated to two separate modules (say, one module for the trunk and another for the extremities). The idea is just that the effects of mutations occurring in *Trunk Module*, say, are largely confined to *Trunk Module*, and likewise with *Extremities Module* (Sansom 2008, 141). Modularity is thought to be conducive to evolvability since modularity helps to downplay the effects of pleiotropy.⁵⁴ Pleiotropic effects are typically seen as a constraint on an organism's ability to move through phenotypic space. Since genetic and developmental links are a source of pleiotropic effects, a mutation affecting the expression of trait T_1 in the $G \rightarrow P$ map will also likely affect the expression of T_2 , and these pleiotropic effects are constraining and largely detrimental, overall. By decoupling traits through segregation into largely non-interacting developmental networks, selection is free to work on different traits separately and at different rates, enhancing the space of possible phenotypic variation (Brown 2014; Stern 2010).⁵⁵ The conceptual underpinnings of modularity and its relationship to evolution and evolvability are in some sense tied to the older notion of "mosaic evolution" (see Mayr 1997, 230). Mosaic evolution is the idea that different parts (especially molecular parts) of organisms have evolved at different rates given different selection intensities in different epochs. Mosaic evolution was seen as largely conjectural in the 1950s and 1960s when Mayr and others were writing about it (see Mayr 1982; Mayr 1997), but contemporary studies of organismal modularity seem to offer a mechanism through which something like mosaic evolution can be roughly corroborated.

⁵⁴ See footnote 36 from Chapter 1 of this thesis.

⁵⁵ William Wimsatt has a well-known position that's relevant to the discussion here. His view is called the "Theory of Generative Entrenchment" (Wimsatt, 1986), which holds that once a successful biological developmental strategy has been realized, it is really quite difficult for an organism to develop an alternative strategy without doing catastrophic harm to the organism. Mutations that promote developmental segregation may suggest a fruitful approach to getting around this well-known problem.

2.1.3 Summing up

These are the major areas of research on evolvability in the contemporary scientific (and, to a lesser degree, philosophical)⁵⁶ literature. There are some other, relatively minor studies on the links between phenotypic plasticity and evolvability (see Pigliucci 2010) and also some rather heterodox work on the (supposed) link between “The Baldwin Effect” and evolvability (see Weber 2013).⁵⁷ These, however, are sufficiently peripheral so as to justify being left to the side for a study such as this.

2.2 Building the functionalist case

Scant philosophical analysis on evolvability has been done as of now. What little work has been done seems to agree, with little controversy, that evolvability, whatever it is, is to be understood as a disposition (see Brown 2014; Sansom 2008). This should not strike the reader as particularly surprising. We’ve already said, after all, that the property of evolvability has to do with an *ability*; namely, the ability of an organismal type to move through morphological/phenotypic space. There is, however, a potential problem here. As Pigliucci (2008) and Brown (2014) are careful to point out, the available work on evolvability makes it look as though the concept of evolvability isn’t really sufficiently unified so as to warrant something like a notion of *the* concept of evolvability. Instead, it looks like we may have something of a loose clustering of different concepts that have been somewhat artificially grouped together under the moniker of “evolvability.” Geneticists have their concepts encoded by the **M** and **G** matrices, developmental biologists have their concepts associated with pathways and networks, and theoretical biologists

⁵⁶ Love (2003) and Brown (2014) are probably the two most thorough philosophical treatments of evolvability to date. Philosophy of biology, in general, is really just now catching up to the newly developed concepts within the ‘Extended Synthesis.’ See Pigliucci and Müller (eds.) (2010).

⁵⁷ Weber argues that novel behaviors may increase the propensity of a population to evolve in a particular direction. His case studies are speculative and merit little mention.

have their neutralist theories along with “exaptation” and “co-optation” effects.⁵⁸ The causal components underlying these different analyses of evolvability don’t seem to have all that much in common (at least as regards their concrete causal machinery).

Alan Love (2003) and Pigliucci (2008) argue that what’s needed is more refinement. That is, we should apply careful analysis to the different conceptual regions of the cluster concept “evolvability,” and break it up into other, individually named concepts that perhaps partially overlap (see Brown 2014, 550). Brown (2014) thinks that this is the wrong approach. Instead, we should treat evolvability as a “broad-based disposition” with robust explanatory power. My view is closer to Brown than to Love or Pigliucci (but more on this later).

2.2.1 Brown on explanatory robustness

Let’s take a look at what Brown has in mind when she talks about “robust” explanations. Brown argues (in a fairly uncontroversial manner) that selection-based explanations are paradigmatically robust. Why? The reason is that selection-based explanations don’t offer explanations of *only* the *actual* evolutionary pathway from one place in phenotypic space to another, but also other *possible* pathways.⁵⁹ Her case study involves the emergence of limb diversity in later primates.⁶⁰ If the target state in phenotypic space is *more limb length diversity*, then a selection-based explanation is robust in the sense that it can tell you how the outcome would differ had there been differences in selection intensity, mutation rate, migration,

⁵⁸ For a *very* clear and engaging discussion on exaptation and co-optation, see Stephen J. Gould and Elisabeth Vrba, “Exaptation—A Missing Term,” in *The Philosophy of Biology*, eds. David Hull and Michael Ruse (New York; Oxford UP, 1998). Briefly, exaptation (or co-optation—they’re used pretty interchangeably) is the phenomenon whereby certain organic structures evolved for one use, and then through (often neutral) mutations, that feature is subsequently co-opted into some other functional role. This appears to be fairly common in certain types of bacteria.

⁵⁹ For a tangentially related corollary on the topic of fitness, see Grant Ramsey (2006). Ramsey argues that a proper explanation of fitness involves not just actually realized fitnesses, but also *possibly* realized fitnesses.

⁶⁰ For more on the case study, see Young, Wagner, and Hallgrímsson (2010), “Serial Homology and the Evolution of Mammalian Limb Covariation Structure,” *Evolution* (59).

recombination, duplication, and the like. The target space denoted by *more limb length diversity* involves more than one possible outcome, and selection-based arguments explain how different regions of the target space might have been hit given a counterfactual analysis of evolutionary forces working on complex systems.⁶¹ Brown contrasts such robust selection-based explanations with “lineage explanations” from developmental biology, which she characterizes as non-robust. A lineage explanation is largely descriptive, explaining how biological components have *in fact* developed to facilitate an organism’s movement from one location in phenotype space to another. On this sort of explanation it is only the *actual* pathway that’s explained, meaning (according to Brown) that such explanations lack robustness.

2.2.2 Making evolvability explanations robust

According to Brown, we can unify the disparate conceptions of evolvability described by Pigliucci (2008) and others by imposing a bit of formalization onto this family of concepts, and by doing so we transform this cluster concept into a form of robust explanation.⁶² The formal representation of the disposition *x is evolvable*, for Brown, takes the form:

$$E: Pr(f_t | x \& b)^{63}$$

Here x is some proposition describing the population of interest, b describes the relevant environmental parameters that provide the evolutionary context for the population, f_t is a time-indexed possible outcome of interest for the population x , and E is the probability of the realization of f_t given the truth of x and b (Brown 2014, 563). For Brown, x can involve whatever population features you like, and b can represent any of a number of relevant environmental factors. Her treatment is meant to be a formal, robust method of explanation, and as such she

⁶¹ Brown (2014, p. 557).

⁶² Ibid.

⁶³ Brown (2014) writes it $E: Prx, b(f_t)$

need not be concerned with whether the relevant population features are of the kind studied by population geneticists, or those studied by developmental evolutionary biologists.⁶⁴ Whatever is relevant to the explanation is fair game as regards inputs for her formalization. This is what Brown means, I take it, when she says that evolvability is a “broad-based” disposition (Brown 2014, 550). What’s unclear, though, is whether she means to argue that evolvability is just *one* disposition, or instead a *family* of dispositions that can be *formalized* in the same way. It seems to me she is claiming something closer to the latter, whereas I will argue that evolvability is just one disposition, though a multiply realizable one.

I think my characterization of Brown is a fair one, given that she seems focused on evolvability as a kind of explanation rather than evolvability as a property of organisms. Further, she refers to the causal bases of evolvability as something like a family of categorical properties that in turn cause the disposition *x is evolvable*.⁶⁵

2.2.3 Evolvability as a unified, causal disposition

In §1.2.1 I noted that the distinction between categorical bases and the dispositions they purportedly cause is itself a questionable ontological bifurcation. A defensible position holds that dispositions are identical to their categorical bases, to the effect that what are usually called “categorical bases” are in fact just dispositions themselves (see Mumford 1998, 93-116). Nancy Cartwright (2015) also seems to claim, in the same vein as Mumford, that dispositions (or capacities, in her own vernacular) are all that’s really there (at least in terms of base-level causal explanations).

⁶⁴ This is to say that her account can take the components of the **G** and **M** matrices as relevant inputs, and this need not be to the exclusion of developmental constraints. Both can be treated, whether separately or together.

⁶⁵ Brown (2014, p. 561).

Evolvability plausibly offers a case in support of the notion of dispositions as causally potent, and in some sense identical to their categorical bases. It's unclear to what extent my own view is a departure from Brown. Her seeming commitment to the categorical/dispositional property distinction certainly suggests that she thinks of dispositions as descriptive or analytical and not properly causal. Relatedly, one might wonder whether I can claim a genuine causal role for a disposition like evolvability, since what's been described so far appears broadly abstract. Strategies and causal structures are fine as far as they go, but they don't seem to fit the bill as the kinds of things that *cause* events to happen in the world.

But my claim is that strategies and structures are directly related to concrete facts about organisms and what it is that makes them able to evolve. The conditional probability of organism *O*'s hitting a particular region of morphological space could be enhanced by a mutation in some cis-regulatory region of its genome, say. It's conceivable that such a mutation does two things simultaneously: such a mutation might, for instance, both loosen certain developmental constraints, enhancing modularity, and also increase the amount of standing variation or future variability within a population, creating a disposition to respond to selection pressures. There is, then, an increase in the available strategies through which morphological change might happen, though it may never come about such change *in fact* occurs. Even so, the organism still holds the disposition of evolvability, and this disposition may be realized in terms of any one of the strategies available to it. The disposition holds in virtue of the functional states described by the strategies that an organism may possibly realize. So, I argue, we have a case of a single, multiply realizable disposition.

Whichever strategy is realized, we are ultimately concerned with the same end state for the system; namely, whether it hits some target space (morphologically).⁶⁶ The biological facts (which are themselves dispositional), then, fix a further disposition by opening up evolutionary strategies and thereby increasing the conditional probability that the type will move successfully through morphological space; namely, the disposition *x is evolvable*. Any one of the strategies may be realized, though none need be.

2.2.4 Some clarifications on functionalist strategies

Contemporary analytic philosophy is rife with debate over functionalism. In the philosophy of mind and philosophy of cognitive science, for instance, there's a great deal of debate over whether "function" can be analyzed independently of the medium within which a function operates. Thomas Polger (2004) holds that no sense can be made of this notion of "medium independent" functions, since functions perform or fail to perform at least in part as a result of the physical context of their operation. But I think the common objections to functionalist theories fail to apply in this context. In the case of evolvability the property that I'm describing is one that holds in virtue of evolutionary strategies, some (or most) of which will not be realized, and so the functionally characterized disposition of evolvability does not hold *merely* in virtue of physical properties. If, as in the above paragraph, we think about evolvability in terms of mutations that increase available strategies for moving through morphological space, then the explanation is part physical and part functional. The mutation (and perhaps a physically realized strategy) accounts for the physical part of the explanation, where the rest is cashed out in terms of a functional space of open strategies.

⁶⁶ See the diagram on p. 557 of Brown (2014).

This is, perhaps, an odd claim. Surely the evolvability of any extant organismal type is to be analyzed in terms of its biological features; namely, whether it's sufficiently modular, whether there exists the requisite variation, whether the genomes in question are sufficiently robust, etc. This is true, but we may be allowed to distinguish between the evolvability of, say, Charlie the Tuna, and evolvability *in general*. This point is supported, I take it, by Brown's claim that explanations appealing to evolvability (or at least her formalization of it) are robust. That evolvability is a robust concept implies that there are many *strategies* for hitting a particular target region in morphological space. My functionalist-oriented claim, then, is that there are many causal structures that facilitate evolvability, and these may severally realize the functional aims⁶⁷ of evolvability as a property of organisms.

2.3 Objections and replies

Having outlined my favored position, I'll now shift my focus to some possible objections. The first objection has to do with the status of evolvability as a biological phenomenon in general, and the other has to do with the functionalist approach that I outline in (§2.2). As to the status of evolvability as a biological phenomenon, it may be objected that evolvability does not describe anything that is not already covered by the traditional toolkit of population genetics. That is, it may be argued that arguments about evolvability just collapse into arguments about natural selection and the like. As for my functionalist account of evolvability, it may be argued (as has been alluded to already) that the failure of functionalism in other domains means that it can't be a viable approach here either. I will take each of these in turn.

⁶⁷ I don't mean anything *teleological* by the word "aims." I mean only that there are causal, functional roles in biological domains with respect to biological organisms, and these are well explained by selected effects and the like.

2.3.1 Evolvability is not a distinct phenomenon

The first objection is that evolvability is not a genuine biological phenomenon. Rather, it is just a special case of other sorts of biological phenomena, like natural selection. The objection from natural selection is especially pressing in Brown's case, since part of her formalization involves an environmental parameter.⁶⁸ Her formalization is supposed to tell us the probability of a population realizing some future morphological state given certain facts about the population and its environment. This sounds a lot like natural selection. It's generally agreed, however, that a proper account of evolvability must be one that's contextualized to the organism's environment. Any other kind of account seems either empty or else unnatural (perhaps like some combination of orthogenesis and ideal morphology⁶⁹). Brown (2014) suggests one possible solution to this problem, whereby our environment-involving propositions are sufficiently fine-grained so as to exclude the environmental factors that would act as selection pressures. This would be difficult to do in many cases. Brown notes, for instance, that temperature can affect evolvability by altering mutation rates, but it's also a well-known form of selection pressure.⁷⁰ But if we can recognize that the effects of temperature (and other difficult environmental parameters) may be multiple, it's reasonable to claim that we can use something like Brown's approach (whereby we make the environment-involving propositions more fine-grained) and contextualize the environment-involving propositions to the specific effects that we're interested in. So, if we restrict our analysis to how temperature affects mutation rates in a population rather than how temperature affects the population overall, we don't run afoul of the selectionist's objection.⁷¹

⁶⁸ Parsing non-selective response to the environment is really quite tricky. Intuitively, we think that natural selection *just is* the response of an organism to the environment. Early precursors to theories of biological evolution seem to hold to something quite like this notion (see Richards, 2002 pp. 216-229). See also Love (2003).

⁶⁹ See Peter Bowler (1992).

⁷⁰ See Sterenly (2007).

⁷¹ See Brown (2014, p. 567).

Setting aside natural selection, it might be claimed that evolvability is just a special case of certain genetic processes, even if they're not directly related to selection processes. That is, perhaps there's no special property called "evolvability" over and above the mutational process that makes certain biological structures manifest in the world. The claim, then, would be that it isn't so much the case that some populations *evolve* more, but instead they just happen to *change* more thanks to the random walk of biological change.⁷² But Pigliucci (2008) points out that experimental evidence from *E. coli* studies suggests that evolvability can actually be *selected for*. Certain structures within bacteria seem to be able to accumulate a higher number of neutral mutations, enhancing their overall robustness. When selection pressures are applied in laboratory settings, the bacterial variants with higher robustness values appeared to have a selective advantage over their less robust counterparts.⁷³ A deeper analysis of the implications of these findings is beyond the scope of this chapter, but this example (and the biological literature more generally) seems to demonstrate that the advocate of mutation-driven evolution has a hard time explaining experimental results such as this.

2.3.2 Objections to the functionalist case

Finally it may be claimed that the functionalist account that I give, which implies that evolvability is one, multiply realizable disposition fails given the strong reasons to doubt the success of such a strategy in other domains (most notably philosophy of mind).⁷⁴ Thomas Polger (2004) has argued that there's no good reason to think that different physical tokens realize a unified type. What we have, rather, are distinct physical systems, each of which reduce locally to

⁷² This would be the picture offered by Nei (2013), for instance.

⁷³ See James Shapiro (2011).

⁷⁴ See Kim (2011), Chapter 6 for a fairly detailed survey of the standard objections to functionalist theories of mind.

their microphysical constituents.⁷⁵ Any claim that some unified type is realized by different physical systems (human beings and extraterrestrial beings with a silicon-based biology, say) is just an argument from analogy gone terribly astray. If the local reduction strategy is successful, then it seems the best we're left with is evolvability as a cluster concept in need of refinement into multiple dispositions, as suggested by Love (2003).

It's unclear how much force Polger's local reductions (or some similar strategy) can really have against the functionalist portrayal of evolvability. The biological facts affirm that evolvability, as a capacity, really can be accounted for by appeal to a fairly wide array of causal processes. Even as the causal mechanisms vary, the ability that we're attempting to describe is the same in each case: the ability to move successfully through morphological space. The fact that we can explain the same ability in terms of several causal processes serves to undercut any objection to the effect that evolvability is not to be considered a unified type, and instead locally reducible to other individuated processes. For the objection to stick one would have to demonstrate that the several causal processes in question do not in fact confer the same ability to the organism in question. It seems clear enough, however, that these several causal processes do confer the same ability on organisms.

2.4 Conclusion

In this chapter I argued for a realist construal of evolvability as a dispositional property of organisms, and further that this disposition is functional and multiply realizable. This is against treatments of evolvability that take it to be a "cluster concept," meaning that evolvability doesn't name a distinct phenomenon, but instead a family of concepts that should be further clarified. I found support in Rachael Brown's (2014) treatment of evolvability. Although she appears

⁷⁵ See Polger (2004, pp. 64-5).

ambiguous on the question of whether evolvability constitutes a single, unified dispositional property, her work in formalizing explanations using evolvability concepts and in making evolvability explanations sufficiently robust proved helpful for shaping my own thoughts on the subject.

In the next chapter I turn to an alternative way of framing the metaphysical commitments of science. Chapter 3 takes a look at *Ontic Structural Realism*, which is a view that understands objects and dispositions of the sort that I've covered up to now as eliminable from our proper ontological picture of the world. It should be clear from the first two chapters of this thesis that I will urge the rejection of such a picture. Some of the reasons I give are related to what's been argued for already, and others are self-contained to the third chapter.

3. OSR: AN ALTERNATIVE VIEW AND A COUNTERARGUMENT

In chapters one and two I argued that dispositions, realistically construed, are important for understanding some of the core concepts and phenomena in evolutionary biology: namely, *fitness* and *evolvability*. Recent literature in the metaphysics of science, however, involves a great deal of discussion and analysis concerning Ontic Structural Realism (OSR), and advocates of OSR claim that a careful analysis of our best fundamental physics seems to obviate any need for an appeal to dispositions (see Ladyman & Ross, 2007 pp. 190-6; French, 2014 pp. 238-45). How is such a claim to be understood? Surely normal empirical science (as in a laboratory or out in the field) relies quite heavily on the idea that we can reliably track dispositions.⁷⁶ If we want to know if a particular organism or population is evolvable, we answer the question by examining certain properties of the organism or population in question (see Chapter 2). If we want to know how a particular enzyme affects the rate of chemical reaction at a certain active protein site in cell biology, we manipulate certain properties of the genes controlling protein catalysts. Science involves the measurement of properties, and at least some of these properties are dispositional. If the OSR defender is to somehow explain away these properties she is going to have to tell a very convincing story.

The defender of OSR, as I will describe below, is arguing against the inclusion of dispositions in our ontology (causal or otherwise) mainly because OSR implies the elimination of ordinary objects, and ordinary objects (frogs, sugar, leaves, etc.) are those things that possess dispositions.⁷⁷ This is to say that because the metaphysical theory on offer under OSR implies

⁷⁶ See Cartwright (2015), Chakravartty (1998 pp. 395-8), Chakravartty (2007 pp. 119-26).

⁷⁷ See French (2014 pp. 212-18), section titled “Causation without a seat.” Here French sketches a possible version of causation without recourse to anything that “seats” dispositions or causal powers. This ends up being an appeal to

that there are no objects (and instead only “structures”), there are no things that can hold properties, and so it follows that there are no dispositions (see French, 2014 pp. 245-52). I will give two arguments against OSR’s eliminative aims. The first is that their position would make the success of the majority of scientific practices utterly mysterious. Here I find staunch allies in the likes of Nancy Cartwright and Anjan Chakravartty,⁷⁸ and I hope to add some strength to the case their cumulative works have already set forth. An instructive question with respect to this argument would be something like the following: How much metaphysics is necessary for making sense of scientific practice? I will argue that dispositions are indispensable to the task of making sense of scientific practice (in biology especially), and so the defender of OSR has a lot to answer for.

The second argument is more difficult, though probably the more philosophically interesting of the two. The defender of OSR is arguing that the elimination of objects implies the elimination of dispositions. A key premise in her argument (as we will see) is that permutation invariant maps in fundamental physics serve to undercut any primitive notion of identity, and so undercut any primitive notion of object-hood. I will argue that this inference is illicit in two ways (and the *second* way is of particular importance where my broader position is concerned): (1) it presupposes that we can legitimately “read off” our metaphysical commitments more or less directly from fundamental physics. This, I argue, is problematically naïve in ways that I’ll mention in detail in the relevant section. (2) It presupposes that metaphysics is domain general. That is, the defender of OSR seems to have it that lack of identity (and so lack of object-hood) in fundamental physics implies lack of identity (and so lack of object-hood) in chemistry, biology,

“structural change” as a sufficiently good causal notion, which is I discuss briefly in the main text.

⁷⁸ It is perhaps unusual to see Cartwright and Chakravartty lumped together, but I do see their work contributing to the same kind of project, albeit in different ways. That is, I see both of them doing the work of bringing metaphysics of science into contact with scientific practice.

and all the rest.⁷⁹ I will argue that there are strong reasons to doubt this (partly on the basis of what's already been said in chapters one and two).

The structure of this chapter will be as follows: in (§3.1) I briefly go over the basic argument from OSR. I will draw more from Steven French (2014) than from other sources in the literature, mainly because French has given the clearest and most convincing case for OSR. There are slight differences between French and other OSR adherents, such as James Ladyman, Don Ross, and Alyssa Ney,⁸⁰ but the core of French's version seems to capture the central commitments of the view as a whole, and this suffices for my purposes. In (§3.2) I offer my argument from scientific practice; in (§3.3) I argue against the central metaphysical claim of OSR, specifically to the effect that the defender of OSR is guilty of illicit inference; and in (§3.4) I offer some brief concluding remarks.

3.1 Ontic structural realism on objects and dispositions

The basic metaphysical intuition that the world is populated by objects, and that these objects in turn stand in certain relations to one another hardly seems disputable—at least at first glance.⁸¹ According to the defender of OSR, this sort of “first glance” metaphysics crumbles under deeper inspection, especially as one begins to take the findings of contemporary physics sufficiently seriously. James Ladyman and Don Ross (2007) refer to the classical, *a prioristic* methods of metaphysical analysis as “Neo-Scholastic” metaphysics. They see contemporary analytic metaphysics as largely the province of bookish schoolmen that haven't paid near enough attention to the methods and findings of our best contemporary science, much like the

⁷⁹ See Ladyman and Ross (2007 pp. 190-6). In this section they discuss and dismiss the disunity of the sciences in favor of a reductive approach.

⁸⁰ Ney's relationship to OSR is a bit more difficult to establish. She's clearly sympathetic to the project, although she may not go in for the whole thing. To get a good sense of her position, I recommend Ney (2009).

⁸¹ See Ladyman and Ross (2007 pp. 7-27).

Scholastics of the pre-enlightenment age. It follows, on their view, that work in analytic metaphysics does not make any *meaningful* contact with reality.⁸² Better, they think, to see how metaphysical commitments can essentially just be “read off” of our most fundamental current science; namely, fundamental physics.

Before moving much further, I want to note that I won’t be treating the difficult topic of how best to understand the notion of *structure* at play in OSR. Others have dealt with these problems already, and I see no reason to recapitulate the recent history of that literature here.⁸³ Instead, the defender of OSR can help herself to whatever manner of structural notions she likes, and I’ll focus my analysis on OSR’s treatment of object-hood (and dispositions, by implication).

3.1.1 The argument from permutation invariance

Whatever account of structure the defender of OSR is to give,⁸⁴ there is broad-based agreement (among OSR advocates) that our best-confirmed theories from fundamental physics imply the elimination of objects from our ontology. The best argument for why the elimination of objects follows from OSR is seen in Steven French’s *The Structure of the World: Metaphysics and Representation* (2014).⁸⁵ According to French, group theoretic presentations of fundamental physical systems constitute the best means we have for analyzing the ontological implications of fundamental physics.⁸⁶ Group theory, as it applies to physics, tells us that quantum statistical states are broadly permutation invariant (French, 2014 pp. 34-35). A simple example might involve a quantum system of, say, two elementary particles (say bosons). If we describe the

⁸² Ibid.

⁸³ See Bas van Fraassen (2006).

⁸⁴ Most treatments of OSR take their notion of structure to be in some sense *modal*, but this is typically left unexplained. For the best explanation of the modal structure of OSR, see Esfeld (2009).

⁸⁵ French (2014, pp. 34-41).

⁸⁶ Ibid.

system as having one boson on the left and one boson on the right, then intuitively, if we were to apply a transformation function that takes the left-sided boson to the right side and the right-sided boson to the left, we would expect that the mapping function gives two possible configurations of the system (the map plus the flip-map).⁸⁷ Counterintuitively enough, there's really just *one* configuration; that is, such a map is permutation invariant. Fundamental physics, posed in terms of group theoretic transformations, has no way of individuating *this* boson from *that* boson. Bosons (electrons, leptons, etc.) appear not to have *haecceities*. The key intuition that this is meant to undercut—at least for our purposes—is the notion that the world is comprised of tiny individuable objects, and that these objects are engaged in an extended series of “micro-bangings” that would support our intuitions concerning the fundamental causal dispositions of things (see Ladyman & Ross, 2007 pp. 7-27; Dyke & MacLauren, 2012).

In the philosophy of science (especially metaphysics of science), objects are typically defended on the basis of their indispensable role in causal explanations (see Chakravartty, 2003; Psillos, 2006). Without objects, it seems difficult to make much sense of causal relations, since our typical notion of causal relations involves the interactions of objects in space and time (see French, 2014 p. 212). French contends, however, that this need be no worry for the structural realist. Cast in structural terms, the things we usually think of as objects can be still be treated as such for all practical purposes (typical linguistic exchanges and the like). But we ought not be deceived into thinking that the things we typically construe as the “objects” involved in a causal series are anything like objects as typically conceived (French, 2014 pp. 99-100). Instead, objects are just nodes within structures, and the nodes themselves decompose into structures once properly analyzed.⁸⁸ There are just structures all the way down, and any talk in terms of objects

⁸⁷ Ibid.

⁸⁸ See French (2014, pp. 215-16).

is merely pragmatic. There are causal relations, but nothing like causal “powers” that the “object-oriented realist” (French, 2014, p. 14) might prefer. Objects and their dispositions are nowhere to be found in the group theoretic symmetries of fundamental physics, and so it follows that they’re nowhere to be found in general. Instead, causal relationships involve only a change in the relational structure of the system under investigation.⁸⁹ Causation involves nothing more than structural change.

3.1.2 Summing up

The fact that quantum systems appear permutation invariant under transformations and that the defender of OSR is still able to save causation (in a manner of speaking) seems to bode well for the view. Permutation invariance seems to support the structuralist contention that there are no objects. The key objection is posed in terms of the need for causal properties (like dispositions) to make sense of causal processes and relations, but the structural realist appears to have an answer. Furthermore, the structuralist takes his position to be well motivated: OSR maps onto contemporary physics better than opposing views, and we should develop our metaphysics to reflect our best fundamental physics as much as possible (Ladyman & Ross, 2007 pp. 27-38). If contemporary physics dispenses with any need of objects and dispositions then it is on the object/dispositional realist to say how we might be justified in keeping them, according to the structural realist.⁹⁰

⁸⁹ Ibid.

⁹⁰ See Ladyman and Ross (2007, pp. 38-45). These pages begin a discussion of what Ladyman and Ross call their “Primacy of Physics Constraint.” The idea is just that what the special sciences (and philosophers of the special sciences) can legitimately posit is significantly constrained by fundamental physics, but fundamental physics is not symmetrically constrained by the other sciences.

3.2 Metaphysics meets practice

The history of science seems to have taught us time and again that “common sense” (whatever one should take that to mean) cannot be trusted. Special relativity cuts against our common sense conceptions of time and simultaneity; general relativity cuts against our common sense conception of the geometry of space; evolutionary biology cuts against the common sense explanations on offer for things like human behaviors and mental content. It’s small wonder then that scientists and philosophers of science are often wont to warn caution when they see a metaphysician arguing for a particular view of reality’s contents on the basis of “intuition” or “common sense” (Dyke & MacLauren, 2012). Science just tells a much different story than the one told by common sense—or this seems to be so at least often enough to warrant skepticism with respect to the epistemic probity of common sense.

French (2014)⁹¹ and others (see Ladyman & Ross, 2007)⁹² argue that explanations in terms of objects, dispositions and the like in the special sciences have *at best* something like heuristic value. So, the special scientist, according to OSR, is engaged in producing something like common sense explanations in the same way that one might spin common sense explanations of behaviors and beliefs. In the same way that the cognitive sciences may suggest that cognitive mechanisms don’t support such common sense stories, the OSR defender says physics doesn’t support the ontological offerings of the cognitive scientist. Object speak in the special sciences is pragmatic, but has no claim to ontological import.⁹³

⁹¹ For French this takes the form of what he calls the “Viking Approach.” Where he thinks traditional metaphysics doesn’t really explain reality in the *ultimate* sense, he thinks the scientific metaphysician can still plunder traditional metaphysics where their ideas have heuristic values or where their argumentative moves and strategies prove beneficial. See French (2014, pp. 49-51).

⁹² Ladyman and Ross sometimes make it sound like “folk” treatments of causation can have heuristic value, and so isn’t *strictly* useless. See Ladyman and Ross (2007, pp. 266-74).

⁹³ French (2014, pp. 326-27).

3.2.1 Against a generalized attack on “common sense”

There are good reasons to doubt that this generalized attack on common sense holds in the case of scientific practice as it relates to the special sciences. As Nancy Cartwright (2015) points out, the fundamental warrant for appealing to objects and their dispositions (or “powers” as she calls them) is basically empirical. That is, we appeal to explanations in terms of the powers of objects because when we observe the world carefully in scientific contexts, objects display arrangement-sensitive powers that we can manipulate.⁹⁴ Far from an obscure, occult manner of metaphysical enterprise, Cartwright (2015, pp. 1-3) argues that a metaphysics involving the powers of objects according to their “canonical operations” is justified in virtue of its “this worldly” character.

Anjan Chakravartty (1998) argues similarly that objects and dispositions are quite important to the basic process of empirical discovery. As an example he cites the well-known case of the Fresnel-Maxwell transition in the study of the behavior of light. This case study has been put to much use in the history and philosophy of science, and to great effect in John Worrall’s seminal 1989 paper, “Structural Realism: The Best of Both Worlds?” Worrall offers a structural explanation of the shift. Here the relevant notion of structure is cashed out in terms of the representative mathematical equations. Worrall shows that Fresnel’s equations for wave optics can be derived from Maxwell’s equations for the electromagnetic field. This, for Worrall, is evidence of structure retention across theory change, licensing realism about structure.

But Chakravartty argues that it’s somewhat misleading to think that structure is all that counts in analyzing this shift. We need to say something, after all, about how it is that we can have a correct structural (mathematical) representation of light phenomena in the first place. This, Chakravartty argues, depends on our abilities to detect certain properties of objects in the course of empirical investigation:

⁹⁴ Cartwright (2015, p. 2).

[...] light is something which, on the basis of these mathematical relations, we associate with the following: influences propagated rectilinearly and made up of two components manifested at right angles to one another and to the direction of propagation; each component as an amplitude or intensity, the magnitude of which oscillates in a specific manner. Those properties of light which compose or give rise to precisely these influences are detection properties, that is, properties having to do with causal regularities on the basis of which we infer the existence of the entity possessing them: light.⁹⁵

Here, an ontology that includes objects and their dispositional properties is not a result of uncaredful metaphysical speculation. Rather, metaphysics is in direct contact with the process of empirical investigation. As Cartwright (2015) says, a “good” metaphysics (and here she means a good metaphysics for science, more specifically) is one with “solid empirical warrant” and one that “will not stretch its neck out farther than necessary.” An objects-and-dispositions ontology seems to be in good standing under such criteria.

3.2.2 Shifting the burden back to OSR

It would be up to the OSR defender, then, to say why we should adopt her view and its eschewal of objects and dispositions even in the face of what looks like good empirical success. That is to say, it plausibly involves an even greater commitment to seemingly obscure metaphysical contortions of reality to accept the implications of OSR. If that’s not the case, the OSR defender must give a very convincing argument why not, since the empirical warrant underwriting the opposing view looks comparatively immense. The OSR defender does think she has a response, but this will be covered in the next section where I discuss OSR’s “heuristic” approach to the special sciences.

⁹⁵ Chakravartty (1998 p. 396).

3.3 Against OSR's argument from (lack of) identity

Recall from (§3.1) that a key part of the argument for OSR comes from permutation invariance in quantum statistics. French (2014, pp. 34-35) illustrates this again using a simple quantum system of two particles. The system also includes two boxes, where each particle is placed in one box, but neither box contains both particles. As already discussed, the intuitive treatment of this system would be to say that it has two possible states, since we tend to think that were each particle to switch boxes with the other, that would count as a different state. But French points out that contemporary physics understands this as just *one* state: there's no difference between the "two" arrangements, and for French and others in the OSR camp this serves to undercut any primitive notion of identity.⁹⁶

3.3.1 Metaphysical underdetermination

This leads to what French (2014) terms "metaphysical underdetermination" with respect to individuals and objects. Our basic intuition, according to French and others, is that some intelligible notion of individuality and object-hood can be given *in terms of* fundamental physics, since the typical way of thinking about fundamental physics involves tiny, individuated, hard little parts that bang against one another in various ways. This is intellectually comforting, since where we may see cases of metaphysical underdetermination with respect to macro objects (tables, chairs, T.V. sets, planets, and the like), we're assured that there *is* in fact some primitive

⁹⁶ French (2014, pp. 36-42). French does consider some recent approaches that aim to break such underdetermination. One approach is to allow relations between particles to count toward establishing individual identities. So, we might distinguish between two particles by saying particle A has the "opposite spin" of particle B. French says two things about this. First, if it's legitimate at all, it doesn't do much, since this makes particles at best "weakly discernible." Second, he actually thinks this is not in fact a legitimate move to save identity, because it cuts against the real spirit of the Leibizian identity conditions in the first place.

notion of identity that can be appealed to in physics, and so getting individuality and object-hood right in the macro world is just a matter of finding the right map from the micro to the macro.⁹⁷

French (2014) tells us that the case is much worse as regards underdetermination, because there's no primitive notion of identity *at all*. So, he says, we might try to hold onto objects and just claim that objects are in some sense real, but that they are “non-individual.”⁹⁸ But he thinks (and I agree) that such a notion seems hardly intelligible at all. That aside, he also claims that such an approach involves too much “epistemic humility” (French, 2014 pp. 54-60). French's idea is that our metaphysical commitments should stay as close to our best fundamental physics as possible. So, if the clear implication from fundamental physics is that there is no clear notion of identity or object-hood, so much the worse for any such notion.⁹⁹ To claim instead that there *are* real individual objects behind the structural descriptions of physics, but that we just can't know them,¹⁰⁰ involves “too much humility” for any scientific realist worthy of the name to stomach (French, 2014 pp. 60-1).

3.3.2 Responding to the problem of metaphysical underdetermination

I think the “object oriented realist” can say at least two things to the OSR defender here. The first is just this: why think that epistemic humility is *not* the right stance to take? French gives essentially two reasons. (1) It flies in the face of scientific realism, and (2) our metaphysical commitments should stick as close as possible to physics. I think the response to (1) is obvious. That is, when our commitments to a particular form of scientific realism run into real problems, it may just be necessary to revise our realist commitments. This shouldn't come as a surprise to

⁹⁷ Ladyman and Ross (2007, pp. 20-7).

⁹⁸ French (2014 p. 43).

⁹⁹ See again Ladyman and Ross (2007 pp. 38-45) on the “Primacy of Physics Constraint.”

¹⁰⁰ This is a live position in the literature on structuralism, usually referred to as “Epistemic” (rather than Ontic) structural realism. French (2014), Chapter 4 includes a thorough and vivid description of the position.

structural realists, since their view just is such a revision. Why should realism be revised up to the structuralist commitments of French's version of OSR, but not any farther than that? This particular metaphysical interpretation of identity in quantum field theory is just one interpretation among several. It may be that some coherent conception of identity and object-hood is possible in QFT, but as of now such conceptions are at best vague and incomplete. French has to tell us why we should not revise scientific realism such that it can accommodate more epistemic humility (I for one would probably welcome such a revision).

A convincing response to (2) takes a bit more effort. French's statements about (2) can be charitably reconstructed into the following argument:

- (1) We have no reason to retain anything in our ontology if it requires too much epistemic humility and there is an alternative metaphysics that doesn't require it.
- (2) Contemporary physics suggests that commitment to individuals and objects does require a great deal of epistemic humility.
- (3) OSR does not require a commitment to individuals or objects.
- (4) We should therefore reject individuals and objects as legitimate parts of our ontology.

My response is directed at (1) and (2) in the argument above. More specifically, neither the science nor the metaphysics really suggest that epistemic humility creates a real problem (except, perhaps, a psychological one), and further, in order to ground our metaphysics in "what physics suggests" we must have a clear sense of what physics should be taken to imply for our metaphysics. I will argue that the case isn't quite so clear.

3.3.3 Undertermination as metaphysically innocuous

For French (2014) the problem of epistemic humility is clearly tied up with the problem of the

metaphysical underdetermination of objects. The physics doesn't give us a clear picture of what an individual object is, so we should get rid of objects and instead cast our commitments in terms of the structures described by group theory and the like. But Chakravartty (2007) argues that the problem of underdetermination isn't really that surprising. Metaphysical underdetermination is involved in (and even motivates) a broad range of metaphysical projects. Ted Sider's (2012) *Writing the Book of the World* attempts to solve problems of underdetermination by identifying a minimally sufficient "joint carving" set of laws and concepts for dividing up the world. Peter van Inwagen embraces the underdetermination of ordinary objects to the effect that he endorses an ontology containing just two kinds: particles and people (and not "people" in the biological sense!). For Chakravartty's part, he argues that we're constantly confronted with cases of underdetermination concerning how we should secure the identities of everyday objects like desks and trees, but we don't typically think that there aren't any desks or trees, and so we may be warranted in thinking of underdetermination of the sort French points out as likewise innocuous.¹⁰¹

French responds that we're not warranted in taking his sort of underdetermination as innocuous with respect to the status of objects because fundamental physics is just what it says: *fundamental*.¹⁰² Because of the fundamental status of fundamental physics, we are to understand that the inability to secure a notion of individual object-hood at the level of physics implies that no such notion could be forthcoming. Chakravartty's "innocuous" underdetermination only works, according to French, if there's something more fundamental one can appeal to in order to secure a notion of object-hood, but he's just shown that this move can't be successful. So, we

¹⁰¹ Chakravartty (2007, pp. 70-89).

¹⁰² French (2014, pp. 42-7).

should close the gap of epistemic humility between our intuitions about objects and what physics has to say about them by tailoring our commitments to whatever the physics says.

Now it looks like French is arguing against epistemic humility on grounds that realists should not permit too much epistemic humility. French hasn't actually given any argument to show that epistemic humility is harmful to realism, except maybe in the sense that too much of it would create some skepticism toward the notion that fundamental physics is directly informing our metaphysics, which seems self-serving. Couldn't epistemic humility be useful for realists, though? It seems to me that epistemic humility is plausibly conducive to epistemic flexibility,¹⁰³ and this is likely something that we want as realists, just given the history of science and scientific change. After all, the direction of theory change looks pretty unpredictable from our present position. A stance that invites theory defeaters as a progressive tool looks like the best stance to take, then, and so an epistemically humble stance looks better than the alternative.

3.3.4 Epistemic humility as the best justified stance

There are two further a position of epistemic humility might be the best justified one with respect to how we should read our metaphysical commitments off of the physics. There are two senses in which we might be justified in claiming this. It may be that the suggestions of physics are at best ambiguous on the subject of individuality and object-hood. If that's the case, we may be warranted in following Chakravartty (2007) in taking underdetermination (even in fundamental physics) as basically innocuous. There is some precedent for thinking the physics is, in fact, broadly ambiguous. As French seems to recognize himself, the early structuralists (both in philosophy and physics) typically thought that there were real objects underlying the structural

¹⁰³ A person who's "epistemically flexible," I take it, is someone that is generally sensitive to evidence (and especially *changes* in evidence) as regards belief formation. This probably involves an attitude toward beliefs that takes them as broadly defeasible, too.

descriptions of physics, but held this knowledge as inferential rather than direct.¹⁰⁴ If this early structuralist position is right (and I'm not claiming it is) then a position of epistemic humility with respect to objects and the like looks like it's just the right position to be in. French hasn't given much of an argument against such a position, except to say that it involves "too much humility" for any realist to entertain. I've already said what I think of that claim.

3.3.5 A further response to the underdetermination problem

The second (and most important) response is this: why think metaphysics is domain general? Which is to say, it may not be the case that one can carry out a metaphysical analysis of objecthood in fundamental physics and then just import that same analysis into the domains of chemistry, biology and all the rest. Establishing what counts as an object in biology may require a different mode of analysis involving a different set of identity conditions than similar questions applied to physics. At least on the face of it, we probably don't think transformation symmetries establish all of the relevant facts of object identity in biology, since biological populations are surely not permutation invariant! Ladyman and Ross (2007) argue that the task of metaphysics *just is* the unification of the sciences. But whether or not the broad unification of the special sciences with fundamental physics is possible is really more of an empirical question, and so far the evidence suggests that it is not (Cartwright, 1999). French (2014, pp. 326-328) recognizes this, but then runs roughshod over the special sciences anyway.

French notes, for instance, that the directionality of thermodynamics poses problems for the reduction of chemistry to physics. He notes similar problems with respect to the reduction of biology to chemistry, but maintains throughout that there must, *in principle*, be some solution to

¹⁰⁴ French (2014, pp. 65-79).

the problem of reduction, and that it may be discovered by some future science.¹⁰⁵ Here French is happy to shift to a heuristic mode, and claim that the biologist can still offer explanations in terms of the workings of genes, proteins, organisms, and the like, but he cautions that when we're doing metaphysics we should not slide into thinking there *really are* any such things. In fact, there are no *things* at all, just the structures described by physics.

The OSR defender owes us more than this, though. The fact that the special sciences have proven so resilient to reductionist explanations seems to be a fact that itself cries out for an explanation, and a better explanation than the promissory notes that we're used to receiving. We have good reason to think (e.g. everything from chapters 1 and 2) that this resilience against reduction really has to do with some non-reducible properties possessed by the phenomena that are treated within their respective domains of investigation. If that's the case, it may be that our metaphysics should follow suit and display the same "patchwork" pattern that our sciences exhibit. It may be that French is right about individuality and object-hood *at the level of the physics*, but a metaphysics that involves objects, powers, dispositions and the like may still be necessary for a robust analysis of the biological domain. The first two chapters of this thesis make a case for two candidate phenomena in evolutionary biology: *fitness* and *evolvability*. Fitness and evolvability, it was argued, are dispositional properties of objects; namely, organisms. Further, if we want to understand how organisms and populations change in evolutionary history, we need to understand the causal contributions of these dispositional properties. For fitness this involves some understanding of the causal interactions between traits embedded in trait complexes, and for evolvability this involves understanding how different

¹⁰⁵ Or, if not discovered by some future science, he argues we may still be permitted to exclude from our ontology any content that doesn't feature in fundamental physics (see French 2014, pp. 325-30). This move is somewhat suspicious, since it amounts to claiming that even if the sciences don't actually do the work of justifying physics' epistemic and ontological authority over the rest of science, physics gets to hold trump cards anyway.

strategies can be deployed in order to realize some target region in morphological space. The OSR defender has done little to show how to explain away such phenomena. French (2014, p.212) gestures vaguely at the problem by talking about “structural change,” but change in evolution (at least of the sort I’m talking about) involves more than just a shift of structural relations or rearrangement of the “nodes.” It involves the interaction of biological objects and their causal powers.

Take as an example the typical account of the origin of upright, bipedal locomotion in later *Homo*. Sparing a lot of the messy details, it’s usually thought that this evolutionary novelty was at least in part due to a selective regime that favored organisms that could travel longer distances on foot in some of the grassy plains of Africa’s Great Rift Valley (the ability to travel longer distances made water procurement easier, for instance). In order to hit the right target in morphological space, it is necessary to have something that can be properly disposed to developing the necessary evolutionary novelties. In this case it was necessary to have an organism that could properly decouple hind-limb development from fore-limb development (Brown, 2014), develop a large musculature structure in the gluteal region for stabilization, and a certain spinal contour that could appropriately support the head (see Lovejoy, 1988). This case clearly involves appeal to dispositions: the ability to decouple certain anatomical structures in the course of development, some standing musculature structure that can adaptively respond to a selective regime favoring gluteal structures capable of performing front-back stabilization, etc. And clearly these are dispositional properties are ascribed *to* something: ancestral apes and early hominids. If physics doesn’t support such notions, this may well be because the things of physics don’t support talk in terms of objects and dispositions whereas evolutionary biology does. If

that's the case, then we should not only shift scientific modes when going from biology to physics, but also metaphysical modes.

3.4 Conclusion

In this chapter I outlined some of the core arguments and commitments involved in Ontic Structural Realism. OSR is a relative newcomer in the metaphysics of science literature. OSR bears relevance to my project because of its potential implications with respect to realism about objects and dispositions of the sort I've argued for in chapters one and two of this thesis. I've argued against OSR's treatment of objects and dispositions on two separate but related grounds. On the one hand, OSR's position on such matters seems to fly in the face of scientific practice, and this without much warrant. Any form of metaphysics that moves so strongly against the empirical grain of the sciences must provide some *very* convincing reasons to warrant doing so. OSR, I argue, has not provided such reasons. I argue further that OSR defenders are guilty of illicit inference in that they take the purported lack of identity manifest in quantum systems as sufficient for lack of identity *in general*. The OSR defender has not shown that such a metaphysical move is legitimate, and indeed there seems to be good empirical motivation underlying sympathies to a disunified view of the sciences and metaphysics.

CONCLUSION

I've argued for a realist interpretation of dispositions and their active role in causal processes in evolutionary biology. In the case of fitness, I argued that the relevant dispositional property is to be understood as applying to whole organisms as a result of the total causal interactions among an organism's traits. In the case of evolvability, I argued that the relevant property may be treated as a functional disposition that's multiply realizable.

More generally, I take the cumulative results of chapters 1 and 2 to show that any ontologically deflationary treatment of dispositions in the metaphysics of science has quite a bit to answer for. Explanations in terms of dispositions, powers, capacities and the like feature quite heavily in the natural sciences, and indeed, close analysis suggests that an earnest commitment to such things as constituent members of our ontology best accounts for our empirical experience of such dispositions and powers.

I've presented Ontic Structural Realism as a potential rival that dispenses with the need to appeal to things like objects and dispositions. I argued, however, that such an account flies in the face of our empirical warrant both for retaining objects and dispositions as parts of our ontology, and for regarding the natural sciences as disunified rather than unified with or reducible to fundamental physics. Because we have good empirical warrant for understanding the sciences as disunified, there are good reasons to think that metaphysics should be thought of as domain specific. We need not see Ontic Structural Realism as a threat to the objects and dispositions that ground our metaphysical analysis of evolutionary biology, then. We're free to treat dispositional properties (like fitness and evolvability) as real and causally active in evolution.

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VITA

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