Kind Historicism & Biological Ontology

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To quote the opening paragraph of this thesis: 'Philosophers are not normal people'. Few understand this better than my loving wife, Cassandra. Thanks for being there.

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Abstract

This thesis develops a new theory of natural kinds for the biological world, called 'Kind Historicism', and addresses the relationship between natural kind theorizing and scientific reasoning. Applied to natural kinds and individuals in biology, Kind Historicism provides an ontology of the biological world. Discussions of biological ontology have struggled to balance insights from scientific practice with tools from analytic philosophy, metaphysics, and ontology. Ontological questions and practical/epistemic questions are often entangled. This thesis separates the two enquires, explaining why an ontological account of 'what-there-is' in biology should not straightforwardly dictate scientific categories, objects, or concepts. More precisely this thesis provides, in two parts, the development of Kind Historicism in light of discussions of natural kinds, essentialism, and monism, followed by the application of Kind Historicism to the natural kind status of biochemicals and to the problem of biological individuality. Finally, the success of Kind Historicism is measured against its ability to account for 'intrinsic heterogeneity' and 'theoretical pluralism', features of the biological world and science, respectively, believed to preclude biological natural kinds.

Contents

List of Abbreviations

CM — Category Monism

CP — Category Pluralism

EP — Evolutionary Psychology

EPP — Essence-Property Principle

HPC — Homeostatic Property Cluster

PR — Promiscuous Realism

SCQ — Special Composition Question

TM — Taxonomic Monism

TP — Taxonomic Pluralism

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Introduction:
The Problem of Biological Ontology

Joey is sitting in his chair in my apartment, a foot or so from the edge of my desk. He has been there more-or-less every day for four years. He is a grey male and a spritely 17.5lbs. He is a cat. Normal people do not find themselves wondering about whether their cats exist, the manner in which they exist, or any other such things. But I do. Philosophers are not normal. Suppose a philosophical colleague of mine asked: 'What *is* Joey?'; how should I respond? Well he is a cat, of course—*Felis catis,* to be precise—but he is also a Chartreux[1], a pet, a mammal, a carnivore, a male, a hunter, a (poor) guardian, and an adult. Joey is also a collection of atoms, molecules, and fields, arranged in a certain way all the way up to cells, tissues, organs, and systems. There are many and more ways to describe Joey, many of them objective and, perhaps, many of them referring to real categories to which Joey belongs. So how should I answer? If my colleague were a breeder I would answer 'He is a Chartreux'. If my colleague were a taxonomist I would answer '*Felis catis*'. If my colleague were a veterinarian I would answer 'overweight adult male with an overactive thyroid'. If my colleague were an ecologist I would answer 'hunter, predator, and carnivore'. If my colleague were a physicist I would

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[1] A breed of cat from France, similar to the British Shorthair.

answer ‘fields, particles, and the like’. And if my colleague were merely being polite I would answer ‘he’s my pet’.[2]

I can propose many categories to which Joey might belong. Each is an appropriate and acceptable answer for some number of different questions, asked in different contexts by different interested parties. The breeding, taxonomic, medical, ecological, physical, and idle questions have simple answers. But my colleague is not a breeder, taxonomist, physicist, or any of these other things; she is a metaphysician and ontologist, and her question was neither innocent nor idle. She knows that answering it to her satisfaction is a tall order. She knows that, in the philosophy of biology, the ontological status of organisms is hotly contested. For any category I propose, my ontologist colleague will ask why *that* category is fundamental, or privileged. If I appeal to science, adopting whatever it tells us about Joey, she will point out that science fails to provide a univocal answer, since different branches of biology classify and identify differently. She might also ask why I look to biology; the sciences of chemistry and physics offer different conceptions of Joey, still. Even if I propose that we accept many of these categories, she will ask for a theoretical account that justifies the acceptance of certain but not all descriptions of Joey. She is asking for a theory of biological kinds.

My colleague also knows that the category to which Joey belongs is not the only relevant issue pertaining to his ontological status. When she asked what Joey *is*, she was also asking about which things are parts of Joey and which are not. Here too there are many answers. Perhaps Joey is everything inside of his fur, in which case the contents of his bowels are a part of him (until they’re not), as is the elastic band he just swallowed. And what about the parasitic tick burrowed under his coat, or the symbiotic bacteria in his gut? Perhaps Joey is all of these pieces that contribute to a physiological system, in which case changing physiology means an ever-changing cat. My colleague will ask for a principled theory that answers these questions and answers them not just for Joey, but for all biological objects. My colleague is asking for a theory of biological individuality.

Taken together, a theory of biological natural kinds and a theory of biological individuality constitute a theory of biological ontology, for the purpose of this discussion. They tell us what biological things are like—what it is that makes them whatever it is that they are. They tell us what Joey *is,* if anything in particular.

These accounts should, ideally, also make sense of the many non-ontological categories and individuations that people use. If ‘overweight’ is not an ontological category to which Joey belongs, then we want to know why it works so well as a scientific category. If Joey’s gut flora are actually parts of him, then we want to know why scientists can successfully treat

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[2] Jean Harvey once recommended to me the phrase ‘companion animal’ rather than ‘pet’. While I support the moral message of this swap, I hesitate at the linguistic awkwardness. See her (2008).

them as being separate. So the primary questions of biological ontology here concern ontological categories and their nature, and these lead naturally to accounts of non-ontological categories and their usefulness.

In short, my colleague's question is not one to be taken lightly. Its answer is neither straightforward nor obvious. Rephrasing it in a slightly more general fashion, the question is the primary question of biological ontology:

Q1: What kinds of things populate the biological world?

My answer to this question will unfold over the course of this thesis, first in the abstract in PART I, then more concretely in PART II.

Q1 structures the thesis as a whole, which develops (PART I) and then examines the implications of (PART II) an account of biological ontology. However even an adequate answer to ***Q1*** would leave unanswered two further and equally important questions:

Q2: What does the correct answer to Q1 entail for monism and pluralism?

Q3: What role should this biological ontology have in scientific practice?

An account of biological ontology should shed light on these issues, if it is to be helpful to philosophical and scientific debates. My investigation into ***Q1*** hinges on my account of natural kinds, developed in Chapters 1 and 2. My investigation into ***Q2*** begins by critically examining the concepts 'monism' and 'pluralism', in Chapter 3. My answer to ***Q3*** emerges gradually, over the course of the thesis, receiving direct attention at various points in Chapter 3, 4, 5, and 6.[3]

There are many uses of 'ontology', but the sense I am interested in concerns individuals and their kinds, whatever facts bear on individuality and kind membership, and whatever facts follow from individuality and kind membership. The sense of 'ontology' used here is thus selective. Kinds and individuals; nothing more. These two ontological categories are of interest for primarily historical reasons: many philosophers of biology have discussed these categories and a non-negligible sub-set of those philosophers have dismissed one or more of those categories as irrelevant or inapplicable to the biological world. I will show that these dismissive attitudes are avoidable.

This project is much more narrow than a straightforward scientific realism. Many things have bases in reality that are nevertheless not natural kinds or individuals. Over the course of

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[3] The relationship between metaphysics, philosophy of science, and science has, during the writing of this work, become a flashpoint of discussion. The most notable work is Ladyman and Ross' (2007) *Everything Must Go*. While the topic of that book was how science (particularly fundamental physics) should guide certain ontological assumptions about objects in metaphysics, my concerns are different—nearly the inverse. I am asking whether and how ontological facts from a scientifically-informed metaphysics should feed into real-world scientific reasoning. My concern with individuality and objecthood, which are more in line with Ladyman and Ross, emerges in Chapter 5. On the topic of scientifically-informed metaphysics, see the volume edited by Ross, Ladyman, and Kincaid (2013).

the project I will at times digress to explain how realism and objectivity interact with the ontological categories I discuss, but realism and objectivity are not my prime targets. On the account developed, 'natural kinds' and 'individual' mark two ontologically special statuses. Why they are special, what sorts of things have this status, and what this all entails for our metaphysics and our science are the topics of this thesis. The last question, concerning science, is particularly important.[4]

My three core questions have, of course, been asked before, albeit often indirectly. But philosophy and biology throw up some unique hurdles to these investigations and often these hurdles go unnoticed by unsuspecting philosophers of science. I will now introduce the two largest such hurdles: intrinsic heterogeneity and theoretical pluralism. I will then briefly explain how these challenges have tripped-up previous investigations into biological ontology. After an outline of the plan of this thesis, I will conclude with a brief postscript on 'Stanford School' pluralism.

1. The Challenges of Biological Ontology

Difficulties for biological ontology start with the recognition that the biological world is messy—incredibly messy. This makes unified ontological theories quite difficult. These difficulties are compounded twice over. First, relying on established philosophical concepts is of no help, because accounts of scientific ontology developed with physics or chemistry in mind do not fit the biological world. Second, appeals to science are of no use because biologists seem happy to use an array of different taxonomic and individuation schemes. Biology is therefore messy in two relevant respects: biological things are heterogeneous in their intrinsic properties and the biological sciences are heterogeneous in their theories of classification and individuation. The claim about biological objects is 'intrinsic heterogeneity', the claim about biological sciences is 'theoretical pluralism'. I will unpack both, below. The interesting questions of biological ontology emerge at the intersection of the two; but not everybody shares this view. The goal of this section is to introduce intrinsic heterogeneity, its

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[4] Paul Humphries (2013), in a volume dedicated to exploring the emerging field of scientific metaphysics, distinguishes two types of ontology: Scientific and Speculative. These correspond, roughly, to scientific metaphysics and analytic metaphysics. Like most scientific metaphysicians, he is critical of the latter. The difference between the two lies in the constraints placed on ontological claims. Scientific approaches ensure that their claims do not conflict with certain core empirical findings (e.g. relativistic accounts of gravity or conservation principles for energy). Speculative approaches ensure that their claims meet certain *a priori* ideals (e.g. those imposed by Humean supervenience). Classically, natural kinds belong to speculative ontology, surrounded by various *a priori* criteria and constraints. I critically examine these constraints, eliminating most (Chapter 2). I then see how claims about kinds and individuals stack up against present scientific knowledge concerning biological groups, structures, histories, and individuals. I might thus be viewed as taking subject matter traditionally examined within Speculative ontology, eliminating much of the *a priori*, and holding it accountable to scientific knowledge.

perplexing relationship with theoretical pluralism, and its hazy implications for metaphysical monism and pluralism. In the next section, I will discuss examples from the recent history of philosophy of biology where philosophers saw the tools and goals of biological ontology somewhat differently.

Intrinsic Heterogeneity and Theoretical Pluralism. The dominant view of the biological world is of a world characterized by heterogeneity and disunity. This can be seen quite easily at the level of organismal taxonomy, since biological things differ greatly across taxonomic ranks. Bacteria are very different from Eukaryota, plants are very different from animals, mammals are very different from amphibians, and tigers are very different from zebras. Even more problematically, biological things also differ greatly *within* taxonomic ranks. There are many ways in which Joey is unlike other *Felis catis*; there are many ways in which one amphibian will differ from the next. Everything from outward appearance to behaviour to genetics may differ from one particular to the next. Robert. A. Wilson calls this 'intrinsic heterogeneity' (2005 Ch. 3). By 'intrinsic' Wilson means to exclude the relational or extrinsic properties of organisms, focussing on features like morphology and genetics. While Wilson meant the term only to apply to organisms, I extend the concept to cover all biological particulars. Wilson would not object; he recognises that heterogeneity is 'a cornerstone of the idea of evolution by natural selection' (2005, p.100).

Intrinsic heterogeneity has implications for biological science.[5] Studying one group of *Felis catis* will not reveal features common to all cats, just as learning to individuate algae will not tell me much about individuating vertebrates and classifying plankton will not help classify daffodils. Biological things share many properties, but only imperfectly. Exceptions are to be expected in biology. Heterogeneity is the norm. As a result, there exist a plethora of taxonomic methods and individuation schemes, each suited to different realms of enquiry, different investigative interests, or different samples. Bacteria may be individuated one way and vertebrates another. Evolutionary biologists may taxonomise organisms differently than population ecologists. Borrowing a term from John Beatty (1994, 1995), I will call this 'theoretical pluralism'. Beatty recognizes that biologists require multiple theories or mechanisms to account for or represent a particular domain of phenomena. For Beatty,

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[5] I will toggle between discussions of ontology and discussions of scientific practice. It is easy to get lost. For clarity, terms such as 'biology' and 'chemistry' will be used to refer to sets of phenomena in the world; 'biological practice' and 'chemical practice' refer to the sciences that investigate those phenomena. Likewise, while 'natural kinds' will refer principally to human-independent classes in nature, 'classifications' will refer to scientists' attempts to categorize. Whether or not classifications ought to approximate the natural kinds is a central question of this work.

theoretical pluralism should not be viewed as an accident. It is a necessary response to the heterogeneity of the biological world.[6]

Theoretical pluralism means that seekers of biological ontology cannot look to biological science for answers. The biological sciences feature heterogeneous representations of biological taxonomy and individuation, each developed with specific questions and subject matters in mind. This theoretical pluralism may reflect an underlying disunity to the biological world, it may reflect the limitations of current science, it may reveal gross error in biological science, it may reveal the poverty of metaphysics and ontology, or it may be something else entirely—and perhaps a mix of all four. Regardless, the state of lay-ontology in biological science complicates rather than ameliorates the problem.

Intrinsic heterogeneity and theoretical pluralism are particularly interesting when contrasted with kinds and classification in physics. Where physical things are homogeneous, biological things are heterogeneous. This is not a new observation. At the mid-point of the 20th century, following great progress in both molecular biology and quantum mechanics, the disconnect between the objects of physics and those of biology became a central focus for scientists speculating about quantum mechanical explanations in biology (e.g. Bohr 1937, 1958, Elsasser 1958, 1966). Geologist-turned-theoretical-biologist Walter Elsasser[7] wrote,

> Modern physics, or much of it, deals not so much with objects as it does with homogenous classes, where one member of the class is completely substitutable for the next. We think that much of the gulf that still yawns between the physics of biomolecules and biology proper results from the conceptual difficulties which arise when observational material as inhomogenous as that of biology is forced into the mold of a conceptual scheme which is too narrow for it. (Elsasser 1966, p.14)

In that last sentence Elsasser was speaking about the conceptual scheme within which scientists form generalisations; but his claim applies equally well to theories of natural kinds. Those theories of kinds that we receive from physics and chemistry require far more uniformity than the biological world can provide. Chemicals in the periodic table, for instance, are uniform in that all members of a kind share a physical microstructure and the requisite microstructure for membership in any such kind will be the same yesterday, today,

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[6] Beatty applies theoretical pluralism to more biological theory than just classification and individuation. I discuss theoretical pluralism in relation to laws and generalisations in the conclusion. See footnote 22 in (Beatty 1995) for other uses of 'theoretical pluralism' in the literature.

[7] Elsasser made his name with the (still-accepted) 'dynamo' theory of the Earth's polarization. Though known to the world for geophysics, he later became interested in theoretical biology (specifically what we now call 'systems' biology). His work on the fundamental disconnect between physics (and its reliance on statistical quantum mechanics) and biology (and its need for individualized (non-statistical) representations) is an excellent work, largely overlooked by contemporary philosophers of biology. See (Elsasser 1966).

and a hundred centuries in the future.[8] Nothing in biology is so uniform or unchanging. Elsasser continues,

> Radical inhomogeneity is by universal consent an outstanding and altogether basic property of all the phenomena of life. The proposition 'no two cells are ever exactly alike,' often enunciated by observing biologists, summarizes a vast amount of empirical evidence. It is not the expression of some vague poetic feeling about Nature but the condensation of the result of innumerable sharp-eyed observations. Moreover, it is a property to be found at all levels of biological organisation. (p.14)

Those who look to the periodic table and believe that natural kinds are perfectly uniform, share an essential physical structure, and are unchanging will be disappointed when they examine biological kinds. Intrinsic heterogeneity means that seekers of biological ontology cannot look to ontological theories developed with only physics or chemistry in mind.

Monism & Pluralism. On a certain naïve traditional account, the world might be thought to contain a uniquely delimited set of biological objects with a uniquely correct ordering, akin perhaps to the periodic table of elements. Intrinsic heterogeneity and theoretical pluralism challenge this simplified view. At first glance, these features of biology and its science appear to suggest that neither can we sort biological objects uniquely nor can we find a single conception of individuality to suit all. This suggests that there is no single answer to questions about the ontological status of biological objects; at best there are answer*s*. This has led to calls for the abandonment of 'monism', often associated with traditional views on ontology, and for the adoption of either 'pluralism' or 'conventionalism', metaphysical views seen as better suited to a heterogeneous biological reality. I will briefly survey these responses.

Contemporary discussions of biological ontology vary in content and application; however two stances undergird most. First is the belief that, though nature may make things 'what-they-are', she does not do so uniquely. There are multiple facts of the matter about biological things' ontological standings. Second is the belief that human convention makes things 'what-they-are'. The facts of the matter about ontological standing in biology come only from humans, not nature. Less-common in contemporary discussions is a third option: the belief that biological objects admit of singular natural identities.[9] These options are often called 'realist pluralism', 'conventionalism', and 'monism', respectively.[10]

The realist pluralist believes that, for any given biological object, there are multiple things that it naturally is. According to the pluralist, two philosophers who disagree about the nature of a particular object may both be right. Joey is *Felis catis*, but also a predator, a carnivore, a

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[8] Throughout this work I adopt the received view physico-chemical natural kinds as a foil for my own account of biological kinds. Since my focus is on biology, I cannot delve into criticisms of the physico-chemical kinds account. I will acknowledge shortcomings in the view where possible, often in footnotes, and will discuss one serious limitation to the view at the end of Chapter 5.

[9] Monists still exist, but they are unpopular. See (Devitt 2008, 2010, Lewens 2012).

[10] These labels will be refined considerably over the course of this thesis, particularly in Chapter 3.

pet, a mammal, and obese. There are many and more ways to describe Joey, many of them objective and, perhaps, many of them referring to real categories to which Joey belongs. Similarly, there are many ways to delimit the boundaries of Joey. From an evolutionary perspective certain of Joey's symbiotic bacteria are not parts of him, while on a physical perspective they are all parts of him, so long as they are inside of him, and on a physiological perspective any functional bacteria are parts of Joey and any non-functional bacteria are not. The realist pluralist belief is that there is indeed a fact of the matter as to what a given biological thing is, whether *qua* kind or *qua* individual, but that this fact is a long conjunctive sentence. Versions of pluralism vary in the length of that conjunction.

Conventionalism holds that human convention makes things 'what-they-are'. The groups into which we classify biological objects are not real features of the world but mind-dependent features of human scientific reasoning. Holders of this view are not just sceptical about our ability to know the identities of things; they are sceptical about that there are any identities in the first place. The same holds true for claims of individuality. A conventionalist will maintain that there are many ways to draw boundaries around biological objects, and that all are merely reflections of different research agendas or perspectives.

Monistic realism, the foil against which the first two options are often presented, is the belief that there is a single 'what-it-is' for any given biological object. Applied to natural kinds, this is the claim that, in spite of the many properties possessed by biological objects and in spite of the many ways in which we classify them, there is one single category to which any given biological object belongs. Applied to individuation, this is the claim that there is exactly one composite of atoms, cells, organs, and tissues that constitute a given thing.

Pluralism and conventionalism represent two distinct reactions to monism in the face of intrinsic heterogeneity and theoretical pluralism. While pluralists take these problems to motivate scepticism about the monist's ontological programme, conventionalists take it to motivate scepticism in ontological programmes altogether. The main focus of this thesis will be the two realist options: monism and pluralism.[11]

Any account of biological ontology must reckon with the problem of biological heterogeneity. Pluralism is a good fit for a heterogeneous biological world, but many questions remain concerning the precise form that pluralism should take and which virtues of monism can be retained. Thus my second guiding question:

Q2: What does the correct answer to Q1 entail for monism and pluralism?

Answering this question requires careful consideration of the challenge from heterogeneity as well as analysis of monism, pluralism, and their implications.

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[11] I view conventionalism as a live option only in case monism and pluralism are eliminated.

In this thesis, the account of biological ontology on offer is Kind Historicism[12]. This is the claim that biological kinds are what-they-are in virtue of historical essences. Kind Historicism in turn supports an account of biological individuality. It also entails a sort of metaphysical pluralism about natural kinds, but also a sort of monism about *biological* natural kinds. The claim is that all biological kinds are historical kinds, but that non-biological kinds are not; they are physico-chemical (microstructural) kinds. There are thus two types of kinds and this is the sense in which my position entails a metaphysical pluralism. I will differentiate this from another sense in which positions can be pluralist: taxonomic pluralism. My account is a form of category pluralism, by contrast, a feature that better navigates the hurdles just outlined.[13]

2. Other Perspectives on the Problem of Biological Ontology

There are two problems of biological ontology that are not my own. The first is the drafting of manifest ontologies; the second is the collection of enquiries known as The Species Problem, which include the Species as Individuals thesis. These problems overlap occasionally with the questions I pursue; however if progress is to be made, some distance must be gained from them.

For some, an ontology is an account of all of the things that are referenced in science, where 'thing' is understood very broadly. Call these 'manifest ontologies'.[14] Manifest ontologies might name all possible things, relations, processes, properties, and so on in a given realm of enquiry. Philosophers of science examine manifest ontologies because they help understand the conceptual tools and representations used in successful science. Though interesting, this is not the sort of ontology I have in mind. I stay much closer to the traditional philosophical understanding of natural kinds ontology, which will come out in the first half of this thesis.

Philosophical work on natural kinds in biology has been dominated by discussions surrounding biological species. The literature is so large as to have acquired its own name: The Species Problem. But the Species Problem is ill defined. It is actually a collection of problems, which are interesting in their own right and overlap in interesting ways with the problems pursued here. Ultimately, however, if progress is to be made on the issue of

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[12] Not to be confused with Ian Hacking's *Historical Ontology* (2004), which articulates a constructivist position.

[13] David Hull is said to have remarked that pluralism is 'the council of despair' (in Kitcher 1989, p.205). That captures my view of certain highly-permissive taxonomic and categorical pluralisms, which strike me as a reaction to intrinsic heterogeneity and theoretical pluralism that shed no light on these hurdles, merely failing to conflict with them. I discuss this in the conclusion.

[14] My understanding is that this use of the word 'ontology' is found primarily in information technology and biomedical science (as in the journal *Applied Ontology*).

biological ontology—if an acceptable theory of biological ontology can be given to my colleague—then it must be developed at arms length from the Species Problem(s) and certain entrenched modes of enquiry.

Species Problems include:

- Whether species are real
- Whether species are collections, individuals, or particulars
- Whether species or their members have essences, and what those might be
- What biologists think species are
- Whether biologists should agree upon at a single conception of species
- Whether there 'really' is a single thing that is a species
- Whether biologists' species pluralism entails (a) nominalism (b) metaphysical pluralism or (c) something else entirely

Many more Species Problems may exist.[15] From this collection and the surrounding literatures, a striking methodological feature is of note: Much of the literature on species is informed by actual scientific practices of classification. These practices are pluralistic[16], many of them fail to meet certain proposed metaphysical ideas for natural kinds, and no single practice is fundamental or primary. These facts lead many to favour nominalism, natural kind pluralism, or an alternative to the claim that species are natural kinds. This philosophical methodology stands in need of justification—thus ***Q3*.**

The main alternative to species as natural kinds is the 'radical' ontological thesis that species are individuals. Over a series of papers in the mid-1970s, biologist Michael Ghiselin (1974) and philosopher of biology David Hull (1978) offered the 'Species as Individuals' thesis. Based on the role that 'species' plays in evolutionary biology, they claimed that species, entire collections of organisms spread over time and space, are single individuals, not natural kinds. Species are enormous individuals composed of pieces, not large groups composed of individuals. For many, this is now the default ontological view of biological species.

Historically, this move followed widespread dissatisfaction with failed attempts to reconcile natural kinds with New Synthesis evolutionary theory. The problem is intrinsic heterogeneity. Natural kinds were thought to be grounded in unchanging physical essences, but New Synthesis evolutionary theory taught that even at the genetic level species were heterogeneous. Ghiselin and Hull's proposals may also be understood as reactions to the state of the natural kind debate, which had come to revolve around the belief that natural kinds were abstract entities. As an abstract entity, the 'species' would be an independently-existing

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[15] Detailed analyses of Species Problems include: (Ereshefsky 2007, 2010b, Wilson 1999b). De Queiroz (2005) articulates three scientific versions of the Species Problem.

[16] Hey (2001) counts at least 20 definitions of 'species'. Mishler and Donoghue (1982), early in the debate, remains one of the scientifically-detailed arguments for pluralism, explaining how heterogeneity in the biological world necessitates plurality in biological practices.

thing in the world.[17] This would guarantee some realism about the species, rather than just about the particulars, but it is extraordinarily difficult to establish the existence of an abstract entity. Abstruse questions over whether that entity 'contains' the organisms that are parts of it, whether it is a special type of set, or the sense in which it exists only add to frustration about species as natural kinds.[18]

There are three main arguments for the Species as Individuals View. First are claims that biological discourse talks about species (or at least populations) as though they were individual things, not collections; second is the belief that the view explains various curious facts about species and their taxonomy; third is the claim that species are a lot like other common-sense individual things in the world. There are criticisms of all of these views, while additional accounts highlight further problems with the view.[19] Slater (2013) questions the putative explanatory virtues of the account. Kitcher (1984) claims that the individuality thesis is too restrictive to be of use to biologists, while Reydon (2003) doubts that the pluralistic use of 'species' in biology supports the thesis.[20] Kitcher (1989) and Ruse (1987) deny the analogy between species and common-sense individuals.[21] Ruse (1987) also points out that the thesis must deny the possibility of organism-level selection. Crane (2004) claims that the thesis faces an insurmountable problem in the indeterminacy of species membership.[22] My own criticism, which comes in Chapter 5, will appeal to discussions of mereology, pointing out that putative biological individuals lack strong arguments for composition.[23]

Quite independently of these criticisms, the Species as Individuals view is altogether avoidable. It is motivated by failures to identify species as natural kinds and by the attendant

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[17] To say that it exists 'in the world' is confusing. Abstract entities are thought to exist in a number of ways, like Platonic ideals. They may supervene on their members or they may participate in each member. These sorts of scholastic worry are what led to frustration with natural kind talk in the first place. Fortunately, as I will argue in PART I, these worries are altogether unnecessary.

[18] Ghiselin (1974) viewed abstract classes as mental constructs, but claimed that species were not such things. Rather, they were concrete individuals, stretched through space and time. This salvaged realism about species. A token organism is not a member in a class, but a part of a whole. That whole comes into existence with a speciation event, goes out of existence with extinction, and in between contains many parts (the organisms), connected genealogically. The individuality thesis therefore secured some realism about species while sidestepping issues of natural kinds and abstract entities.

[19] Slater (2013, ch. 4), discusses much more of the relevant critical literature than I have space for, here.

[20] This is a response to Coleman and Wiley's (2001) study of biological discourse. Those authors claimed that biological discourse 'contains an ineliminable reference to individual things called species' (p.516). Reydon doubts that the discourse of such a broad discipline privileges any single ontological stance (while I doubt that ontology is helpful or appropriate in such discussions).

[21] Though Kitts and Kitts (1979) use that same analogical argument to undermine Hull and Ghiselen's theory.

[22] Crane, however, appealing to pluralism in taxonomic practice, ultimately supports the individuality thesis on the grounds that species terms are rigid designators. The point about indeterminacy also appears in Slater (2013).

[23] My target is not the species as individual thesis specifically, but rather the more general claim that any biological composites are individuals. It will be clear how the criticism covers populations/species, as well.

claim that species exist as abstract entities. First, regarding natural kinds, the view of natural kinds often considered as inapplicable to species is a straw person. Hull (1976) assumes that natural kinds, as classes not wholes, are groups of perfectly similar organisms. Intrinsic heterogeneity would therefore preclude species as natural kinds. Not only is this not necessarily true of natural kinds, a theory of biological kinds must abandon this assumption in order to represent the diversity of the biological world.[24] Second, the belief that natural kinds theory is committed to the existence of abstract entities is simply false. Platonist worries such as these need not arise. I discuss both of these misinterpretations of natural kind theory in Chapters 1 and 2. Third, for both advocates and detractors of the Species as Individuals thesis, there is an intimate connection between the ontology of species and their practical classification. This assumption plays a big role in arguments on both sides. If the Individuals thesis is a claim about how scientists do or should conceive of species, then it would be appropriate to look at present taxonomic practice and attempt to provide a concept that better fits with that practice (however individuality theorists have failed to do that (Dupré 2001)).[25] If the Individuals thesis is a claim about the ontological status of species, however, then the pragmatic practices of scientists or the discourses they have developed should matter little. Questions about how species exist, behave, change, and so on are appropriate; questions about how scientists label, talk about, or work with those changes are not. We must step back from this approach, separate the scientific and ontological issues, and re-visit the questions. This is the motivation for ***Q3***. These two errors—attacking a straw-person natural kind theory and assuming a normative role for scientific practice in natural kinds theorizing—are common to much of the Species Problem literature.

The lesson of The Species Problems is not that species are or are not natural kinds. It is not that species are or are not individuals. The lesson is that answers to questions of biological ontology require an approach that navigates scientific practice and ontology such that neither practice nor ontology dominates. If headway is to be made on the issue of biological kinds, some distance must be gained from The Species Problem. The literature is too vast and lacks a common purpose. Some philosophers are interested in classificatory practice, some in ontology, and some in a curious mix of the two. A discussion of biological ontology would do well to avoid the common examples and entrenched views that come with discussions of species, since so many carry baggage from tangentially related research projects. I do not mean to suggest that recent work on the problem is not worth pursuing; however one helpful

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[24] See discussion in Chapter 2, especially section 2.3.

[25] Dupré claims that the aims of scientific classification are far more pluralistic than the individuality thesis allows. Species, the units of classification, are not individuals, he claims. But, according to Dupré, the units of evolution *are*. The mistake, according to Dupré, is to assume that biologists are attempting to capture the units of evolution with species classifications.

way of moving forward is to first develop a framework for thinking about natural kinds independently of scientific taxonomy and secondly to consider other cases of biological kinds. I develop my framework in PART I and consider other biological kinds (biochemicals) in PART II.

3. Plan of the Thesis

This thesis is divided into two parts, each with three chapters. PART I provides conceptual background and the development of Kind Historicism, covering natural kinds, essentialism, and monism. PART II is the application of Kind Historicism to biochemical kinds and biological individuality, and a discussion of the role of natural kinds in two scientific disputes: the case of race and the nature of cognitive modules.

PART I. In order to develop Kind Historicism I must first discuss natural kinds and essentialism, and clarify the monism/pluralism distinction. Also note that this first part of the thesis focuses largely on natural kinds, not individuals, because my account of individuality ultimately piggybacks on my account of natural kinds.

The first steps toward a division of labour between metaphysics and scientific practice are made in Chapter 1. There I seek a theory of natural kinds suited to the task of answering questions about the identities of biological objects, settling on a minimalist brand of neo-Aristotelian essentialism. This is distinguished from a second view of natural kinds, which seeks to characterize groups of things that are objectively similar, such that they will support inductions. I reserve the term 'natural kind' for the former group, and call the latter 'induction-supporting kinds'. I selectively survey natural kind literature from ancient to present, showing how the realist worries of the induction-supporting camp are not the same as the ontological worries of the natural kinds camp.

Chapter 2 is a brief defence of essentialism. Neo-Aristotelian essentialism may seem strange to philosophers of biology, for whom essentialism is taboo. For this reason Chapter 2 seeks to counter a number of objections to essentialism, claiming that they attack non-essential (!) features of that view. Philosophers of biology frequently attack a straw person of essentialism, claiming that essences must be intrinsic physical properties, that essences must explain the characteristic properties of their kinds, and that essentialism requires belief in Platonic forms or Parmenedian cosmology. To the extent that any of these have been advocated by essentialists, they are accidental features of the view, not necessary ones. Abandoning these claims reveals essentialism to be a metaphysically-light doctrine, which many philosophers should find less-objectionable.

Chapter 3 begins with a conceptual analysis of the monism/pluralism distinction, before introducing Kind Historicism relative to that analysis. I examine the varieties of metaphysical monism and pluralism about natural kinds, distinguishing two. 'Taxonomic monism' is a claim about the uniqueness of kind membership, focusing on the number of taxonomic arrangements of particulars into kinds. 'Category monism' is a claim about variation within the category 'kind', focusing on the number of types of kind category in the world. Most often, philosophical attention is focussed on taxonomic claims; but categories are the more interesting target if we are to understand intrinsic heterogeneity and theoretical pluralism. The difference between the physical and biological worlds, I claim, is one of kind categories. Physico-chemical things are united into kinds in virtue of shared physical structures. Biological things are united into kinds in virtue of shared histories. Understanding the differences between the two kinds explains the differences between the world studied by biology and that studied by physics and chemistry. Understanding the peculiar features of historical kinds explains intrinsic heterogeneity.

These three analytical tools, natural kinds, essentialism, and monism/pluralism situate my approach to natural kinds, which is conciliatory to the pluralist but at the same time preserves the virtues of taxonomic monism. Pluralism comes in the admission that there are two ways in which a thing can be 'what-it-is': in terms of its physical structure, or in terms of its biological history. The preserved virtue of taxonomic monism is the definiteness of the type-identity of token particulars. For any given biological object there will be a single kind to which it belongs. So too for chemical objects. No single object is subject to kinds of two different categories. There is one world, it has an order, but the world is heterogeneous insofar as it contains two fundamentally different kinds of kinds of things. In PART II, I unpack the implications of this view. I address a problem involving natural kinds, examine the related issue of biological individuality, and look at two cases that help explicate the role of kinds (natural or induction-supporting) in science.

PART II. After PART I, three tasks remain. First, the duality of kind categories introduced in PART I leaves an obvious complication: historical biological particulars are, ultimately, masses of physical things. I am a pile of carbon, hydrogen, oxygen and many other elements before, after, and in-between. Addressing this complication requires an account of the real-world relationship between historical and structural kinds. Second, the topic of biological individuality must be addressed. Third, answering ***Q3*** requires an explanation of the roles of natural kinds versus induction-supporting kinds in actual scientific investigations. Chapter 4 addresses the first task. Chapter 5 addresses the second. The third is addressed gradually across Chapters 4, 5, and 6.

In chapter 4, I examine biochemical kinds, focussing on protein molecules. Proteins are biochemical macromolecules; that is, they are big chemical molecules that occur inside biological systems and perform physiological functions. This dual identity means that proteins are an ideal case to test the structural kind/historical kind distinction. They are at once chemical molecules and biological species. Biological kinds are formed from masses of individual chemical molecules, which are formed of aggregates of atoms. All of these are natural kinds. Most biological kinds will not, as wholes, be chemical kinds. There is no sense in which there is a single chemical molecule that is me, which is liable to be both a historical and structural kind. But cases like proteins are more difficult. Here we have singular molecules that are at once structural and historical. A number of pluralists have examined the case of protein molecules, claiming that natural kinds are at best pluralistic and at worst non-existent (Slater 2009, Tobin 2010, Goodwin 2011). Natural kinds of protein are difficult to describe because there is a radical disconnect between proteins' amino acid sequence, their final folded structure, and their biological functions. A protein with one function can have multiple structures and sequences. A protein with one sequence can fold into entirely different shapes, carrying out different functions. Structural kinds err when it comes to the biology. Historical kinds err when it comes to the chemistry. I use the protein case as an opportunity to sharpen the theory of biological kinds developed in PART I. It also sets up a discussion of the real-world relationship between the two kind categories. I describe chemical kinds as pieces or parts that get picked up, ordered, re-ordered, and used by selection and chance. It is from this process that historical kinds emerge.

In Chapter 5, I use my theory of kinds and discussion of biochemicals to address the problem of individuality. The relationship between one chemical kind and another is one of nesting. The relationship between the aggregate chemical kinds and the biological whole they form is one of composition. Nesting is easy to explain; composition is not. It is no easy feat to explicate the conditions under which some set of smaller things compose a larger one. In metaphysics the problem is called 'composition', in the philosophy of biology it is called 'the problem of biological individuality'. But the issue is the same. In referring to composite wholes, a theory of biological kinds seems to require a theory of composition. We must know which chemical particulars form parts of biological particulars, and whether there is something to the biological whole that is greater than its chemical parts.

The problem of biological individuality is a specific instance of a larger problem in mereology, known as the problem of composition, or the Special Composition Question. But there is nothing particularly special about the biological cases. There are two main types of solution to the problem. The first ties individuality to some metaphysical facts about the world. The second ties individuality to evolution. I examine one attempt from the first camp, which attempts to tie individuality to causal agency. This attempt fails because it will not secure the sorts of individuals that biologists and philosophers need, and would populate the biological world with many unwanted gerrymandered individuals. The second approach is more plausible and ties individuality into my account of biological kinds. Individuals just are whatever things are members of biological kinds. Philosophers and biologists expect individuality to tell them whether a flock of birds is one thing or dozens, whether a forest of cloned trees is one thing or many, and whether a pile of symbiotic organisms are one thing or many. Whatever the answers to these questions are, they will be supplied by evolutionary and selective histories, not physical or metaphysical (e.g. causal) facts about the objects themselves. The answers biologists and philosophers expect out of a theory of individuality will actually come from a Darwinian theory of natural kinds. The individuals are whatever clusters of matter are members of natural kinds.

In Chapter 6, I address directly the tendency among philosophers and scientists to ask what certain categories in science *are*, in a metaphysically-loaded sense. By looking at two separate cases, that of race and that of cognitive modules, I show how worries about ontological status can mislead investigation. Building on my discussion from Chapter 3, in which I explained how the metaphysics of natural kinds should be divorced from scientific classification, I explore the limited implications of finding that a scientific class does or does not manage to obtain some special status. In the case of race, debate has recently moved on from questions of natural kinds. However the new focus is on 'biological meaningfulness', which I claim is merely a stand-in for more overtly ontological statuses. Critics of racial classification claim that races lack 'biological meaningfulness' and use this fact to argue against the use of racial classification in science. Since there is no link between 'meaningfulness' and utility, I suggest ways in which the debate might productively move forward, focussing on induction-supporting kinds. In the case of cognitive modules, proponents of Evolutionary Psychology have claimed that the utility of their approach comes from an ability to carve nature at its joints, yielding cognitive modules. I criticise the evolutionary arguments in favour of this position. I then explore the presumed link between natural kinds and scientific utility, arguing that cognitive psychology, with its focus on induction-supporting kinds, offers a perfectly acceptable science of classification without any need for Evolutionary Psychology or its 'natural kinds'.

In the Conclusion, I re-visit the whole of the thesis from the standpoint of 'biological exceptionalism'. After identifying exceptionalism with intrinsic heterogeneity and theoretical pluralism, I re-trace the ways in which Kind Historicism and the general account of natural kinds developed in this thesis provide an account of biological exceptionalism. I sketch an argument whereby this account might be extended to cover the problem of laws in biology before concluding with some meta-philosophical reflections on scientific metaphysics.

4. A Note on Promiscuity and the 'Stanford School'

There exist a set of pluralist works on biological natural kinds that appear quite similar to my own, but support different conclusions. What I have in mind here are various realist pluralisms, of the sort often associated with so-called 'Stanford School' philosophy of science, particularly Hacking, and Dupré.[26] A reader of this thesis familiar with these works would be hard-pressed not to notice first their influence but second the ways in which my own account differs—at times radically. The differences between these views and my own will develop slowly, but it is worth highlighting the main sources of disagreement at the outset.

First, these views offer a brand of scientific realism true to the realist remit of belief in the content of successful scientific theorizing. Since biological taxonomy and individuation are successful, we should accordingly view the categories of biological practice as real categories.[27] Dupré (1993) offers a book full of reasons to think that many categories in biological practice map on to real, useful, objective, non-arbitrary facts in the world. You will find no opposition to that point, here. The pluralistic taxonomic practices of biology are in use because they work, and they work because they utilize objective non-arbitrary handles for classification. Where I diverge from Dupré and similar scholars is in the belief that a realist interpretation of these pluralistic practices amounts to pluralism about natural kinds.[28] Thus the second major disagreement between us is the true source of divergence: our stance on natural kinds.

I develop two accounts of kinds. One is an essentialist natural kind; the other is a pragmatically tailored induction-supporting kind. The 'natural kinds' of realist pluralists are induction-supporting kinds that utilize objective properties. I prefer my own essentialist natural kinds, believing that these are more in line with a strict investigation into metaphysics and ontology. When possible, throughout the thesis, I answer the principle objections that a realist pluralist might level against my conception of natural kinds. I do not explicitly criticise their conception except to say that induction-supporting kinds offer a weaker sense of

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[26] The motivations and dealings of this school are far greater than my own. And I should make it clear at the outset that I do not disagree with the general anti-unity-of-science stance at the core of Stanford School world. It is certainly the case that treating actual scientific investigations as aiming at unity is a foolish misrepresentation of what scientists are attempting to do. I do no such thing. A more recent articulation of this broader pluralist project, 'The Pluralist Stance', can be found in Kellert, Longino, and Waters' introduction to their edited volume on the topic (2006).

[27] Chakravartty (2011), offers the most recent and succinct articulation of this problem of scientific realism. See also (Nanay 2011) in that same volume.

[28] This point is developed in Chapter 1 section 3. See also Chapter 3 section 4.

ontology, one that I believe is more helpfully viewed under banners of ‘realism’ and ‘epistemology’, rather than ‘ontology’ and ‘metaphysics’.

I view positions like these, particularly Dupré’s ‘promiscuous realism’ (1993), as helpful attempts to articulate an account of what-there-is in biology in such a way that makes sense of theoretical pluralism. My debt to these works is huge. These pluralists and I differ in our views on natural kinds and, as a result, differ in our views on pluralism and on the role of ontology in scientific practice (***Q3***). I believe that the account I offer does a better job of explaining intrinsic heterogeneity and theoretical pluralism, while articulating a robust ontology that helps situate the metaphysics and ontology of biology relative to those of physics and chemistry.[29] But the proof is in the details, which now follow.

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[29] In the conclusion, I make this claim about Kind Historicism relative to a family of broadly pluralist approaches to biological kinds.

I

Investigating Biological Ontology: Natural Kinds, Essentialism, Monism, & Kind Historicism

1

Two Traditions of Natural Kinds

Ontological investigations take many forms. Here, ontology is pursued in the traditional Aristotelian sense of *being qua being*. This is not an ontology that drafts lists of existing things, but rather an ontology that asks how things are whatever it is that they are.[30] The issue is not what things exist, but the nature and features of that existence. Investigations of this sort are most closely associated with natural kinds. Questions about whether certain objects have natural identities and the whether those identities are uniquely determinate can be rephrased as questions about whether things belong to natural kinds and whether they belong to those kinds uniquely. In this chapter, I search for a theory of natural kinds that is fit for this type of investigation.

Ian Hacking (2007) recently declared that natural kind talk should be abolished. Perhaps this comes as a surprise to those who know Hacking as, himself, a noted contributor to the philosophical discussion of natural kinds. But during his career he became increasingly convinced that there were too many incompatible theories of natural kinds, that this incompatibility reflected an increasingly diverse range of unrelated research projects, and that the research programme (if the definite article is even appropriate) was degenerative. According to Hacking, the topic has now descended into 'scholastic twilight': debates surrounding 'an inbred set of degenerating problems that have increasingly little to do with

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[30] A wonderful recent revival of Aristotelian ontology, running far deeper than my interest in natural kinds, can be found in Schaffer (2009).

issues that arise in a larger context' (2007, p.229). I am somewhat sympathetic to Hacking's diagnosis, but disagree with his remedy.

The problem with natural kind theory is that there is no theory of natural kinds—there are many. The concept has acquired far too many meanings to be of any use. Natural kinds are assumed in some way real, juxtaposed with the nominal kinds of human convenience. On this everyone agrees. But kinds are also supposed to play important roles in human language. They are supposed to support important rules of human inference. They are supposed to be unchanging, and eternal. Differences between them are supposed to be stark, not fuzzy. Reference to them is supposed to be grounded in reference to an 'essence'. Few philosophers hold all of these assumptions, but most will hold some. Many of these assumptions are part of contemporary discussions not because they play any motivated philosophical role, but because they are part of the baggage of natural kind talk. An overarching goal of this chapter and the next is to unpack this baggage.

This chapter explores the history of natural kinds. I begin with Aristotle, then examine empiricists Mill, Whewell, and Locke, before moving on to 20th century discussions featuring Goodman, Quine, and Boyd. Two distinct traditions emerge. The first tradition, which I associate most closely with Aristotle, is comprised of theories of kinds aimed at classifying things according to natural identity. The second tradition, which begins in earnest with the empiricists and reaches its zenith in the present day, is comprised of theories aimed at providing classifications suited to scientific reasoning, particularly induction. The latter tradition dominates talk of natural kinds in philosophy of science today. It is also dominant in scientific classification. For this reason much of this chapter concerns induction-focussed kinds. Though better suited to the needs to science, this tradition is in fact poorly suited to the ontological enquiry in which I am interested.

Induction-supporting kinds require robust similarity. This presents two problems for an ontological application. First, the kinds suitable for this tradition range from the natural to the human. Many things can exhibit reliable clusters of properties, such that they are good candidates for induction. The induction tradition possesses no mechanism to limit the scope of its kinds to natural objects. A second and more serious problem for this tradition is the interest-relevance of its kind groupings. Similarity is a notoriously tough notion (see Quine 1969). Similarity comes in degrees. Similarity comes in types. The degree and type of similarity required of a kind is determined by the types of inductions in which that kind will feature. Thus proponents of induction views will speak of things being natural kinds *for* geology or natural kinds *for* mineral collectors (e.g. Boyd 1999a, p.160), determining the grain of classification. There are no natural kinds *simpliciter*. The induction kinds tradition cannot investigate ontological questions because it takes a stance on those questions: Things do have identities, but they have as many identities as there are ways to group their properties, or inductive roles they might play. Induction kinds presume a sort of pluralism. They tell us more about epistemic features of science and less about the mind-independent organisation of things in the world.

Many recent discussions of natural kinds in biology are best understood within the induction tradition. Philosophers are chiefly concerned with the relationship between kind classifications, the characteristic properties of a kind, and the ability of classifications to feature in inferences, explanations, and predictions. Yet philosophers are prone to drawing conclusions about monism, pluralism, realism, anti-realism, nominalism, and nihilism, issues better suited to the ontological tradition of natural kinds. These mis-targeted claims are perhaps invited by the realism of recent induction-focussed approaches, such as the Homeostatic Property Cluster account. However the realism on offer concerns either the causal processes that support the grouping of kinds or the objectivity of the properties used in grouping; it is not realism about the independent groupings of kinds themselves. Since induction kinds are incapable of investigating ontological questions, we cannot rely on any such conclusions. Nevertheless, that such questions are being asked at all demonstrates some interest in the ontological investigation I pursue. I conclude by briefly considering claims of this sort.

Two clarifications are in order: First, in this chapter I endorse no specific account of natural kinds, biological or otherwise. I speak broadly of two traditions of natural kinds. Within each there exist many accounts of what makes something a natural kind and what follows from membership in a kind. I evaluate specific accounts sparingly and endorse none. My own account of natural kinds will emerge in Chapter 3. Second, in this chapter the ontological tradition I endorse is an essentialist tradition, of the sort often called 'neo-Aristotelian'. Neo-Aristotelian essentialism has a very, very bad reputation in philosophy of biology, but to defend essentialism here would detract from my main argument about natural kinds, and so I ask the reader to provisionally bracket the standard objections to my view. I defend essentialism by clarifying the commitments and entailments of the position in Chapter 2.

1. Into the Scholastic Twilight

It should be clear to anyone wading into the literature on natural kinds that the waters are murky. There are as many conceptions of natural kinds as there are natural kind theorists. A survey of philosophers on the meaning of 'natural kind' would reveal an alarming lack of consensus. Hacking is not the only philosopher to notice this diversity. In the introduction to a recent monograph on natural kinds, P.D. Magnus (2012) writes,

> 'natural kind' is a term of philosophical jargon. We cannot start from a pretheoretical concept of *natural kind* and provide an analysis of it. A modern Socrates would learn nothing by asking some unsuspecting fellow in the agora what a natural kind is… We might instead ask what philosophers mean when they use 'natural kinds' … but there is no univocal answer to this either. (p.5)

Standard entries for natural kinds in our field do not even attempt to give a consensus definition.[31] They proceed as surveys of possible meanings, offering lists of frequently used criteria or desiderata. It is certainly not possible (or at any rate advisable) to deliver a paper on kinds without specifying at the outset what conception of kinds you have in mind.

The variety of meanings for 'natural kind' is mirrored in the wide range of uses to which kinds are put. Natural kinds are invoked in discussions of laws, causation, inductions and generalizations, scientific methodology, the nature of reference, modal metaphysics, scientific realism, and inference. Kinds appear not just multi-faceted but also multi-talented. Or perhaps people are just confused. Because 'natural kind' does not have a clear meaning, and because philosophers have as yet found no conception of natural kind that does not reflect the particular questions they are addressing, Hacking (2007) claims that natural kinds have nothing to offer science. When we label something a natural kind, he claims, we have achieved nothing:

> Take any discussion that helps advance our understanding of nature or any science. Delete every mention of natural kinds. I conjecture that as a result the work will be simplified, clarified, and be a greater contribution to understanding or knowledge. Try it. (p.229)

Knowing that species are, or are not, natural kinds, Hacking suggests, does not help biologists understand species, taxonomy, or Darwinism any better. The same is presumably true of cell types, chemical molecules, or any other scientific categories discussed by natural kind theorists.

Hacking's point here is a bit puzzling, connected as it is to the point about a proliferation of theories of natural kinds. But perhaps the claim is that the addition of the label 'natural kind' does not do anything to advance our understanding in the way that the addition of other predicates might. When we discover that some skeleton is a 'vertebrate', for instance, this tells us a lot of other facts about the animal to whom the skeleton belonged. It allows us to make predictions and forward explanations that follow on our knowledge of the class *vertebrates*.

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[31] The Stanford Encyclopedia of Philosophy entry 'Natural Kinds' (Tobin & Bird 2008) lists six basic properties of natural kinds. The Routledge Encyclopedia of Philosophy entry focuses only on the existence of a shared property that is 'theoretically interesting' (Daly 1998). The glossary definition in Sterelny and Griffiths' (1999) standard text on philosophy of biology focuses only on the non-arbitrariness of natural kinds.

When we discover that a classification system represents natural kinds, by contrast, no comparable predictions or explanations are licensed. Suppose I prove that the class *vertebrates* is not just a convenient group but is in fact a natural kind. Does this help me identify vertebrates better? Does it permit new or stronger inferences ranging over the class? Does it tell me something new about the properties of the members of the class? Probably not.

If this is what Hacking means then he is correct but misguided. Identifying natural kinds is not helpful in the way that identifying other predicates can be. But Hacking has attacked a straw person. Even among those traditions of natural kinds that *are* interested in helping science, they would not claim that the label 'natural kind' helps us understand the groups or objects of which it is predicated. As I will soon explain, the tradition of kinds focussed on scientific explanation aids science by helping in the formation of classes better suited to induction. The tradition of kinds focussed on ontology operates at a level of abstraction from everyday scientific practice. The target of Hacking's criticism is a theory of natural kinds that does not exist.

A parallel argument was introduced by John Dupré (1993), which is far easier to motivate. Just as Hacking sees the abundance of philosophical views about kinds as indicative of a problem, so too has Dupré, among others, used the plethora of scientific practices of classification as a motivation for re-examining the meaningfulness of the natural kind project, particularly the traditional presumption of natural kind monism.

Scientists identify all sorts of kinds. Take the common example of species. There are at least four main taxonomic methods, relying on interbreeding, morphology, phylogeny, and genetic similarity. Each of these yields a classification system that plays important theoretical and practical roles in biology, yet none of these systems classify organisms the same. A number of philosophers have rejected traditional (monistic) approaches to natural kinds on these grounds (e.g. Kitcher 1984, Ereshefsky 1992, cf. Ruse 1987). The general and very practical problem is well-described by Alan Love (2009), who demonstrates how a natural kind theorist's traditional assumption that there is just one way to represent a diverse group like species neglects the nuances of particular questions in biology, which might require alternative classification systems. These philosophers will argue that the natural kind project, insofar as it is focussed on finding a monistic select group of natural kinds, is too far removed from scientific investigation and all its diversity.

This challenge is more complicated than Hacking's. On the face of it, it is tempting to dismiss this point as misguided: Natural Kind theorists are either making claims about the structure of the world (and thus not about scientific practice), or they are making *normative* rather than descriptive claims about scientific practice (perhaps all of the scientists are using the wrong classifications, or perhaps only one is correct). I think that the first of these initial reactions is on the right track, but need not be viewed as a dismissal of the challenge. However this response will need greater attention in the face of arguments from the induction tradition, which describe inferences that should be drawn from predictive success in science to realism about the kinds used in those predictions. I will return to this challenge later.[32]

Scepticism about kinds is helpful, for it forces caution. There is no consensus about what natural kinds are, or what they do. But the mere existence of sloppy philosophy, equivocation on the term 'natural kind' across camps, or the pragmatic deployment of pluralistic classification schemes by scientists is not evidence enough to abandon natural kind talk altogether. It just means we should think about it a little more carefully.

Attempts have been made to construct grand narratives about natural kinds and science, construing the project from Locke and Mill to Russell, Quine, Putnam and Kripke, and on to contemporary usage (see Hacking 1991, 2007, McOuat 2009). Some extend the history further, to Aristotle (Ayers 1981, Reydon 2010). I will construct no such single narrative, here. There likely *is* no single narrative to be told. The projects of these philosophers were as different as their conceptions of kinds. Recent bibliographic scholarship and history of philosophy suggests that we should view talk about natural kinds in a series of episodes, rather than a coherent narrative (Magnus 2013). Some episodes have parallels with others, but few follow perfectly in the footsteps of another.

2. Early Discussions of Natural Kinds

My story of natural kinds begins with Aristotle, leaps forward to British Empiricists Mill, Whewell, and Locke, and then takes a final (small) step to Goodman, Quine, and Boyd. The empiricist interest in kinds began by borrowing an essentialist notion of kinds from Aristotle, but eventually moved on to a new account, focussing more squarely on induction. In the 20th century, talk of induction continued. And there are large gaps in the bibliographic record between British empiricist discussions of natural kinds and mid-20th century concerns, despite certain similarities between these two projects (Magnus 2013). After each period of inactivity, talk of natural kinds was not so much resurrected as introduced anew, with slightly new conceptions of kinds fit for new philosophical problems. This episodic and disjoint history explains the patchwork that is the modern conception of kinds. To try to connect all of these episodes is folly, but to understand how each set of problems shaped its own theory of kinds is to understand how the contemporary conception became so muddled.

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[32] This is discussed at the end of the chapter. Over the course of the thesis, my answer to ***Q3*** bears on this issue. My claim is that we should view theoretical pluralism as a reflection of induction-supporting kinds, and that this should be kept separate from worries about *natural* kinds. My view thus aligns with that of Dupré, but differs substantially in its premises.

These three episodes are used to illustrate two very different types of project. Rather than attempt to cover the totality of Aristotle's complex views on kinds, I focus simply on his ontological project and his use of essentialism. In contrast with this Aristotelian project and its interest in the ontological structure of the world, I present the much more practical project of the British empiricists. Their aim was to erect categories of objects that would make good tools for scientific reasoning. Many have followed in their footsteps, fewer in Aristotle's. In the past century philosophers once connected kinds to scientific inquiry, lately combining it with a form of scientific realism, seemingly uniting the practical epistemic roles of natural kinds with ontological investigations. In section 3 I will claim that this scientific realism is not equivalent to the ontological and metaphysical interests extracted from the Aristotelian project.

2.1 Aristotle

Confusion about Aristotle on kinds abounds, because he moved from epistemology to metaphysics and through philosophy of language. For this reason I will abstract a theory of kinds from the rest of Aristotelian epistemology, philosophy of language, and metaphysics, separating the three strands where possible. The stripped-down result is a theory of kinds aimed at understanding not what kinds exist, but how they do so. I am interested in the specific concept of essence and the analytic ontological role it plays.[33] Within Aristotle's thought this theory applied to many kinds of objects that few would recognize as natural kinds. On its own, however, the stripped-down theory of kinds provides an excellent tool for asking after the ontological standing of certain classes of object. Aristotle did not use the phrase 'natural kinds', which is one reason why it is difficult to form a unified account of his views. However his work on the nature of *substances* has informed many modern discussions, and so this is where we begin.

Aristotle was concerned with the types of things in the world. Specifically, he was interested in the most natural or basic things in the world. Aristotle called these things 'substances'. Substances are 'that of which everything else is predicated, while it is itself not predicated of anything else' (1028b36). In other words, substances are things, not properties. At one point Aristotle took living creatures as the prototypical substances, later he appears to have moved toward smaller and more abstract objects. Aristotle's *Categories* gives the examples of 'horse' and 'man', while in the *Metaphysics* he provides examples ranging from the chemical elements to mathematical objects. At the same time Aristotle was working with the

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[33] This is to be distinguished from the very different epistemic role, which is incompatible with its ontological role unless it were the case that essences guaranteed perfect similarity among kind members. See Chapter 2.

notion of essence. It, too, changed as time passed. Interpreters have struggled to regiment these discussions (Ayers 1981, Cohen 2012, Makin 2009, MacLeod 2010).

What is it to be a substance? This is not the question of what sorts of things are substances, but what it is that *makes* a substance the substance it is. What is it that makes one lump of matter a cat and another lump a man? For Aristotle, substances are what they are in virtue of their *to ti ên einai* (literally 'the what it was to be'). He sometimes used the shorter phrase, *to ti esti* (the 'what it is') but this is not much more informative. When translated from Greek to Latin these phrases became *essentia*, which gives us the modern term, 'essence'. The essence of a substance is that which makes something what it is. The essence is thus something a thing cannot lose without ceasing to be what it is.

Essences are analytic. A distinguishing feature of substances, especially living beings, is that they are complex. A living creature is a developing network of complex and integrated parts. Aristotle worries that this observation drives the intuition that these parts are more basic than the whole, or that the whole is merely a network of parts, not a unified singular thing (*Metaphysics* 7.17). To address this worry, Aristotle reckoned that something had to unify the componential complexity. Whatever that unifying feature is, it cannot be merely another part of the whole; otherwise we would need to know what unifies *it* with everything it unifies. This unifying thing must be a 'principle', says Aristotle, rather than another element. That principle is the 'what-it-is-to-be'. It is the essence. Aristotle suggests that when substances are 'formed by nature', as opposed to artificially, then their essence 'would seem to be this nature, which is … a principle' (*Metaphysics,* 7.17). These essences are something *de re*, rather than *de dicto*. They are in the substance, not predicated of it.

Appeal to essences in turn permits a discussion of types, as in *Categories* and *De Interpretatione*. Objects can be viewed as differing in matter but the same in substantial form, or essence. Two individual people occupy different and different looking bodies, but are both still humans. The essence is what makes any given person a 'human'; sharing the same essence makes the two people of the same kind.[34] The theory of substance and essence grew complicated as Aristotle noticed that one thing can belong to different types. An apple is an

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[34] It is here that great controversy enters. Some wonder whether Aristotle meant to claim that co-typical individuals share the same essence, or whether each individual has a separate essence of the same type. We might wonder further as to the ontological status of essences on either picture. If they are universals, do they exist independently 'out there', or do they exist imminently in the particulars that instantiate them? The same questions can be asked of the *secondary substances* themselves. Are we to expect that the type 'Human' is floating somewhere in the ether, as a Platonic form? Cohen (2012) refers to this collection of problems as 'perhaps the largest, and most disputed, single interpretative issue concerning Aristotle's *Metaphysics*'. It is present in ancient Aristotelian-Platonist debates, was picked up in medieval times by Boethius and Ibn Senna, and continues to the modern day. Fortunately I need not weigh in. What matters for present purposes is the general notion of essence and its relation to what we would now call a theory of kinds.

apple, but is also *fruit*, *round*, *red*, and so on. To correct for this, Aristotle introduced a hierarchy of substances. To translate this into the present investigation, natural kinds are the 'primary' or most fundamental substance.[35]

It is tempting to think of essences as intrinsic physical properties, but Aristotle had a much more liberal view. Though he never explicitly details the sorts of things that can serve as essences, his use of 'rationality' for humans suggests that materiality is unimportant. Rather than speaking of essences *as* stuff, he talks about essences being somehow *in* the stuff from which a thing is made. You are a person, he claims, because the essence of person is present in the meat that comprises you. Aristotelian essences are 'occult', to quote Ayers (1981), 'consisting in powers or functions such as, in the case of man, rationality. We have to conceive of them … as the law or tendency governing the behavior of the kind' (p. 254).[36] Essences are the property that makes a thing what it is, and that a thing cannot lose without ceasing to be what it is. An essentialist is not necessarily committed to any specific stance concerning the types of properties that can serve as essences.

Before moving forward, I would like to acknowledge the confusing relationship between Aristotle's discussions of ontology and his discussions of scientific methodology. My intention in this section is simply to introduce the notion of essence as a tool for ontological theorizing. That much Aristotle provides. However, Aristotelian essences have been put to many more uses, some (perhaps) by Aristotle himself. Often, Aristotelian essences are given epistemic roles in science. Whether this is advisable depends on features of the world being investigated (as discussed in Chapter 3). Whether this is what Aristotle intended is unclear, but, for present purposes, unimportant. The following should provide some sense of the complicated relationship.

On a simple reading, it might be thought that, for Aristotle, the epistemic role of essence flows from its ontological one: To know an essence is to know the substance, in a way, and so scientific understanding should aim at knowledge of essences. Similarly, essences might be thought epistemically useful for picking out kinds because they divide kinds at a corresponding ontological level. On this interpretation, epistemic and metaphysical essences are one and the same. However this is difficult to square with many of Aristotle's writings on classification, which often do not mention essence and which generally advocate a pragmatic pluralism. Pellegrin (1982) claims that Aristotle treats classification as a pragmatic exercise, tailored to the needs of the investigation. In contrast, Henry (2011) reads Aristotle as a realist

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[35] The primary substance is thought to 'ground' the higher substances (properties). Thus *apple* grounds *red* and *round*. This tradition has recently witnessed a modern revival in analytic metaphysics and ontology. See (Schaffer 2009).

[36] Chapter 2 will explore some of the ways in which modern appeals to essence have shied away from the occult, tending to stick much more closely to intrinsic material essences.

pluralist, sampling a variety of non-interchangeable but nevertheless real classification schemes. It is also possible that Aristotle held one privileged class of natural kinds (called 'Great Kinds', '*megista genê*') but recognized that pragmatic classification must occasionally deviate from the privileged class.[37]

It is also within this talk of epistemology that we get a discussion of the relationship between essence and the proprieties of a kind. For Aristotle, a full scientific account of a thing includes not only the essence but also the characteristic properties. Modern versions of essentialism sometimes assume a causal relationship between essences and properties, but that is not consistent with Aristotle's view in the *Categories*. He presents a contrast between what a substance *is*, its essence, versus what it is like. A person is *human* but is like *pale* or *short*. The latter can change, ruling out a causal relation.[38]

The interpretation of Aristotle's claims about substance, essence, properties, and kind are a matter of considerable academic debate. Even the most charitable treatments have trouble reconstructing the entire account across the *Metaphysics*, *Categories*, *Posterior Analytics*, and *Logic*. His discussion of (what we now call) kinds is problematically tied up with his theories of language, reference, scientific inquiry, metaphysics, and ontology. For present purposes, consider just the type of question he asked and the tools he developed to help answer it. Aristotle wondered whether some of the things we find in the world form kinds, and what it might mean for objects to be members of kinds naturally. He gave an affirmative answer to the first question and provided the theory of essence to fill in the details.

2.2 Three Empiricists on Kinds: Locke, Mill, and Whewell

The empiricist engagement with kinds began with Locke's discussion of Aristotelian substances and essences. As an empiricist and mechanist, Locke's focus shifted away from ontology and metaphysics, and toward epistemology. This shift came full circle with Whewell, who sought to connect epistemology and ontology by positing a realist method for verifying scientific kind classifications.

Locke introduced a nominalist approach to kinds (1689). He was, among other things, attempting to reconcile an Aristotelian ontology and scientific epistemology with the 17th century mechanist worldview. The emerging ontology of the day did not allow the Aristotelian 'occult' essences needed to bestow kind membership. Instead, Locke argued, what makes things members of a kind must be physical sub-microscopic essences. His conception of essence otherwise mirrored that of Aristotle: 'Essence may be taken for the very

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[37] In this case, Henry (2011) can be read as an argument for the claim that Aristotle's deviant pragmatic classifications were still objective, and thus real, rather than natural kinds.

[38] I discuss this at length in Chapter 2 (section 2.2)

being of any thing, whereby it is, what it is' (II.iii.15). Yet these sub-microscopic essences are beyond our ken, according to Locke. In an attempt to map the unknowable essential structure of things, humans rely on our subjective grasp of the similarity among visible properties, since this provides the best available epistemic handle for classification (see Ayers 1981, Jones 2014).

Lockean kinds are nominal, grouped by humans using properties as 'nominal essences'. Humans group objects into kinds using widely-shared properties as epistemic handles. Locke called these 'nominal essences'. We should not, Locke claimed, believe that nominal essences carry any metaphysical weight; they are purely matters of convenience and utility.[39] Though it is nature that makes things members of kinds, it is human understanding that classifies.

There is a tension between Locke's empiricist nominalism about kinds and the similarly empiricist project of characterizing rational principals for induction. It is a fact about kinds that they are or are not suitable for use in inductions. This is a fact about the kinds, not about the way that we happen to sort them. If we are to use kinds in inductions, we are only justified in doing so if we have some knowledge of this suitability. But we cannot hope to have this justification, since we cannot know anything about nature's kinds. We know only facts about how we happen to sort things in attempts to reflect nature's kinds. Locke's empiricist embargo on metaphysical knowledge dictates scepticism about the possibility of scientific knowledge.[40]

Over a century and half later, Mill was less sceptical about scientific knowledge than his predecessor. He never used the phrase *natural* kind, though he is frequently cited as having done so.[41] Mill's theory of 'Kinds' (Mill used a capital 'K'), simpliciter, was presented in *A System of Logic*. Kinds distinguish classes of things that are well suited to scientific inquiry from those classes that are ill suited.

Mill's focus was on induction—projective inferences from examined to unexamined cases. Since this type of inference requires uniformity in nature, Kinds must be groups with the relevant type of uniformity.

> We must first observe, that there is a principle implied in the very statement of what Induction is; an assumption with regard to the course of nature and the order of the universe; namely, that there are such things in nature as parallel cases; that what happens once, will, under a sufficient degree of similarity of circumstances, happen again, and not only again, but as often as the same circumstances recur. This, I say, is

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[39] From this point onward, 'essences' in the induction-focused tradition are purely epistemic (handles for classification) not metaphysical (properties that bestow identity).

[40] For Locke, we have real knowledge when our grasp of nominal essences happens to correspond to *real* essence. In the case of material objects we can never verify such correspondence. He believed, however, that nominal essences in math and morality (!) corresponded to real essences, permitting mathematical and moral knowledge.

[41] As in the Stanford Encyclopedia entry (Tobin & Bird 2008).

> an assumption, involved in every case of induction. And, if we consult the actual course of nature, we find that the assumption is warranted. The universe, so far as known to us, is so constituted, that whatever is true in any one case, is true in all cases of a certain description; *the only difficulty is, to find what description.* (1882, p.223 emphasis added)

It is no straightforward matter to determine which description or organization of things will do. Kinds must not only group things that are alike, but group things that are alike in the correct way. It is easy to stipulate a class of objects that share a property, but these are not necessarily Kinds. Take the class of 'round things'. This class is not a Kind because round things will not have anything in common beyond shape. There is no deep commonality between members of the class.

> [E]ven the strongest understandings find it difficult to believe that things which have a common name, have not in some respect or other a common nature; and often expend much labor very unprofitably (as was frequently done by [Plato and Aristotle]) in vain attempts to discover in what this common nature consists. (p.67)

Classes such as this do not help us learn anything about their members that is not specified in the stipulation of the class. Of members of the class 'round thing', we know nothing for certain other than that they are round. Scientific inquiry requires broader uniformity:

> The ends of scientific classification are best answered, when the objects are formed into groups respecting which a greater number of general propositions can be made. (p.499)

Members of Kind must be similar in indefinitely many ways and the multitude of shared properties should not simply follow analytically from one another. Not only does this allow us to infer a multitude of facts about an object based on its membership in a Kind, it also ensures that inquiry into those Kinds is fruitful.

> [A] hundred generations have not exhausted the common properties of animals or of plants, of sulphur, or of phosphorus; nor do we suppose them to be exhaustible (p.97)

Mill did not start with a theory of kinds and then prove that they are apt for induction. Rather, Kinds *just are* whatever classes are best for induction.

William Whewell, Mill's contemporary and academic antagonist, also assumed that natural kinds were groups with robust likeness relations. Like Locke, however, he shared some empiricist scepticism about our ability to know or verify those relations. He nevertheless went beyond Mill's account by recommending a realist inference: for Whewell, the repeated successful use of kinds in science was evidence of their reality.

Whewell (1858) started from the observation that likeness was not a clear-cut relation:

> Upon what principle, under what conditions, is the idea of likeness thus operative? What are the limits of the classes thus formed? Where does similarity end, which induces and entitles us to call a thing a [member of a kind]? (p.99)

No universal set of necessary and sufficient likeness relations make all objects members their kinds. It is not just that these conditions are difficult to specify, it is that they are impossible.

Kinds can be tested, however, through their repeated use in scientific reasoning. The kinds that pass the test of induction are those that are most robust:

> … the Condition which regulates the use of language is that it shall be capable of being used;- that is, that general assertions shall be possible … the condition of use of terms is the possibility of general, intelligible, consistent assertions. (p.100)

What makes Whewell's theory particularly illuminating is the context. He was attempting to stand outside of disputes between ontological realists and scientific pragmatists. Working on geology, his realist contemporaries argued over whether nature's geological joints were marked chemically or mathematically. His Lockean pragmatist contemporaries, by contast, simply sought the most useful system of classification based on essences.[42] Though he opposed essentialist realists, Whewell was no nominalist. He argued that we could infer the naturalness of a taxonomic arrangement from its epistemic success. If a taxonomic arrangement supports inductions, especially if a few distinct methods of sorting the same materials supported the same inductions with the same or similar taxonomies, then we could presume the identified kinds to be natural kinds.[43,44] Interestingly, however, he claimed that this did not tell us anything about the method used to arrive at that arrangement, since we may well reach a correct taxonomy by way of inaccurate assumptions about the essences of the objects being classified. Thus the wrong use of essence could still land on the right taxonomy. A taxonomy of kinds should be presumed natural on the basis of inductive success, but inductive success not vindicate whatever ontological assumptions about those kinds were used to erect the taxonomy.

2.3 Kinds in the Twentieth Century: Goodman, Quine, and Boyd

Mill's approach was influential in the short period immediately after his *Logic*. This is most notable in the writings of John Venn (of diagram fame), to whom Hacking (1991) erroneously credits the phrase 'natural' kind as an adaptation of Mill.[45] By the turn of the century discussions of natural kinds had fallen out of fashion (Magnus 2013). A similar tradition of kinds soon sprang up, however, to which we now turn.

Mid-twentieth century concern with kinds was brought about primarily in reaction to Goodman's (1955) *New Riddle of Induction*. Goodman's *New Riddle* states a now-familiar

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[42] Whewell believed the former approach to be ultimately unverifiable and both approaches to be dangerous, since each focus on static definition and took attention away from the constant flux inherent in the natural world.

[43] Ruse (1987) uses this argument to support the claim that species are real, though perhaps not (Aristotelian) natural kinds or individuals. He sees the convergence of multiple species taxonomies as evidence of their reality or objectivity, in direct analogy to Whewell's geological case, discussed in (Ruse 1978).

[44] Henry (2011) offers a reading of Aristotle that appears to utilize the same type of realist inference.

[45] On Hacking's error, see Magnus (2013).

problem. It has been used to many ends, but we will restrict ourselves to its relationship with kinds and induction. We are asked to imagine a property called 'grue'. Objects are *grue* if observed before now to be green, and if observed after now to be blue. This means that all objects until now observed exhibiting greenness have also been consistent with *grue*ness. If this is the case, then we can make the *prima facie* bizarre claim that all emeralds are grue, along side the much more acceptable claim that all emeralds are green. Grass is grue; there may be grue apples. We might ironically re-name coniferous trees 'evergrues'. Something is amiss.

The *New Riddle* motivates two claims. First is the claim that there must be something that distinguishes predicates like 'grue' from predicates like 'green' and 'blue'. What is it about the predicates blue and green that make their instances count as evidence toward inductions, and why does grue lack this quality? Second is the claim that induction is not a purely syntactic matter. We tend to think of inductions as observations of predicate application followed by generalization. Inductions begin with the observation that the As in a sample are P, and proceeds to the general claim that *all* As are P, or that future As are likely to be P, and so on. The *New Riddle* shows that the meaning of 'A' and 'P' are just as important to induction as their syntatic relations. To join the two claims: Goodman's puzzle forces us to concede that there must be something special about the predicates 'blue' and 'green' that makes them suitable for induction. Whatever this special feature is, grue does not have it.

Goodman calls this special feature 'projectability', since the inductions in question are 'projective' inferences. Thus green and blue are projectible predicates, while grue is not. The question remains, however, as to what 'projectibility' is.[46]

It is interesting that we haven't settled on grue as a category, nor any other of Goodman's bizarre non-projectible categories. In general, humans seem rather good at deploying categories, a feat that is doubly-impressive when we remember that most people cannot give an account of projectibility. Goodman's solution was thus to claim that certain predicates are 'entrenched' in our conceptual vocabularies. Of all the predicates that we could use, we settle on those that are very good at informing reasoning, inferences, etc. Blue behaves well in inferences; *grue* does not. It is these projectible predicates that become entrenched. Much like Whewell's Kinds, Goodman's projectible predicates must pass the test of reasoning.

Following on from Goodman, Quine (1969) unpacked projectibility as a feature of predicates that is grounded in similarity-based grouping heuristics. We seem to know that

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[46] The easy answer is to claim that 'green' and 'blue' are primary, given that 'grue' is defined in terms of them. If grue is decomposable into green and blue, then they seem the more fundamental properties. Perhaps projectability is simply a matter of primacy. But this answer rests on the rather coincidental fact that we have come up with the terms 'green' and 'blue' first, rather than the other way around. To life-long users of 'grue', the terms 'blue' and 'green' would surely appear secondary.

blue things form a more coherent group than non-blue things, or than grue things. Blue things are more similar to other blue things than non-blue things are to other non-blue things. Grue things are extremely internally dissimilar: some are green and some are blue! In this light 'blue' seems natural, whereas the 'grue' seems artificial. It makes *prima facie* sense to group things in this way.

According to Quine, projectible predicates are those that are true of things in a kind. The puzzling feature of Goodman's puzzle is simply that we lack an adequate notion of kind. But we risk here replacing the question of projectibility with the question of natural kindness.

> Two green emeralds are more similar than two grue ones would be if only one were green. Green things, or at least green emeralds, are a kind. A projectible predicate is one that is true of all and only the things of a kind. What makes Goodman's example a puzzle, however, is the dubious scientific standing of a general notion … of kind (p. 42).

In a now-familiar move, Quine identified kind relations with similarity.

> The notion of a kind and the notion of similarity or resemblance seem to be variants or adaptations of a single notion. Similarity is immediately definable in terms of kind; for things are similar when they are two of a kind. The very words for 'kind' and 'similar' tend to run in etymologically cognate pairs… we cannot easily imagine a more familiar or fundamental notion than this (p. 42).

Quine's move is a productive one. He began with a puzzle about induction and ended with an account of natural kinds.

Quine ultimately argued that the kinds we have are the result of long processes of conceptual evolution. Those kinds that are not projectible, or are poorly projectible, get replaced with categories that are more projectible and thus better kinds. Even though we lack an understanding of similarity, we have a refined mechanism for seeking it and grouping things accordingly.

Quine's conception of natural kinds explicitly focuses on the need for similarity, but runs shy of explaining what similarity actually *is*. This is no shortcoming, however. Quine argues convincingly that there simply is no objective notion of similarity. The concept is 'logically repugnant'. Like our kind terms, the type or degree of similarity we seek seems to have been refined over time. Different investigations, in different theoretical contexts, will require different standards of similarity. The point of Quine's paper is thus not to present a theory of natural kinds, but to argue that scientists should aim at replacing their pre-theoretical notions of similarity with more refined, discipline specific rules. This is the mark of a mature science.

It is striking how these mid-20th century are reminiscent of the older discussions just surveyed. Oddly, Quine gives no mention of Whewell's realism about natural kinds, with which he had much in common. However Quine stops short of certain claims from the earlier empiricist. Rather than offer the claim that successful induction permits realist assumptions about kinds, he believes that the epistemic success of our inductive practices is evidence for the robustness of the groupings on which those practices are based. The objective similarity of these groupings is reflected in the successful inductive inferences they license. Quine and Whewell both licence inferences from inductive success, however Whewell's inferences concern the reality of groupings, while Quine's concern their robustness.

In recent years a new theory of kinds has arisen, Richard Boyd's 'Homeostatic Property Cluster' (HPC) theory (Boyd 1991, Boyd 1999b, Boyd 1999a).[47] The view has come a long way and is arguably the most popular view of natural kinds among contemporary philosophers of science. Boyd tries to provide a theory of kinds that is true to the empiricist aim of specifying the conditions of induction while also maintaining a commitment to realism, which is needed for but (seemingly) at odds with the prior commitment. Boyd (1999a) sees himself as resolving Locke's impasse, justifying induction absent knowledge of metaphysically spooky identities. He aims at a theory of kinds that is mind independent but admits a role for human construction of kinds, and yet still possesses a mechanism of epistemic justification concerning the principles of induction and scientific reasoning. It is this justification that is missing from Quine and Goodman.

HPC theory shares with Whewell, Quine and Goodman the theory of kind term entrenchment. Boyd also shares with Whewell the realist claim that successful induction is evidence of the naturalness of our categorizations. Boyd differs from these philosophers, however, by providing a much more substantial theory of what the natural kinds actually are, and explaining just what it is that makes them natural. The key is to ground the similarity of kind members in the causal processes that sustain kind membership.

Boyd claims that kinds are characterized by clusters of properties. He adds the requirement that these properties coincide non-accidentally. Their co-occurrence should be the result of systematic and sustained causal tendencies—what Boyd calls 'homeostatic causal mechanisms'.[48] For a class of putatively kindred objects, there will be some set of properties that most of the objects share in common. They share these non-accidentally. The properties are the result of a number of causal mechanisms that happen to be relatively stable in our world. Because some accidental features of our world might interact with the causal mechanisms differently, certain members of the class will have only some of the common properties. It is not the case that all members will have all of the same properties; this is an expected consequence of dispositional causal natures and an ever-changing world.

Take the HPC kind 'lemon'. They look, smell, feel, and taste like lemons. I won't bother you with the details. These properties of lemons are not accidental; they are the product of

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[47] Though now the most popular theory of natural kinds among philosophers of science, Boyd's theory originally comes to us from his work on moral realism (1988).

[48] By homeostatic, Boyd means simply that they are relatively stable.

inheritance, genetics, epigenetics, nutrition, temperature, pressure, and many other relatively stable factors. We can imagine how temperature interacts with pressure to influence epigenetic factors; how molecular mechanisms dictate the stability of genetic elements; how nutrition affects the rate of replication; etc. These causal mechanisms are interdependent. This is homeostasis. This homeostasis causally explains why the properties of lemons tend to cluster. The mechanisms behind one property are dependent on the mechanisms behind another, and so on. Like a house of cards, they keep each other in order.[49] Importantly, these mechanisms also explain why some lemons are smaller, sweeter, or waxier than others. Though lemons tend to pass on genes perfectly, molecular mechanisms occasionally breakdown and result in mutants. Though temperature is often uniform in tropic lemon-groves, off years are inevitable. Changes in some of a lemon's clustered properties are the unavoidable result of breakdown in one or more causal mechanisms. Any farmer could have told you that.

One might wonder how we can *know* that a set of properties cluster because of a 'systematic, causally sustained tendency', rather than by accident or happenstance. It is here that Boyd employs the familiar claim that inductive success is evidence for the accuracy of our kinds. When scientists deploy kind terms, and do so successfully over time, what they have done is find ways to fine-tune their use of language to the causal structures of the world. Boyd calls this 'accommodation'. In a bizarrely apt metaphor, Boyd characterizes the process as akin to the passing of a bill through the houses of Parliament and Lords:

> Think of natural kinds as being established by a sort of bicameral linguistic legislation in which we and the world jointly legislate. Our legislative role consists of implementing disciplinary matrices with their associated accommodation demands. The legislative role of the world consists in determining how and to what extent those demands can be met. Together we thereby establish the explanatory definitions of natural kinds (1999b, p.89)

For Boyd, the study of natural kinds just is the study of how scientists accommodate their classificatory language to the world. This in turn explains how we can have rational principals for induction: the successful use of natural kind terms gives reason to believe that these kind terms accommodate the causal structure of the world. Accommodation, or belief in it, justifies our inductions. It is an abduction about induction.

Boyd is no stranger to Quine's claims that the kinds scientists use are inductively successful only against a given theoretical background. Accordingly, Boyd allows that the natural kind terms we use are a function of our interests and focus, and are shaped by the theories within

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[49] Boyd claims that the point is not just that there are similar processes going on behind each lemon, but that this *single* set of causal processes is behind all lemons. This allows him to avoid worries about whether two sets of causal processes are the same or different (for instance if the process at work in lemon groves in Florida is the same as those in Spain).

which they are formed. These kinds are nonetheless not wholly constructed, since the world, not the theory, imposes the causal structure that language must accommodate.

> The use we make of reference to the kind in induction and explanation requires that it be defined by a set or cluster of properties whose membership is determined by the causal structure of the world and is thus, in a relevant sense … independent of our conceptions or theorizing (1991, p. 129)

This allows a role for *a posteriori* creation of natural kind systems that can map to the causal structure of the world in a way that supports induction—Locke would be pleased.

3. Lovely Theory, but What Can it Do?

This truncated history of natural kinds discussion reveals two distinct traditions. The more popular tradition is pragmatic, aligning kinds with induction and scientific inference. Less popular is the tradition interested in metaphysical and ontological questions about what makes things 'what-they-are'.[50] I stated at the outset that my interest in natural kinds was tied to questions about the human-independent identities of objects in the world. In this section, I evaluate these theories in light of this task. Both traditions proffer views on kinds that permit degrees of realism. This is the best place to start thinking about kinds and ontology. The realism of the HPC theorists concerns (at best) a certain causal structure of the world. The realism of other induction theorists concerns the objectivity of the similarity relations amongst kind members. The realism of the ontological tradition concerns the identities of the entities themselves, and is therefore the best choice for my aims.

3.1 The Induction Tradition

Boyd's HPC theory is in certain respects the culmination of Mill, Whewell, and Quine's concern with scientific reasoning. These authors share a great deal, and give us the requirement that natural kinds feature in induction. This comes with attendant claims about kinds and similarity.

These theorists did not find that natural kinds in the Aristotelian sense also happened to be good for inductions. Nor did they find that the things that do well in inductions happen to be Aristotelian kinds. Rather, the theory of induction-supporting kinds is built around similarity in order to work in scientific reasoning. A relationship between kinds and induction is assumed from the beginning. Boyd is explicit on this point:

> It is a truism that the philosophical theory of natural kinds is about how classificatory schemes come to contribute to the epistemic reliability of inductive

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[50] Plato recognized this distinction, too. In *Statesman* 236D, in a discussion about classification, an interlocutor explains that, if cranes could talk, they would divide the world into cranes and non-cranes. The charge is that classification is always relative to the interests of the classifier—and thus always anthropocentric. The theory of Forms offers an alternative, classifying the world according to natural divisions, carving nature's joints.

> and explanatory practices. Quine was right … that the theory of natural kinds is about how schemes of classification contribute to the formulation and identification of projectible hypotheses (in the sense of Goodman). (Boyd 1999a, p.146)

It is not the case that these philosophers start with a conception of kinds and later find that it happens to be well-suited to inductions. Rather, they start with the need for groups that allow for scientific reasoning and then call whatever it is that fills this need 'natural kinds', 'Kinds', or Homeostatic Property Clusters.

Focus on induction yields a notable outcome. The kinds of things that feature in inductions need not be based on natural groupings, only objective ones. Thus 'natural' takes a very different form for these views. Take HPC theory. Boyd's HPC kinds are realist. Reality on the HPC view comes from accommodation. Accommodation ensures that property clusters are non-accidentally clustered; they are causally clustered. Clusters are out in the world, rather than in our heads. So realism or naturalism on this view is akin to objectivity. The clusters are real insofar as they are *really* out there, clustered. This avoids the mind-dependence of property clusters but not the human-dependence of the kinds. Human kinds, after all, can exhibit causally-supported clusters of properties—they can be HPC kinds.

Induction is supported by all sorts of groupings. Since the worry in induction is about consistency, inductions will range successfully over any groupings that exhibit robust similarity. These need not be divorced from humans. Consider the kind *classic car*. In many circles this kind is entrenched and supports a plethora of inductions. Knowing that a certain Volkswagen Beetle is a *classic* tells me, for instance, that it is at least 25 years old, that it likely runs off of a carburettor rather than fuel injection, that the value will be better than a slightly newer but non-classic Beetle, and that my auto insurance will fall into a certain category. There are many more inferences to be drawn about the mechanics, operation, and value of the car. *Classic car* supports induction because it is robustly internally similar and is entrenched, not because classic cars would exist without humans or because *classic car* is in any traditional sense natural. Induction is blind to the natural/artificial distinction.

Another, more interesting, feature of induction kinds is the way in which even seemingly human-independent kinds still require human intervention. There are many ways in which things can be similar or dissimilar—as many ways as there are properties. A non-negligible set of these similarities will be causally sustained and so will permit inductions. The same is true of degrees of similarity. It is for humans to wade in and determine which similarity metrics are the relevant ones and how much similarity is enough. Consider the mineral commonly known as 'Jade'. Jade is actually a class of two separate silicates, Nephrite and Jadeite. The outward properties of these two stones are similar enough that they are still classed as being of the same type. Both are green, reasonably hard, polish to an oily sheen, and so on. Even at a certain chemical level they are similar; both are silicates. Yet, at a deeper chemical level the two are actually quite different. Nephrite is a silicate of Calcium and Magnesium, where Jadeite is a silicate of Sodium and Aluminium. They have different molecular weights, slightly different crystallization systems, and different optical properties. So Jade might be described as either one homeostatically maintained class or two, depending on which properties we choose to include in our analysis. Which properties are relevant? It depends on what you plan to use the categories for. Surely we could come up with a list of social factors that lead these to both be called 'Jade'. Yet we can specify a different set of social factors that lead jewellery specialists to care about the differences between Jadeite and Nephrite. In certain circles it might be helpful to group the two, in others they might be best kept separate. For a theory like HPC, this plurality does not matter. *Jade, Jadeite*, and *Nephrite* are all natural kinds. All three really represent the causally sustained clumpings of properties in the world. All three are entrenched. *Jade* is a natural kind for certain fields; *Jadeite* and *Nephrite* are natural kinds for others. Because the world contains so many clumped properties, humans have to enter the picture and determine which clumps are the best clumps to name. This is all still objective (since the similarities *really* exist) but it is not human independent because humans wade in and determine the similarities that matter in a given context.

Quine (1969) was aware of this limitation. He concludes that there is no objective notion of similarity and that in devising natural groupings humans will always be needed to wade in and determine which similarities matter, and how much similarity is similarity enough. It is for this reason that Quine viewed natural kinds as prescientific, claiming that it is the job of a mature science to eliminate the need for such categories.

These problems might explain the absence of 'naturalness' from many discussions of kinds in induction. It is interesting that Russell (1948), writing before Quine but long after the three empiricists, also notes that the connection between kinds induction entails a certain nominalism about those kinds. In a move that has puzzled some interpreters (Magnus 2013) Russell claims that *natural* kinds are not used in the actual scientific practices of induction. Induction requires only reliable correlations of properties. In other words, Russell thinks that induction does not require natural kinds, merely groups based on similarity.[51] Though we don't know exactly what he thinks natural kinds *are*, he implicitly separates *natural* kinds from the groups or sets used in scientific reasoning. Recall now that Mill and Whewell also

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[51] In a footnote, Magnus (2013) confesses: 'I am not entirely clear on what the contrast is supposed to be. On Mill's view, as we saw above, regularities of correlation just are natural kinds.' Given his aim of tracking the development of the induction-focused tradition, Magnus understandably misses the possibility of the other tradition of kinds. Russell seems to assume that natural kinds latch onto deeper metaphysical facts about things, whereas similarity is a superficial (albeit useful) fact.

avoided 'natural', using 'Kind' for a special type of set. Quine's theory is properly an account of the predicates that feature in induction, not natural kinds in any metaphysical or ontological sense. Similarly, Boyd gives an account of the categories that are required for scientific reasoning, characterizing these as HPCs.[52] Though many of the progenitors of this tradition of induction did not regard their kinds as *natural* kinds, followers have not been so careful.

I am perfectly happy to claim that HPC kinds (as pinnacle of this tradition) do indeed describe the categories of scientific inquiry in a way that is both realist and conciliatory to constructivists. The theory is robust and well articulated. One question is whether we should call this or any other theory of induction-supporting kinds '*natural* kinds'.[53] This is a purely semantic dispute, which I see no hope of settling. It would not even be a problem, in fact, if natural kinds had not also become associated with other and very different philosophical problems. For the remainder of this work, I shall reserve the term 'induction-supporting kinds' for this tradition. The main question of this chapter is whether these kinds are suited to ontological inquiry. They are not.

3.2 The Ontological Tradition

We need to be cautious when interpreting Aristotle on kinds. It is doubtful that all of his diverse claims about substances and essences are compatible. But a few things seem certain. Aristotle thought at least some objects of the world came pre-divided into kinds of things. Groups were not 'the workmanship of man' but fundamental features of reality. It was the job of philosophers to determine where the kinds were and what it was that made certain objects the members of their kinds. It is also clear that Aristotle took particulars to be members of their kinds in virtue of some essential properties, though it is far less certain just what he thought that entailed. Finally, it is clear that Aristotle meant to separate natural kinds from those kinds of things that are made by humans (artefacts).[54]

We can strip away Aristotle's epistemology and philosophy of language to arrive at a simple theory of kinds that is well suited to ontological enquiry. The theory asks after what things *are*, not how we treat them, relate to them, or want them to be. It assumes as a matter of logic that there is something that makes things what-they-are. This feature, whatever it is,

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[52] He now occasionally calls his kinds 'HPC kinds' or 'HPC natural kinds'.

[53] Khalidi (2013) calls these natural kinds, so long as the kinds are 'world-dependent'.

[54] It is a matter of some debate whether he thought both could be substances or have essences (Katayama 1999), but we know at least that he saw some distinction between the two. Dominant opinion seems to be that substances were not artefacts. A separate question is whether Aristotle conceived of there being human-kinds, where objects are grouped according to human convenience rather than according to nature. His methodological suggestions would imply that he thought this ill advised.

is an 'essence'. An essence is a property a kind cannot lose without ceasing to be what-it-is. Essences allow investigation into the naturalness, structure, and uniqueness of kinds of things. If essences are anthropocentric, then the kinds are human kinds. If essences are discrete, the kinds are discrete. If things have multiple essences, then there is pluralism to the structure of kinds. If things have only one essence, then there is only one structure of kinds. Looking at essences tells us much about the kinds themselves.

Many challenges arise for essentialism when we attempt to combine essentialism as an ontological project with essentialism as an epistemic project. When Aristotle turned his attention to scientific classification, he either developed an independent account of taxonomy or radically revised his ontological position. If it is the latter, then he rendered it untenable. The untenable epistemic version of Aristotelian essentialism has been the target of critique in the philosophy of biology for the past half-century. In Chapter 2, I will show how the general ontological project of essentialism is untouched by these critiques. The monism versus pluralism debate that centres round kinds and essentialism requires some conceptual clarification, which I tackle at the beginning of Chapter 3. Combined, these discussions lay the groundwork for a modern theory of essence. The second half of Chapter 3 will introduce this theory, Kind Historicism, and apply it to the problem of intrinsic heterogeneity.

4. Conclusion: The Scope of Pluralism and Deference to Science

We are now in a position to briefly evaluate the scope of the recent trend toward metaphysical and ontological pluralism about natural kinds. I do not mean here to criticise this trend, only situate it relative to the duality of kinds just introduced. Many authors advance such views; some will be encountered in PART II. For present purposes, it will suffice to consider the grandfather[55] of these positions: Promiscuous Realism (PR), mentioned in the introduction (Dupré 1993, Dupré 1996).[56] PR is taken to include a host of claims, but the central claim is this: 'there are many equally legitimate ways of carving the world into kinds' (1993, p. 6). More specifically, 'there are countless legitimate, objectively grounded ways of classifying objects in the world. And these may often cross-classify one another in indefinitely complex ways'. As such, PR is taken to constitute 'a metaphysics of radical ontological pluralism' (p. 18).

Arguments for PR come first from theoretical pluralism as a reaction to intrinsic heterogeneity. Dupré's (1993) main target is classification systems for biological species, of which there are many and more. Biologists classify species in many different ways, using many different properties as epistemic handles. Some use morphology. Some use

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[55] Because it is so often the source of the subsequent positions.

[56] See also (Daly 1996).

reproductive strategy. Some use geography. Some use phylogenetic history. And these are just the main contenders. Most of these classifications have some reality.[57] The properties they employ as nominal essences really exist, are really shared among some non-arbitrary set of species, and are really useful for classifying. Scientists will insist that certain of these classifications represent *the way the species really are*, but Dupré presents good reasons to think otherwise. Critics suppose that there might be hope for a single species classification (Wilson 1996), as yet undiscovered. However it is hard to imagine what this would be, and how it would fill the various roles played by each of the current systems (see Dupré 1996). And this is where the force of pluralist arguments originates. Each of these classification schemes is incredibly useful in a circumscribed domain. Within that domain, predictions, explanations, and generalizations are most powerful if made using one species system but not another. Morphological systems are good for explanations involving body plan development, and physiology. Phylogenetic systems are good for explanations involving historical relations, symmetries, homologies, and analogies. Each classification scheme is a great scheme for some things and not others. Since it is impossible that a single scheme will categorize in the same ways as these incompatible schemes, it is impossible that a single scheme will replace them.

The claims of PR are best assessed within the induction-focussed tradition. Within that tradition it is understandable, indeed expected, that different modes of scientific inquiry will require different kinds. This explains why multiple conceptions of the biological kinds can exist, and also why each can have claim to 'legitimacy', 'objectivity', or 'reality'.

It is not hard to imagine, however, how these sorts of investigations could mislead concerning ontological issues of the sort I am pursuing. The language surrounding PR evidences strong scepticism about the uniqueness of the identities of biological objects. Claims such as PR's might lead the reader to wonder whether there is a single thing that given organism *is*, or whether an organism is as many kinds of things as there are scientific perspectives on it. The arguments presented above should, I hope, dissuade the reader from drawing such conclusions on these bases. I have not yet offered a positive account of biological kinds, but I have shown how attempts to find the best kinds for science should expect pluralistic results, and how these can be amenable to realist interpretation without thereby being identified with natural kinds. The kinds used in successful science have no direct role to play in ontological investigations (given the sense of 'ontology' employed here).

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[57] Dupré (1993) originally claimed that all classifications were real insofar as they latched onto some really shared property. More recently (2001), he has attenuated the claim, believing that certain classification schemes can work well in spite of being non-real in this sense. Some classifications are metaphysically arbitrary while nevertheless helpful.

I anticipate two objections, the full responses to which will emerge over the coming chapters. First, one might object to my separation of natural kinds from science. Surely metaphysics of science should be deferential to scientific practice, says the critic, and not descend into armchair philosophizing. With this sentiment I agree completely. I view this as compatible with my investigation. In cases such as PR, the philosophy is deferential to the scientific *practices* of classification. I believe this suitable only in case we are investigating induction-supporting kinds, in which instance we will want to know how well kinds perform in scientific reasoning. In my own case, I am deferential to scientific *knowledge*, but not necessarily practices of classification. Scientific findings will help determine what it is that makes something what it is, but scientific classifications will do no such thing. I will elaborate this point considerably, by demonstration, in Chapters 4 and 5.

Second, I anticipate a general objection to my separation of natural kinds from similarity. A critic might grant that similarity in properties is not what makes things members of a kind and that critic might even grant the point (made above and defended in chapter 2) that we need not assume a causal relationship between essences and characteristic properties. Even still, there seems room to deny that two members of a kind will differ in characteristic properties. How could two things be of a kind, the intuition goes, if they are dissimilar? As a matter of methodology, the similarity (or not) of natural kinds should be an open question. Just as we view the singularity or multiplicity of natural kinds (monism/pluralism) as an ontological fact to be investigated, so too should we view the various features of natural kinds, similarity first and foremost. Notice also that a partial response to this objection can be found in the norms governing lay-theory application of kinds. We tend to think that all sorts of things form kinds that are dissimilar in certain important properties. Biological kinds are wonderful examples of this sort. As any pet owner can tell you, kind membership helps you understand animals to a degree, but each is highly dissimilar in psychology, dietary preferences, and even morphology. The point is that the same kinds can interact with the world in different ways, resulting in potential differences in any and all outward properties. In Chapter 3 I will explain in more detail how this holds for biological kinds, and in Chapter 4 I will explain how two chemical molecules of the same kind can have different shapes and thus different physical properties. In each instance the explanations of within-kind differences lie in the interactions between particulars, their (historical) essences, and the world.

2

Three Things Essentialism is Not

Popular opinion has it that essentialism is dead in the water: It was harpooned by Darwin from the deck of *The Beagle*. This is simply not true. While essentialist doctrine must be carefully applied, and while many specific applications of it are untenable, the Darwinian criticism leaves the general project of ontological essentialism untouched. A few brave philosophers of biology defy the received view and offer theories of natural kind essentialism (Devitt 2010, Griffiths 1999, Millikan 1999a, Wilson 1999a, Wilson et al. 2007). That such theories are proffered at all suggests that we should re-think the 'anti-essentialist consensus' (Okasha 2002). As Wilson (1999a) claims,

> [T]he concept of an essence need not be viewed as the concept of substance came to be viewed within modern science, as unnecessary metaphysical baggage to be jettisoned. Rather, … essentialism represents an important way in which Aristotle's views of the unity to the biological world … have proven to be correct. (p.205)

I do not endorse all of these contemporary essentialisms; in fact I criticise several in this chapter. I do however share with these authors the recognition that essentialism about kinds need not be set adrift before philosophizing about the biological world. I go further than these 'new biological essentialists' (Ereshefsky 2010a) by not only offering a theory of biological essentialism (to be elaborated in Chapter 3) but by first addressing directly the widespread criticisms that render essentialism taboo. That is the purpose of this short chapter.

My positive general account of essentialism was given in Chapter 1: particulars form kinds in virtue of shared essences, where an essence is a property or property set, in the widest possible sense of 'property'. Others share the general positive conception of essentialism I employ, at a minimum; but essentialism in the philosophy of biology has far more critics than defenders. At least, a lot of philosophers critique views that they dub 'essentialism'. Central to these critiques are three beliefs about what essentialism requires or entails. First is the claim that essences are sets of necessary and jointly sufficient intrinsic physical properties. This version of essentialism, we are told, was slain by Darwin (or at least by Darwinism). Second is the claim that essences should causally explain the characteristic properties of the kind. This quality of essences is used by proponents to justify kinds' presumed role in scientific practice, but it is also seized upon by critics who note that causal relationships in biology are imperfect. Third is the claim that natural kinds are eternal and unchanging, like Platonic forms. This fact does not sit well with observers of biology, who notice that Parmenidian cosmology is incompatible with Darwinian change within species and with speciation.

There are anti-essentialist critiques of each of these three claims, which invoke distinctively biological premises. I have no qualms with these. My target is not these arguments against essentialism, but rather the belief that any of the three claims standardly at issue is required for a mature natural kinds essentialism applicable to the biological world. In this chapter, my goal is to sidestep the standard criticisms of essentialism by distancing essentialism as a general position from the specific essentialism(s) critiqued. The first claim about essentialism was initially applied only to essentialism as an account of scientific epistemology, and pertains to the specific view that essences are physical property sets. The second claim appears to arise from unwarranted combination of the two traditions of kinds described in the previous chapter. The third claim was certainly believed by Aristotle but is in no way a necessary component of natural kind essentialism. Distancing essentialism from these claims is paramount for the future of essentialism in biology, and understanding the scope of the anti-essentialist consensus is important for avoiding the pitfalls of essentialisms past. Before addressing these three claims, I will briefly introduce the 'anti-essentialist consensus' that I am attempting to avoid. By way of conclusion, I will explain how the stripped-down conception of essentialism that avoids the anti-essentialist consensus is still worth using, for it permits a discussion of the similarities and dissimilarities of classes across the sciences by making clear the similarities and differences between kinds in the biological and physico-chemical worlds.

1. The Anti-Essentialist Consensus

Traditional essentialism has been the subject of much debate in the philosophy of biology, nearly all of it in connection with the species debates and nearly all of it negative. The received view holds that traditional versions of essentialism will not provide an adequate account of biological species. Samir Okasha (2002) refers to this as the 'anti-essentialist consensus',

> [The] attack on essentialism has met with almost universal acceptance among both biologists and philosophers of biology. (p.190)

Elliot Sober (1994) claims,

> Essentialism about species is today a dead issue. (p.163)

Wilson, Barker, and Brigandt (2007) assert,

> Philosophers' and biologists' rejection of kind essentialism has become part of a canonical view of the history of essentialism in the biological sciences. (p.4)

Citing a number of authors, Michael Devitt (2008) sums up the consensus nicely:

> Alex Rosenberg says: "The proponents of contemporary species definitions are all agreed that species have no essence" … Sober expresses this consensus as follows: "biologists do not think that species are defined in terms of phenotypic or genetic similarities"; tigers are "not *defined* by a set of traits" (1993, 148). Sterelny and Griffiths put the point bluntly: "no intrinsic genotypic or phenotypic property is essential to being a member of a species" (1999, 186). (p.350)

Philosophers of biology agree that they all agree that essentialism cannot provide an adequate account of biological objects.

These critiques are echoes of David Hull's (1965a, 1965b) famous argument against the use of Aristotelian definition in biological taxonomy.[58] Hull's essay exposed the incongruence of Aristotelian scientific method and contemporary knowledge and investigation of the biological world. A definitional approach to taxonomy requires drafting lists of essential and accidental observable properties, but modern biological science tells us that all physical properties of species are liable to variation across and within biological groups. This is part of intrinsic heterogeneity. All properties appear accidental, preventing a definitional approach to taxonomy. Carefully articulated though it was, the pragmatic bent of Hull's claim has been lost over time. Adaptations of Hull's point flirt with conclusions about metaphysics and ontology, in addition to those about scientific method.

A related criticism, which was slightly older, was revitalized and strengthened in light of Hull's work. This is the argument by philosopher Karl Popper (Hull's mentor) and biologist Ernst Mayr that Greek thought had prized fixity over change, and that this corresponded to an *a priori* rationalist approach to science. The belief being criticized was that the world consisted in unchanging types. According to Popper, this led to dangerous armchair science. If things are unchanging and eternal, then we can presumably come to know them by simply

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[58] Hull penned a now-famous paper in which he claimed that taxic essentialism had led to 'two thousand years of stasis'. His seminar leader, Karl Popper, was so impressed with the epic as to seek its publication in the *British Journal for Philosophy of Science* without Hull's knowledge! The narrative was soon picked up by others, most importantly by Ernst Mayr, as it made its way into philosophical folklore. The view served as a convenient device for Mayr, who critiqued taxic essentialism as a proxy for views of speciation that disagreed with his own population approach.

reflecting on their essential natures rather than by studying them empirically. This is at odds with a view of the biological world that emphasizes change and lawlessness. The motivations of these thinkers were a mix of philosophical and political[59] but the legacy of the argument is the anti-essentialist consensus.[60]

The consensus can be helpfully distilled into three core criticisms, targeting three versions of essentialism. The first targets the claim that essences are intrinsic property sets. The second targets the claim that essences should cause/explain the characteristic properties of the kind. The third claims that essentialism commits us to eternal or unchanging kinds. I will deal with each of these in turn, generally conceding the criticism while also demonstrating why essentialism need not be committed to intrinsic property sets, causal relations with characteristic properties, or Parmenidean ontology.

2. Three Perspectives on Essentialism

2.1 Essence as Slain by Darwin (Intrinsic Property Sets)

There is an old story in the philosophy of biology about Aristotelian essentialism and its eventual but long-overdue defeat at the hands of Charles Darwin. I recount this story below. It serves often to obscure rather than illuminate the place of Darwinian theory in the history of ideas, and it is also often used to construct a straw person of essentialist approaches to kinds. In spite of its exposure as confused and historically inaccurate, the story continues to appear in print. Even more frustratingly, this story is still deployed in philosophy of biology circles whenever an argument employs the term 'essence' in anything but a derisive sense. The story is useful, however, because careful attention to the anti-essentialist argument helps reveal the limitations of essentialism as a scientific programme. Darwinian theory precludes essentialism about scientific language and method, but leaves essentialism as a general ontological tool untouched.

The anti-essentialist story goes like this: Before Darwin, biology was saddled with an Aristotelian essentialist methodology, which held that all (natural) kinds, of which species were an exemplar, ought to be defined by shared sets of jointly necessary and sufficient properties. Wilkins (2013) calls this 'taxic essentialism'. Taxic essentialism was a problem, the story continues, because it ignores the manifest diversity of individuals within a species that results from and enables evolution by means of natural selection. Surely Darwin's theory put an end to taxic essentialism by showing that species were not groups of members sharing necessary and sufficient properties, but rather populations of individuals who exhibit

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[59] Popper associated methodological essentialism with fascism (and all that was wrong with science in society).

[60] A detailed account of Popper and Mayr, and their differences, can be found in Wilkins (2013)

considerable variation. For some time it was possible to hold out a Lockean naturalist hope that one-day science would reveal the shared microphysical properties that species had in common. For a time it was not unreasonable to believe that the material of heredity would fill such a role. But this hope was dashed. In contemporary forms, critiques of taxic essentialism typically culminate with the observation that even DNA change within a species—no property is safe from selection![61]

The evidence against this tale as a historiographic claim is huge (Amundson 2005, Farber 1976, Hodge & Radick 2009, McOuat 2009, Wilkins 2013, Winsor 2003, 2006) and I will not recount it here. John Wilkins (2013) summarizes the body of historical work succinctly: 'There is little evidence that *anyone* was … [a] taxic essentialist' (p.3). Darwinian theory did not change an incumbent theory of classification and kinds; it contributed to an existing discourse of species as heterogeneous and changing entities.

History aside, it is worth understanding how taxic essentialism works and to what degree this represents essentialist doctrine. The birth of the contemporary erroneous pre-Darwinian history is generally credited to then-graduate student David Hull (1965a, 1965b), who attacked the claim that species have sets of 'essential' properties.[62] His frustration is palpable:

> Presented with the welter of diverse forms to be classified, a taxonomist can greatly simplify his task if he pretends that certain properties are 'essential' for definition. But he would have to do just that—pretend—since the names of taxa cannot be defined in terms of essential characters without falsification on a scale which should have been evident even to the most uncritical investigator with only a limited knowledge of the organisms being classified. (1965a, p.316)

Two features are of note. First, Hull's claim is about scientific methodology and thus targets essentialism as a methodological thesis, not an ontological or metaphysical one. Second, Hull targets even more specifically the version of essentialism that identifies essences with intrinsic physical properties.

Understanding the appropriate scope of this Darwinian anti-essentialist argument requires identifying three versions of essentialism:

> **The Ontological Claim** that (at least some) natural types are governed by a principle that unifies the many components of that thing. This unifying principle makes the thing what-it-is.
> **The Semantic Claim** that the definition of a thing is an account of its essential properties.
> **The Methodological Claim** that the aim of science is knowledge of definitions (essences).

At issue are the semantic and methodological claims, which can be lumped together for present purposes. Hull's point is that, as scientific methodology, defining species and

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[61] Not only that but it differs from cell to cell within an organism!

[62] Hodge and Radick (2009) remind us of Dewey's similar proclamation, nearly 50 years earlier, that Darwin's theory had ended the 2000 year reign of fixidity and perfection over change and origin.

searching for definitions cannot proceed by looking for intrinsic physical essences, since the physical properties of biological objects are by their very nature heterogeneous.

Recall that for Aristotle a definition is a statement of essential properties. In this context, essential properties are contrasted with accidental ones. Essential properties should be part of a scientific definition, accidental ones should not. Are 'essential properties' in the context of the semantic and methodological claims the same as the 'essence' in the sense of the ontological claim? Interpreters have wondered whether Aristotle has the same account of essence in mind for these epistemic issues as he did when making more metaphysically-loaded claims. It is unclear. It is certainly the case that Aristotle will not have separated ontological and epistemic issues, the way a post-Kantian philosopher should. But Hull was savy. He asks us to forget about the term 'essence' and focus instead on the methodological point about science:

> Disregarding all the talk about essences, what Aristotle was advocating in modern terms is definition by properties connected conjunctively which are severally necessary and jointly sufficient … Such a mode of definition is eminently suited for defining eternal Forms. It is not very well suited for defining the names of evolving species or for 'species' itself, and yet it is exactly this mode of definition which has been assumed to be the only mode of definition permissible until recently. (1965a, p.318)

Hull is targeting not essentialism (understood as the Ontological claim) but the Semantic and Methodological essentialisms, specifically Aristotle's versions of these claims. Hull hits his mark. When it comes to classification in biology, strict essentialism will not do.

That Aristotelian definition is ill suited for biological classification should be clear. But modern applications of Hull take the argument to extend to essentialism as an ontological thesis. This is possible, but, even still, only essentialisms committed to intrinsic physical property sets are at risk. It is worth looking briefly at this assumed link between ontology and methodology

Suppose we claim, as a neo-Aristotelian might, that a natural scientific classification should result in categories reflecting sets of necessary and jointly sufficient physical properties. We might be interpreted as simultaneously making an assumption about the objects being defined: We are assuming that those objects are members of their kinds in virtue of those properties. In other words, we might assume that methodology should track ontology. One way of to connect Aristotle's epistemic account of scientific method (definition) with his ontological account of essence, is to assume that Aristotelian methodology is a consequence of Aristotelian ontology. The assumption at work here is problematic. Recall that, in this thesis, the relationship between natural kinds and scientific practice is an open question (***Q3***). The link cannot be assumed. In order to establish a link from what-there-is to what scientists should treat there as being, we need to know a lot about the suitability of natural kinds for scientific inquiry. I will return to this in the next chapter.

For now, even if there *was* a good link from ontology to epistemology, then Hull's critique of essentialist method has implications only for ontological essentialisms committed to intrinsic property sets. The failure of Aristotelian methodology might be thought to reflect the deeper fact that biological categories do not admit neat sets of necessary and jointly sufficient physical properties. This is intrinsic heterogeneity. This mode of argument is fine only insofar as the ontological essentialism targeted shares with its methodological counterpart a theory of what essences *are*.

Hull has assumed essences to be sets of intrinsic physical properties. We can safely conclude that neither methodological nor ontological essentialism about these types of essences is viable. But this does not tell us anything about the more general essentialist claim that objects are members of their kinds in virtue of some essence. While Hull's criticism is valuable in eliminating one specific view of essences, it should not be taken to support a more general anti-essentialist consensus. There are views of essence on offer that do not require necessary and sufficient property sets, and so do not run afoul of intrinsic heterogeneity. Some of the new biological essentialists have sought essences outside of the list of intrinsic physical properties. Various forms of relations, histories, and phylogenies can fill the role of essence without being liable to Darwinian variation.

However, there is still an option for the anti-essentialist. Even if property sets are not themselves essences, they are still causally guaranteed by the presence of the underlying essence—or so one interpretation of essentialism claims. If this were true, of course, then a shared essence would entail shared property sets. Once again, Darwinian theory would remind us that this is untenable. It is to this presumed link between essences and properties that I now turn.

2.2 Essence as Explanation

A common assumption within contemporary discussions of essentialism is that essences must be explanatory of the properties of members of the kind. Call this the 'Essence-Properties Principle' (EPP). Notice that EPP just is the claim that essences are causally responsible for the characteristic properties of a kind, since the sort of explanation required here is a causal one.[63] The position is popular.[64] Even philosophers who flaunt the anti-

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[63] Locke held something like the EPP. He followed an Aristotelian account of essence as that which makes something 'what it is', but believed also that the properties of a thing depending on its essence: 'thus the real internal, but generally … unknown Constitution of Things, whereon their discoverable Qualities depend, may be called their Essence' (III.iii.15). Later on, however, Locke intimates that the

essentialist consensus nevertheless subscribe to versions of EPP (e.g. Devitt 2008, Griffiths 1999, Okasha 2002). Proponents and opponents of essentialism reject theories of natural kinds on the grounds that they do not meet EPP (e.g. Ereshefsky 2010a, Slater 2009, Lewens 2012). They are right, of course, that these essences often fail to be explanatory, however the principle on which these criticisms depend, EPP, is unmotivated by ontological essentialism.

Nothing about essentialism requires EPP. If EPP *is* included as part of essentialism, it quickly collapses into the claim (above) that essences are intrinsic property sets. If EPP is motivated by any version of essentialism at all then it is from epistemic versions of the claim, likely having crept in from the induction-focussed tradition of natural kinds. A recent essentialist theory of biological species offered by Michael Devitt (2010) demonstrates precisely this type of account, but in the end fails to serve as either a scientific tool or an ontological theory.

Causal links between essences and properties are doubly problematic in biological contexts. First, such a link entails the existence of shared property sets. If every member shares an essence and if those essences cause some set of properties, then all members share a set of properties. As just discussed, few things in biology perfectly share sets of physical properties. Second, causation in biological contexts is always highly contingent. Though we frequently specify relations of biological cause and effect, we tacitly know that these are subject to the presence of certain background conditions. A certain combination of hereditary materials might be thought essential to tigers, given that it makes an organism look like a tiger, but this causal relationship holds only in the presence of life-supporting environmental conditions, adequate nutrition, and the absence of certain other genetic mutations. So either the essence does not always produce the property set, or we roll the supporting conditions into the essence such that it does. Neither option is attractive to the essentialist.

But is an essentialist necessarily committed to EPP? Essentialism is the claim: 'particulars form kinds in virtue of shared essences'. Nothing about this claim commits the essentialist to the further view that those essences are causally responsible for properties characteristic of the kind. The causal claim follows only if we add the extra belief that part of being a kind (part of the 'what-it-is') is having some set of shared properties. However this addition robs the essence of its purpose, making the properties essential, too. The following example illustrates this point.

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connection between essence and properties is simply *assumed* necessary or causal, given the frequent concurrence of those properties (II.xxiii.3).

[64] According to Hacking's history of natural kinds (1991), the EPP (my term not his) is part of the natural kinds of both Peirce and Leibnitz. A watered-down version is endorsed by Russell.

Many theories of kinds claim that a molecule of gold is what-it-is in virtue of the atomic number 79. Theories with EPP add that this atomic number should be causally responsible for the properties of gold, such as its ductility, malleability, and colour. But notice that this rests on the further assumption that part of being gold is having the right set of properties. If this assumption were not in place, it would not be necessary for the atomic structure to cause the physical properties. This assumption robs the essence of its primary function, such that it is now no longer the case that the essence makes something what-it-is. Atoms need atomic essences *plus* the set of characteristic physical properties caused by them. The properties very quickly become parts of the essence, at which point we are back at the first anti-essentialist challenge.

A better way to view this case is to note that, in this instance, the essential property, which happens to be a physical property, bears some causal relations to other physical properties. There is no need to assume the causal relation necessary. It may be viewed as an interesting rather than constitutive feature of the kind-membership relation. It is interesting that physico-chemical kinds have this feature while biological kinds do not.

With EPP so easily revealed as unmotivated, the question remains as to its origin. If I may speculate, EPP likely crept in from theories of kinds aimed at induction, since the addition of EPP strengthens the epistemic justificatory status of knowledge of kind membership. This move, and its flaws, is exemplified in recent work by Michael Devitt (2008, 2010).

Devitt claims that philosophers and scientists assume kind membership to be 'explanatory' rather than merely 'informative'. For Devitt, explanation is a causal account, while informativeness is a weaker epistemic justification. As we saw in the previous chapter, membership in an induction-supporting kind should be indicative of the presence of certain shared properties. Members of the kind *tiger* are likely to be stripped, ferocious, etc. We know this because these properties tend to cluster together. If induction-supporting kind membership is indicative in this way, then knowledge of kind membership can be seen as a justification of inductions about the kind. We are justified in assuming that future tigers will have stripes because most members of *tiger* observed until now will have stripes (or because stipes tend to co-occur with other properties of tigers, for HPC). This is the 'informative' role of kind membership. But Devitt is not happy with kind membership merely justifying inductions; he wants it to *explain* them. Kind membership can only be explanatory, according to Devitt, if members of kinds have essences that causally guarantee the presence of the traits over which we want inductions to range.

The latest molecular biology tells us that tiger stripes owe to the possession of an activator-inhibitor pair of proteins, which act in a specific pattern of alternation during the pigmentation of fur. Devitt is therefore committed to the claim that these are parts of the essence. It is partly in virtue of having stripes that tigers are tigers, and so it is partly in virtue of having the genetic machinery for making stripes that tigers are tigers. If it were not, then we could not know that membership in *tiger* explains the presence of stripes. It is implicit in Devitt's argument that if it were *not* partly in virtue of having stripes that tigers were tigers, then we would not make inductions about stripes.

What Devitt has done is to unpack the syllogism behind induction. We do not predict that future tigers will have stripes *because* they will be tigers; we so predict because these future tigers will have the machinery for making stripes. Notice however that Devitt has brought us to the same point as the misguided intuitive argument above. He has simply defined as 'essential' all of those properties we take as typical of a kind. He has also defined as essential all of the machinery required for making those properties. He has done this only so that inductions ranging over typical properties will count as 'explanations' rather than 'indications'. This burden is too much and the reward is unneeded. If Devitt is truly committed to the full explanation of all traits of kind members, then his essences will swell to enormous size.

Devitt has not supplied an explanation as to why tigers will have stripe-making machinery. The answer: 'because they're tigers!', will not suffice, by his own logic. The protein explanation is proximate, not ultimate. As a result, he will have to admit that the evolutionary history behind activator-inhibitor proteins is also part of the tiger essence, along with any other historical and physical machinery necessary for the presence of activator-inhibitor proteins. And this is only for one trait. Devitt is tumbling down a slope. Absent a way to limit the sense or scope of requisite explanation, the essence very quickly becomes the entire array of physical and non-physical facts required to explain typical features of the kind. The essence is nearly everything.

Inductions ranging over well-formed kinds work, and Devitt seeks an account of why this is so. His account must ground the success of explanations in real features of the world that make the inductions true. This requires explaining the reliable recurrence of the traits/properties involved in inductions. Since this reliability consists in a large range of causal relationships, essences become now long, unwieldy and unconstrained conjunctions. Devitt's essentialism effectively becomes the claim that things are what they are in virtue of themselves and the world. This would make essentialism virtually meaningless. It is for this reason that Devitt's view has been dismissed as 'too bland to be of interest' (Lewens 2012).

Devitt's goal of grounding inductions can be met with far less metaphysical baggage, since his theory is in many ways a metaphysically-loaded version of HPC. HPC is explicitly epistemic. On that view, a cluster of properties does not make something what-it-is; their role is epistemic, not metaphysical. Property clusters are independently existing things in the world that humans latch onto in order to form categories for induction. The causal structures that maintain these property clusters are objective, but they need not be 'essences' in order to do so. Property clusters and homeostatic mechanisms do not bestow identity or kind membership. We do that (for HPCs). It is hard to see what the extra baggage of 'essence' adds to this discussion.

Ereshefsky (2010a) offers a similar criticism, noticing that Devitt's essentialism clashes with biological practice. He agrees with Devitt that biologists cite many intrinsic properties and causal relationships in order to explain the success of various projective inferences, but wonders why it is necessary to call these things 'essential'. Practicing biologists do no such thing. Devitt's view takes the induction-supporting kind tradition, which stands very well on its own, and supplements it unnecessarily with a metaphysical account of explanatory essences.

2.3 Eternal and Unchanging Kinds

The final claim about the Darwinian overthrow of essentialism is present to some degree in Hull (1965a, 1965b) and Mayr's (1976) 20th century accounts, but was in fact developed much earlier by John Dewey (1910). In 1909 Dewey delivered a lecture on the influence of Darwinism on philosophy, published one year later. He began by claiming that 'the combination of the very words 'origin' and 'species' embodied an intellectual revolt and introduced a new intellectual temper' (p.1). Dewey saw Darwinian theory as overthrowing the out-dated view of a world constituted by eternal and unchanging forms. He credited this idea, rather broadly, to 'The Greeks', and praised its demise:

> The conceptions that had reigned in the philosophy of nature and knowledge for two thousand years, the conceptions that had become the familiar furniture of the mind, rested on the assumption of the superiority of the fixed and final; they rested upon treating change and origin as signs of defect and unreality. In laying hands upon the sacred ark of absolute permanency, in treating the forms that had been regarded as types of fixity and perfection as originating and passing away, the 'Origin of Species' introduced a mode of thinking that in the end was bound to transform he logic of knowledge, and hence the treatment of morals, politics, and religion. (1910, p.1)

Dewey challenged the standard narrative about the acceptance of Darwinism, which focused on the clash between evolution and religion. Instead, the clash was between fixity and flux—Parmenides versus Heraclitus all over again.

A similar version of this criticism has strong connections to the first anti-essentialist claim (2.1). Consider the following, from Ruse (1987),

> Evolution says you can take virtually any property you like, and if you go back (or forwards) enough in time then ancestors (descendants) did not (will not) have it … this is just what Aristotle [essentialism] cannot handle. (229)

Like Dewey, Ruse identifies a clash between the Greek worldview of fixity and a Darwinian biological worldview of flux.

Recent scholarship has demonstrated that Dewey's historiography is questionable (Hodge & Radick 2009). Few in the time of Darwin held the Greek ontology that his theory challenged. Even among 'The Greeks' a diversity of views were held, some of which were compatible with Darwinian theory.

Popular or not, however, Parmenidian ontology was indeed challenged by *The Origin*. Yet such an ontology of fixity is in no way a consequence of essentialism. Aristotle's essences were unchanging. The essence of a horse is the same now as it was in Aristotle's time and the same as it will be 2000 years hence. Yet this is not a property of the essence itself but a postulate based on Aristotle's general cosmology. Aristotle believed that the universe and everything in it had always existed. Continual existence is a form of perfection and the universe, on this view, is perfect. Fixity is a feature of the divine perfect superlunary sphere. Sublunary material beings (like horses) are imperfect insofar as they are subject to generation and decay, but approximate the perfection of the superlunary by reproducing perfectly, generation after next. This allows animate objects on earth to obtain some perfection (by being members of unchanging kinds) while admitting of some obvious imperfections (namely dying). Organic bodies die but the essential souls remain the same from parent to offspring. A similar focus on fixity can be found in Plato, who held that objects are what they are in relation to the Forms, which were unchanging and eternal.[65]

These Greek views are untenable in light of Darwinian theory. Both lines of thought may have been compatible with the variation in traits required for evolution (perhaps an Aristotelian could admit change in 'accidental' properties of a kind), but Parmenidian ontology is incompatible with evolution itself, which tells us kinds change, that new kinds arise, and that old kinds die off. Interesting though this is, it is in no way a problem for essentialism more generally. That some early essentialists happened to also hold these ideas about eternal perfection should not count against essentialism. This line of argument is however a useful reminder that nature's joints appear to change and also to come into and out of existence. A theory of kinds must make sense of this.

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[65] Dewey lumped Aristotle and Plato together. Both emphasized perfection but in entirely different ways. Regardless, Dewey was correct to the extent that both Greeks presumed the things in the world to be unchanging. It is this belief that was overthrown by Darwin—though few people held it by that time, anyhow. For details, see Hodge & Radick (2009).

3. What is Essentialism for?

There is one potentially 'spooky' power of essences left: their ability to make things what-they-are. Recall that essences can be presented as the logical consequence of assuming that at least some things have their identities independently of human cognition. Within this framework it would seem that some feature of those things or the world must make the things what-they-are, since we are assuming that it is the world, not us, that makes them what-they-are. The question is how to understand this ability to 'make' something what-it-is.

One available interpretation treats this as a causal claim: essences 'make' things what-they-are in a causal sense. A related interpretation is modal in nature: The essence of a thing makes it necessary that the thing is a member of the kind of thing that it is. Perhaps these causal or modal powers of essences lie behind some of the off-putting metaphysical baggage that offends certain philosophers of biology.[66] If so, it is needless; for I do not think that problematic versions of the causal or modal interpretation need to be adopted.[67]

Part of the problem is the reification of kinds required for these claims. They treat the natural kind as an object or a property, something into which a particular can be transformed, or something that can be added to the particular. Some undesirable consequences follow from reification. Most notably, we must then ask after the ontological status of the kind itself. Is it an object, property, individual, Platonic Form, independently existing universal, or something else? These debates have been sampled and I have no interest in re-starting them.[68]

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[66] I will treat the causal and modal claims as the same, since what I am interested in is the idea that the essence performs some action on the object.

[67] A prominent school of thought interprets 'essence' as a modal operator, believing that essences render things members of their kinds in all possible worlds in which that thing possesses that essence. This is often associated with Kripke (1972, 1980) and Wiggins (1980), among others. Applied to discussions of natural kinds and classification, this modal essentialism is either (i) too crazy to believe, or (ii) trivial. (i) Some things do seem to change their kinds. A molecule with 8 protons is a member of the kind 'Oxygen'. In all possible worlds this holds. But, as any science nerd will tell you, we can take this Oxygen molecule and spin it around under Switzerland and France until it loses a proton and becomes Nitrogen. It looks an awful lot like this molecule has changed kinds and thus modal essentialism is false. On the other hand, (ii) one might try to claim that the Oxygen molecule ceased to exist and that a new molecule of the kind Nitrogen arose like a phoenix from the ashes. If this is the case then the position seems a trivial addition to essentialism as we already have it. This addition requires only that we re-think our theories of object persistence, tying them to kind membership. Weird as this thesis would be, it would not affect anyone's claims about what is or is not a natural kind, or whether the world contains natural kinds.

[68] There are many philosophers who indeed view natural kinds as universals. Hawley & Bird (2011) argue that instantiation of shared properties unifies particulars and that this brings into existence a universal, which is the natural kind. Lowe (2006) treats natural kinds as 'substantial' universals, which are a fundamental ontological category, separate from particular objects or their properties. Particulars instantiate kinds and exemplify properties. Views such as these assume and characterize rather than establish the existence of universals. I view these approaches as too ontologically-loaded, for this reason. It is not clear at any rate that these discussions of kinds are worried about the sorts of classificatory issues that concern philosophers of biology.

In fact it was this type of failed and seemingly fruitless enquiry that frustrated Ghiselin and Hull into forwarding the species as individuals hypothesis.

It may seem that reification is the only route to realism about natural kinds, but that is not so. Consider the alternative. Kinds are groups; they are special sorts of groups but they are groups nonetheless. Groups are things that minds recognize.[69] In this sense they exist only in cognition. Does that mean they are all non-natural, mind-dependent? No. We can admit that the group does not exist as a singular thing or attribute in nature without thereby falling into nihilism or conventionalism. Naturalism can come from facts about the grouping. If the objects really go together, naturally, then we might have natural kinds. If they go together only from our perspective or according to some interests then they are probably not natural kinds. Essentialist theories of natural kinds are simply theories about what it means to 'really go together, naturally'. Adding some additional invisible metaphysical netting surrounding the group does nothing but engender confusion and invite criticism.

Even non-natural kinds admit of essences. All things are made what-they-are in virtue of something. The more interesting question is the nature of that something. Kinds like clothing, currency, and other artefacts are what-they-are in virtue of humans' attitudes, designs, reference, and uses. Since this fact about the world is quite obviously not mind independent, we would not want to call these things natural kinds. But we would not allow that to stop us from saying that something (or sets of things) makes them what they are.[70] The interesting feature about *natural* kinds is that their essences are such that they are human-independent, even if we need human perception to recognize them and human language to talk about them.

In sum, essentialism is at base a rather simple notion. It does not entail that things have lists of necessary and sufficient properties; it does not require that essences explain the characteristic properties of a kind; and it does not require that we adopt Parmenidian cosmology. Essentialism is simply the claim that, if there are kinds in the world, objects are members of their kinds in virtue of some shared essence. Any further claims about what essences are or how they operate are the creation of specific natural kind theorists and their critics.

I hope to have dissuaded the reader from expecting that essences do many of the weird and wonderful things often claimed of them, for these expectations are unmotivated by essentialism itself and it is these expectations that render the position problematic. The role of

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[69] It seems that the argument can also be run where kinds are viewed as types. One needn't worry whether a type is an independently existing universal. One need only worry whether the type is out there in nature to be discovered or whether it is being imposed on nature by human minds with specific interests.

[70] Perhaps some would not want to call that an 'essence' but that is a semantic dispute.

essences in my discussions of natural kinds will not be explanatory or causal or anything like that. I look at essences because looking at essences *is* looking at kinds. With theories of kinds back in view, thinking about the nature of essences provides some insight into the types of kinds at work, and into the features of those kinds. In the next chapter, I will begin to describe a distinction between biological kinds and physico-chemical kinds. The difference manifests as a difference in types of essence. Where physico-chemical kinds have at their core a physical structure, biological kinds have at their core a Darwinian history. The biological world is different from the chemical world because the essences of biological kinds are of a fundamentally different sort than those of physico-chemical kinds. This raises a host of interesting questions about the unity of the world as a whole, and the internal unity of biological things and physico-chemical things.

These questions involve *monism* and *pluralism*. Philosophers of biology and philosophers of physics and chemistry are divided on these issues. The former largely preferring pluralism, the latter largely preferring monism. Before these issues can be assessed, some clarity must be brought to the meanings of the central terms in the debate. It is to *monism* and *pluralism* that I now turn.

3

Carving Monism at the Joints & Introducing Kind Historicism

In this chapter I will introduce Kind Historicism, but in order to do so I must first introduce a distinction concerning types of monism and pluralism. Kind Historicism is a pluralist account insofar as it does not exclude other categories of natural kinds. However it is not (necessarily) pluralist in the sense of supporting multiple non-translatable taxonomies of natural kinds, which is how 'pluralism' about natural kinds is generally used. The distinction should be helpful in its own right, and will help situate Kind Historicism with respect to other perspectives on natural kinds.

'Monism' can mean different things to different people, but for the purposes of this discussion it is chiefly a claim about metaphysics and ontology.[71] Some philosophers of science have taken to using the terms 'monism' and 'pluralism' to describe states of epistemic scientific practice, as in Beatty's (1994) 'theoretical pluralism', defined in the introduction. In this chapter, however, these terms will always refer to metaphysical or ontological theses, unless otherwise stated. The metaphysical reading of monism sets to one-side questions about classification as a scientific tool and focuses on questions about the metaphysics of natural kinds. Ruphy's (2010) characterization is typical:

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[71] Monism as a claim about kinds should not be conflated with monism as a claim about the relations between the sciences, which has more to do with (explanatory) reduction. I call the latter claim the 'unity of science' thesis, to avoid confusion.

> Metaphysical monism states that there exists some natural order, that is, some objective, mind-independent divisions that cut nature at its real joints in a unique way. In other words, the world comes pre-packaged with a unique set of … natural kinds. (p. 1116)

Monism takes a stance on the general constitution of the kinds in the world, believing them to be somehow unified or homogenous.

Philosophers of science will be aware of the staggering number of cases taken to challenge or support monism. Recent discussions have focussed on celestial bodies (Ruphy 2010), molecules (Hendry 2006), and proteins (Slater 2009). This is not to forget the perennial debate: biological species.[72] Calls for pluralism take a number of forms. The weakest of these arguments appeal only to theoretical pluralism. Philosophers document a plurality of successful classification systems in play in a given field of enquiry and argue that multiple of those systems are natural. A stronger argument builds on these characterizations by suggesting that facts about the objects in question preclude monistic classification and so require metaphysical pluralism. There are a number of ways to move from theoretical pluralism to metaphysical pluralism. A different line of argument appeals to intrinsic heterogeneity, noting that biological things are importantly different from those of chemistry or physics, necessitating a uniquely biological natural kind. Both lines of thought warrant careful consideration; the success of pluralistic taxonomies and the failure of traditional accounts of kinds to characterize biology constitute challenges to monism. However it may be clear already that the 'monism' being challenged in each of these two cases is not the same. There are multiple monisms at issue.

Before the pluralist challenge can be assessed and before its implications can be understood, we need to sharpen our conceptual tools and fully characterize monism and pluralism. This chapter will set out two monisms about classification. Ruphy's 'metaphysical monism', above, evidences the received interpretation, which focuses on the uniqueness of the arrangement of kinds. This is 'Taxonomic Monism'. A second interpretation is also available. It focuses not on the number of arrangements of kinds, but on variation in the kind category itself. I call this 'Category Monism'. Category Monism is necessary in order to represent cases where variance in the kind category is hidden within a single taxonomy. This is because, tempting though it is to assume that differences in the kind category will show up as distinct taxonomies, such entailment relations between the two monisms do not in fact exist. The first task of this chapter is to motivate and introduce this distinction, and then show that the two monisms are independent.

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[72] See citations in introduction.

The second task of this chapter is to introduce Kind Historicism. According to Kind Historicism, biological kinds are historical kinds. That is to say that biological things are what-they-are in virtue of their histories, and so the unifying feature of biological kinds, the essence, is a shared history. Historical kinds should describe all biological objects—insofar as all have a history. Biological kinds are categorically distinct from physico-chemical kinds. Fundamental heterogeneity thus exists between the physical and biological sciences, in the form of categorical differences, not necessarily within the biological sciences themselves, in the form of taxonomic plurality or indeterminacy. I will conclude by sketching the relationship between historical kinds, intrinsic heterogeneity, and theoretical pluralism. This will be followed by a brief aside on the relationship between scientific realism and monism/pluralism in light of my taxonomy/category distinction.

1. Monism(s)

Monism and pluralism are ancient and well-travelled positions, applied now to everything from science to art to ethics. What binds all monisms together is that they attribute *oneness*, contrasted with pluralists' *many-ness*. Because 'monism' refers to such a great range of positions, it is necessary to clearly articulate any given monism by specifying first a target of the predicate, the thing being counted, and second the unit for counting. One might thus be monist about objects, counting types. This is 'substance monism', the claim that there is only one type of object in the world. Compare this with pluralism about objects, counting tokens. This is 'existence pluralism', the claim that there exists more than one token object. Notice that this *pluralist* position is entirely compatible with the above *monist* position. They are compatible because they count different units. Combined, they yield the claim that there is only one type of thing, but that there are many instances of this type. The point in all of this is not to survey the world of ontology and monisms, but to emphasize the importance of clearly specifying the target and unit for any monism, classificatory or otherwise. Changing either unit or target changes the meaning of the ascription.

For the purpose of target and unit specification, some terminology will be helpful. Kinds begin with things. These things may be ideas, relations, groups, individuals, processes, or objects. They are the things to be classified. Out of convention call these 'particulars', but notice that they may be singulars or groups thereof. Particulars get divided into categories. These are the kinds—call these the 'kind categories'—which may themselves serve as particulars for yet further categorization. The sum total of all of the categories, categorising all of the particulars, is the 'taxonomy' [*fig1*]. Taxonomies are often depicted as hierarchical, containing kinds of kinds, but they could be much simpler, containing no superordinate

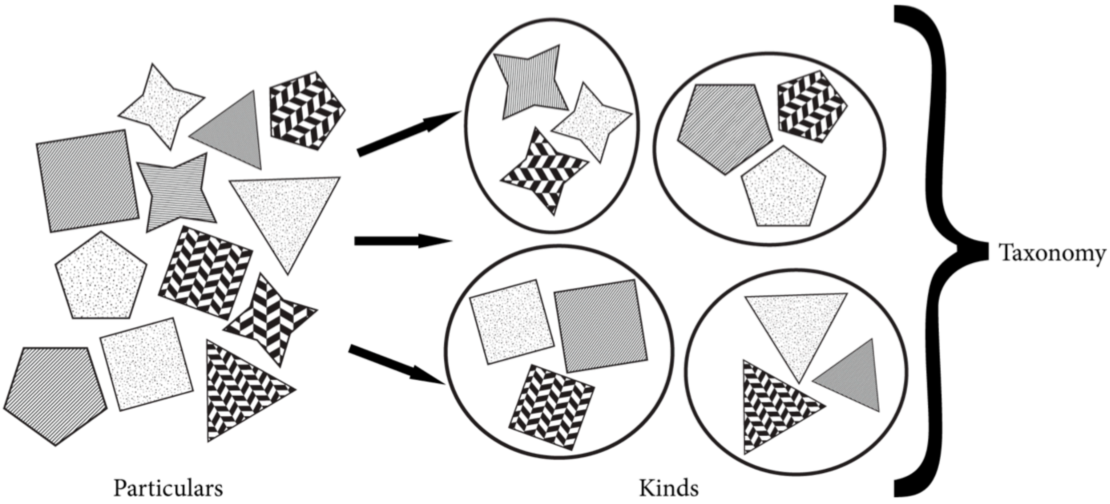

fig1: The Units of Taxonomy

categories. This all holds true whether we are discussing natural kinds or simply human practices of classification.

Discussions of monism/pluralism oscillate between two different targets, with correspondingly different units. Some discussions call for an examination of taxonomies, counting their number; others call for examination of kinds, counting types. These are the two readings of monism/pluralism that I will distinguish. The former has implications for the uniqueness of the identities of particulars; the latter has implications for metaphysical interpretations of theoretical pluralism. It is all too easy to erroneously run the two together.

1.1 Taxonomic Monism and its Limitations

The received interpretation of classificatory monism targets the taxonomy, counting by token. This is 'Taxonomic Monism':

Taxonomic Monism (TM): ***There is a single unique taxonomy***

Taxonomic Pluralism (TP): ***There is more than one distinct taxonomy***

A token taxonomy is, admittedly, an unfamiliar notion. Focus at the taxonomic level is on the number of distinct arrangements of particulars into kinds. Two taxonomies are distinct if they are non-translatable. Distinct taxonomies may have different token kind categories, different particulars in the same categories, the same particulars in different categories, and so on. Many differences can make for a plurality of taxonomies. Perhaps this is often difficult to determine, but the actual determination of TP or TM (or any monism) is an epistemic concern and thus not my own. The core of TM is that there is a single, unique, best, or most natural arrangement of particulars into kinds. Sometimes this is articulated as the claim that, among many possible arrangements, *one* is privileged. Other times, this comes through as the claim that the world's kinds have a unique or definite structure. TM is the interpretation behind Ruphy's 'metaphysical monism', quoted above.

One reason for interest in TM is its promise of a real sense in which science can get its classifications right … or wrong. This claim is bound up with the objectivity of science, and is often seen as a requirement for scientific realism. I will return to this shortly.

Besides taxonomies, oneness and manyness can occur at any level of kind classification. Monism targeting particulars receives attention under the banners of existence and category monisms, noted above. For kinds these positions are uninteresting. This leaves only the kind categories themselves. Counting token kind categories is possible but trivially unhelpful. Unless there is only one natural kind, we expect a plurality of token natural kind groups. Counting category types, however, allows us to describe similarity and difference in the world's kinds in a manner quite different from that permitted by TM. Not only do we want to know whether there is a single taxonomy, we also need to know if there is one type of kind category.

It may seem at first glance that different kinds make different taxonomies, but this is not necessarily the case. I will shortly show how the relations between the two monisms are in fact not so simple. First, to introduce this position and illustrate the need for a second monism targeting the kind category, consider the following illustrative tale:

A classic image in discussions of classification and kinds is that of an ontological butcher 'carving' nature at its joints.[73] Gruesome as it may be the image is helpful, for it drives the TM reading of monism. Instead of a butcher, however, I will talk about an 'ontological anatomist' which is both more apt and easier to stomach.

The ontological anatomist spends her days carving at joints. Using major joints as her guide, she discerns that the thigh and the rump go together; as do the shin, calf, and foot; and the same goes for the breast, rib, and loin. These are three large categories; but the anatomist can do better than this. She is familiar with the most nuanced of cuts, capable of finding the smallest of joints. If required, she can carve at the joints of the foot, neck, or hand. However fine a grain of carving required, the subject admits of a (finite) hierarchy of natural categories, marked by joints. But no matter how many times she wields her knife the resulting

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[73] This image comes from Plato's *Phaedrus*. Socrates claims that the world comes divided into parts and that a good interlocutor 'is able to cut up each kind according to its species along its natural joints, and to try not to splinter any part, as a bad butcher might do.' (265E). Socrates and Phaedrus are speaking here of love and rhetoric—specifically the fact that one must be conceptually clear in dialogue and that the language we use should map to the structure of nature. Nevertheless, the general idea that the world is pre-divided has obvious carry-over to discussions of natural kinds. A certain class of discussions about essence and reference in metaphysics and philosophy of language retains some affinity to Plato's original discussion. These are different from the essentialism in this thesis. See Chapter 2.

arrangement is the same. It is a uniquely natural arrangement. Our anatomist believes Nature to be monistic. This is TM.

Suppose that a new ontological anatomist arrives on the scene, carving not at the joints but at points where his knife cuts through smoothly, adopting an apparently pragmatic approach to his work. Sometimes he happens to carve at the meeting of two bones, other times not. But he is not bothered. He insists that nature has marked its kinds in many ways and that we would be foolish to privilege joints over changes in tissue type, cartilaginous fusions of bone, bone density changes, and so on. These are all excellent natural places to carve. Any point of physical difference is apt to be separated by his knife—if sharp enough—and the particular points he happens to pry depend on his needs, interests, and desires. Any and all of his carvings, he explains, will yield workable natural classifications. Each time he wields his knife the resulting arrangement may be distinct from the last. All of these may be equally natural. He believes Nature to be pluralistic. This is TP.

For the old anatomist, *qua* taxonomic monist, what makes the upstart so objectionable is that, in his lab, there is no uniquely correct answer to the question: 'to which category does *this* piece belong?' The answer will be: 'it depends on my needs at the time of carving'. He does not believe that any one of these carvings is privileged.

But the anatomists do not *just* disagree about numbers of taxonomies; their taxonomic dispute is just one corner of a more substantive disagreement. They have very different ideas of how to do their jobs because they fundamentally disagree about Mother Nature's joints (kind categories). While the old anatomist believes there is only one type of joint in Nature, the upstart disagrees. He believes there to be many.

Does this disagreement impact our ascriptions of 'monism' or 'pluralism'? The category dispute is hardly ineffectual, for it undergirds differences at the taxonomic level. Their respective views on Nature's categories cause the old anatomist to carve one taxonomy and the upstart to carve many. Efficacious as it may be, however, divergence in views about Nature's categories perhaps seem incidental from the standpoint of the monist if we think that oneness or many-ness about Nature's categories will show up at the taxonomic level, as in this case. This is very often the case in scientific disputes, where different conceptions of the kind category turn out to be driving competing taxonomies. Sometimes these are disagreements about how Nature marks her joints; other times they are disagreements about the entailments and requirements of kind membership. Regardless, when we say in these cases that the science is 'pluralist', based on taxonomic differences, we are in effect also capturing any additional disagreements about kind categories. One might thus think that a stance on Nature's joints *just is* a unique taxonomy. For even if the upstart had just one idea of Nature's joints (rather than many) he still would have come up with a different taxonomy from the old anatomist so long as their conception of nature's joints were not the same. There is an apparent dependence of taxonomies on kind categories.

This dependence appears to gain support when we consider cases of identical taxonomies resulting from (apparently) different conceptions of Nature's joints. To really stretch the metaphor, suppose one anatomist believed nature to have marked her joints using axes of movement, while the other focussed on fluid-filled synovial capsules. These different conceptions nonetheless produce inter-translatable taxonomies, and thus a verdict of TM. And yet this monist verdict seems appropriate, for shared taxonomies reveal that difference in the two conceptions was superficial (anatomically, axes of movement *are* fluid-filled synovial capsules). Sameness or difference at the taxonomic level seems to trump sameness or difference at the level of kind categories.

It really does appear that taxonomic output is determined by and thus tied to a conception of Nature's categories. It looks as though differences in categories just are taxonomic differences—and thus captured by TM/TP. But looks can be deceiving. Consider a postscript:

The old anatomist, frustrated by the young upstart, consults Mother Nature. Nature confirms that the upstart was indeed mistaken, but so too was the old anatomist. Nature's categories are anatomical, marked using joints in the way supposed; but this only holds true for musculoskeletal pieces. In the brain and central nervous system, Nature explains, kinds are not anatomical, but functional.[74] These kinds are accordingly marked not by joints, but with a capacity for function. Nature, it turns out, has more than one sort of category.

Is Nature monistic? The taxonomic interpretation seems to misjudge. Though Nature has two types of kind category, they are relativized, each occurring in a circumscribed domain. This means that there exists a uniquely natural taxonomy, and so for any given particular there is a unique answer to the question: 'to which category does *this* thing belong? From the taxonomic standpoint we must conclude that nature is monistic. Surely this misses something.

'Monism' and 'pluralism' are supposed to capture something about the homogeneity or heterogeneity of the world. Here, Nature has revealed herself to be heterogeneous in an important respect: 'Kind' is not one thing, but two. There are two different sorts of kinds, one structural, one functional, which come with different conditions for and entailments of membership. This is different from a case where Nature has just one type of category, as in the old anatomist's original position; but TM and TP lack the resources to characterize this difference. The singular label, 'monism', hides underlying pluralism. While multiplicity in

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[74] To be clear, I am not advocating any of these accounts of kinds. It's a story.

types of kind categories may seem incidental when it happens to line up with plurality of taxonomies, it seems much more important to recognize when concealed underneath a single taxonomic structure. We need a monism/pluralism targeting kind categories.

1.2 Category Monism

TM cannot capture all of the relevant information in the above cases. My point is not that TM is an irrelevant measure; just that it is an insufficient one. Category monism and pluralism are precisely the sorts of concepts the ontological anatomist needs to make sense of her new position.[75]

Category Monism (CM): ***The world admits of one type of kind category***

Category Pluralism (CP): ***The world admits of multiple types of kind category***

Much of this section will be dedicated to articulating CM, focussing on explicating the notion of a category type.

Essences are answers to the question: 'what makes this object a member of its kind?'. For the old anatomist an essence is a relation to a joint. For the novice anatomist there are many essences, all marked by some physical difference. Different conceptions of what makes a particular a member of its kind drive different conceptions of the kind category. The same is true in philosophical and scientific disputes. Essential differences are the principal ways in which philosophical (and scientific) stances on the kind category vary.

There is some tacit recognition of the category dimension of monism in the literature, albeit generally mixed with observations about scientific classification. In Ruphy's (2010) analysis of celestial classification, for instance, she helpfully insists that we separate arguments over particular views about kind membership conditions from 'the claim that there exists a *single* kind-membership condition (or set of them)' (p.1114, *emphasis mine*). Though she stops short of calling this latter claim 'monism' (identifying it instead with essentialism, which she takes to entail a taxonomic monism) she is correct to note that debates about the number of kind-membership conditions are worth pursuing on their own terms.

Ruphy is not alone. In his discussion of biological species, Dupré (2001) may not explicitly discuss the kind category itself, but he does give serious weight to considerations of different approaches to the science of classification:

> …there is no theoretical grounding for a classificatory system that will universally or even generally provide a practically applicable taxonomy [of biological species], we are free to embrace *taxonomic pluralism*. Approaches to classification will vary from one group of organisms to another … In many parts of biology, for example bacteria and many orders of flowering plants, it is doubtful whether any evolutionarily

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[75] To the best of my knowledge this TM/CM distinction is novel, having been introduced separately yet simultaneously by Matthew Slater and I. See his brief distinction between 'taxic' and 'category' pluralism (Slater 2013 Ch. 7) compare to (Bartol 2014). Slater does not dwell on the distinction.

> grounded taxonomic scheme will be feasible, and it may be necessary to resort to morphology. (p.209, *emphasis mine*)

Dupré calls this TP, which is it, but it is more interestingly also CP. He is not simply making the familiar point that biologists use competing taxonomies. Rather, he is claiming that biologists *need* different conceptions of species (different conceptions of the kind category) in order to make sense of different domains of the biological world. This is CP applied to extant classificatory techniques, rather than natural kinds; but the concept is the same. And since Dupré gives descriptive and normative accounts of scientific practice a serious role in his (deflated) metaphysics of kinds, it is not a far stretch to turn this CP into a metaphysical thesis.

There are two ways to read the implications of this claim: (1) There are multiple taxonomies, each corresponding to a unique kind category, and some of these taxonomies are better at representing some domains and worse at representing others; or (2) Different domains feature different kind categories, which are relativized to that domain—and so there is one taxonomy (as in [*fig3*]). Dupré appears to opt for (1), embracing taxonomic pluralism, but (2) is equally compatible with the CP he espouses. At any rate, this range of possibilities is impossible to see with only one concept of monism—the taxonomy/category distinction is at the very least useful. Because conceptual utility is not enough, in the next two sub-sections I prove that TM/TP and CM/CP are logically independent.

CP does not Entail TP. Claims about taxonomies are claims about the order or grouping of things in the world. Claims about categories are claims about the nature of those groups. These are difficult to disentangle, since TP often seems to come via CP. One set of particulars wholly sorted into two kinds of kinds would yield two taxonomies. If every particular in a set is subject to every type of kind category, then there will be as many taxonomies as there are types of kinds. In [*fig2*], for instance, there are two taxonomies of the same particulars in virtue of two types of kinds. Dashed lines and different likeness relations represent different kind categories. This is TP via CP.

Yet CP can also occur in a taxonomically monistic system. This occurs when types of kind category are localized to specific sub-sets of particulars. This was illustrated in the postscript about the ontological anatomists, and is also described in the second interpretation of Dupré, above. In [*fig3*], one set of particulars are classed using two different kind makers in a non-problematic way. Some particulars are members of their kinds because of their colour, while others are members of their kind in virtue of their geometry. If this strikes you as *prima facie* implausible, I will come to some examples shortly.

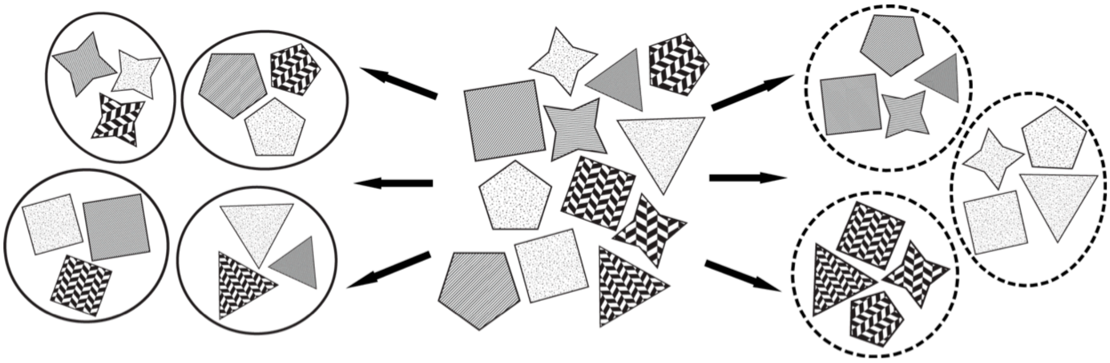

fig2: Taxonomic Pluralism in Virtue of Category Pluralism. One set of particulars are wholly sorted into two taxonomic arrangements, owing to two different types of kind (shade and shape).

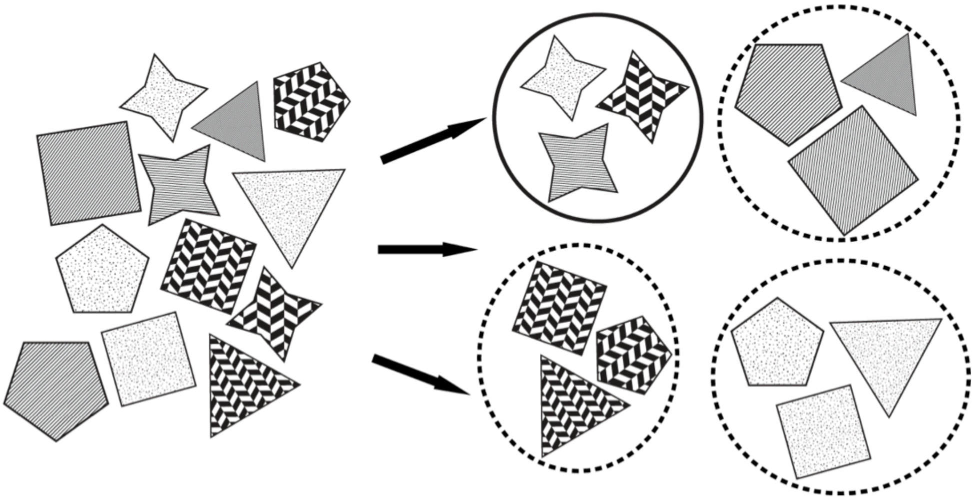

fig3: Category Pluralism. One set of particulars are sorted into a single taxonomy, using two different types of kind, relativized to specific sub-sets of particulars. Those with eight sides are subject to kinds based on shape, all others are subject to kinds based on shade.

There is a possible misreading of these figures, which is instructive. Both types of kind category pictured utilize structural properties to sort their kinds. Perhaps this difference is insufficient to warrant the claim that the world admits a plurality of types of kind categories. Perhaps these differences are not difference enough. It should become clearer as we proceed, however, that more substantive differences in kind categories may be possible. It is nonetheless worth highlighting the fact that not just any difference would render two kind categories fundamentally different. In these figures, trivial differences like shape and shading might be best interpreted as representations of deeper ontological difference.

TP does not Permit Inference to CP. That CP does not entail TP is reason enough not to roll the two monisms together. Yet these cases do not rule out entailment in the opposite direction. It is hard to imagine how plurality of taxonomies could be supported by a single type of kind category (TP & CM). Fortunately, we have already seen a position that may be stretched to fit our need: the upstart anatomist.

Many discussions of kinds begin with the observation that there are numerous divisions in the world and that many of these appear to be natural. In a simplified case, think of these as the many different properties that a set of objects possesses. Similarities and differences across objects abound. Monists suppose that it is the job of the natural kind theorist to figure out which of these are privileged. The upstart anatomist disagrees, as you will recall. He thinks that carving along multiple natural properties provides different kind classifications and that these may be equally natural. The upstart thus views the world as composed of innumerable natural properties, some of which naturally go together to form kind classifications, many of which will not cohere with one another, thus comprising distinct taxonomic arrangements: TP.[76] To get CM, all the upstart needs to do is to claim that only one *type* of kind classification is natural or that only one type of property supports natural classifications, but that there are many classifications of that type. Perhaps all and only the orthopaedic properties mark kinds. If so then the multiplicity of such physical properties entails TP, but restriction to these properties entails CM.

1.3 Two Objections to CP

There are two lingering objections to the possibility of CP, which deserve attention. The first asks after the nature and degree of difference that constitutes a new category. The second doubts that it is possible for different types of kind category to still qualify as 'kinds'. On balance, the first objection is epistemic, the second semantic. Though instructive, neither pose serious problems for the metaphysics and ontology of natural kinds.

A critic may wonder which differences, or how much difference, constitutes a new type of kind category. Any time we have differences in scientific classification that look like CP, we cannot rule-out that the differences are merely artefacts of cognition. Suppose we were to appeal to two domains of science that manifestly require different essences. Even granting that both *are* describing natural kinds, the critic can still claim that the ontological category underneath is in fact unified. Heterogeneity in the kind category is only apparent, claims the critic, perhaps a function of our fallible attempts to classify things into kinds.

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[76] Chakravarrty's (2011) 'sociability-based kinds' provide a full treatment of this sort of position, absent the extra claim that gives me CM.

There are two worries here: one is scepticism about scientific knowledge and the other is a more serious worry about the unified/disunfied nature of the kind category. There is little to say about scepticism except to hope that the right mix of science and metaphysics can carry the day. The second worry has more bite. This criticism is more easily motivated via scientific classification, rather than natural kinds. Consider the various competing approaches to biological species. Any observer must acknowledge that at least some of these differences are cosmetic, reflecting fallible attempts to find epistemic handles for the same underlying kind category.[77] This is arguably the case with interbreeding and genetic approaches to species, which are attempts to cash-out the intuition that species are groups that can persist as groups into the future, where the mating of two members does not (normally) give rise to particulars of new species. One might attempt to describe many species concepts in such a way that they all come out as differing attempts to grapple with ostensibly interchangeable notions of what makes a species what-it-is. Returning to the natural kind versions of this objection, it is possible that even a metaphysics of natural kinds may fall victim to this sort of accidental consilience of kind categories, masked by superficial differences.

There is little to be done to satisfy this objection except acknowledge that distinctions between types of kind category are at times murky. Nevertheless, I insist that starker differences in kind categories arise when we move beyond singular taxonomic endeavours and look across broader domains. In the next section I will claim that the kinds of biological world are categorically distinct from those of the physical world. One type are historical, the other are physical. These different kind categories underlie important differences between their respective kinds. I leave it to critics to show how historical essences, on the one hand, and intrinsic physical essences, on the other, might fail to mark a difference in kind category.

But first there is a related objection. Suppose we agreed that two domains of objects really did have fundamentally different kind categories. A critic might then press for an explanation as to why both of these categories are *kind* categories. Perhaps one is a kind category and the other is something else entirely. If so, then it would hardly warrant CP. Playing the critic's game, it is hard to imagine what sort of account would satisfy the demand that we show how these are both kind categories that did not also establish that the two types of categories are, in some more fundamental way, the same type of kind category after all. Either we admit that the two types of category are not both natural kinds, or we run the risk of establishing that they are at base the same kind of kind.

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[77] This is the intuition that Whewell pressed in the geological case from Chapter 1. See (Ruse 1978)

I do not think we need to play the critic's game. Perhaps we might want to claim that the two categories *are* both kinds, but resist the further claim that both are members of some yet more-fundamental singular *Natural Kind*. This resistance would not be without principal, for this further claim seems to assume rather than establish category monism. Alternatively, we could admit that only one category is a *kind* category and the other is *kind**. Call this CM if you wish, but the existence of *kind** is philosophically significant nonetheless. It is hard to imagine what interesting metaphysical or ontological claims follow from CP that do not also follow from the existence of *kind* and *kind**. This second objection amounts to little more than a semantic dispute.

2. Biological Kinds are Historical Kinds

According to the received view, in the world investigated by the physical sciences, when it comes to natural kinds, things are what-they-are in virtue of their physical structure. The canonical kinds of physics and chemistry are united by structural essentialism. According to the account developed in this thesis, this type of kind category does not extend to the biological realm, for in biology kinds are determined by long-run interactions between particulars, development, and the environment. Biological kinds are not structural; they are historical.

Structural essentialism is at its best when applied to the paradigm case of natural kinds in science: atoms. It is generally agreed that atoms are all what-they-are in virtue of their unique atomic micro-structures (or simply, 'structures').[78] Similar accounts can scale up to describe larger molecules (as I will describe in the next chapter) or down to describe the fundamental particles of physics (e.g. Lange 2011).

For these objects microstructure is essential in the sense defended in Chapter 2. All other properties of a microstructural kind particular can be gained or lost without affecting the fundamental kind of thing it is. An atom, for instance, can have a different genesis, location, mass[79], and it can be bound to different partners, all without changing its kind. Yet changes to atomic structure constitute a change in kind. An oxygen atom can be part of H_2O or CO_2. It can originate from fractional distillation or helium fusion. It can exist on earth or in space. It

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[78] I am skeptical that microstructuralism should be taken to support the periodic table of elements as a natural kind classification. Surely the periodic table is too coarse, and a taxonomy focusing on nuclides or isotopes is more in line with microstructuralism (since one element in the periodic table will subsume multiple physically-distinct isotopes/nuclides). But this thesis is about biology, not chemistry, so I will adopt the received view of microstructuralism and the periodic table as the default account of natural kinds in chemistry. At any rate, the change I imagine would constitute an amendment to the application of microstructuralism, not a challenge to microstructuralism as a theory of non-biological kinds. See Chapter 4 for more on microstructuralism.

[79] Relativized atomic mass; not relativized isotopic mass.

can be stable or transient. But if it is bombarded with particles and loses a proton then it is no longer oxygen, it's nitrogen.

Structural essentialisms are a poor fit for biological kinds, owing to intrinsic heterogeneity. It is on differences in physical traits that evolution acts, and so insofar as evolvability is a feature of the biological world, intrinsic heterogeneity is a feature of the biological world. Every property of a biological object is the result of complex and highly contingent historical processes—interactions between selection and the world. Because the process occurs imperfectly, in different environments with different starting conditions and different intervening forces, intrinsic heterogeneity is inevitable. Outside the realm of organisms intrinsic heterogeneity is pervasive as well; even cells exhibit considerable structural variation within types (Slater 2012). Increasing intrinsic heterogeneity has recently been proclaimed the 'first law' of biology (Brandon & McShea 2010). In all of its forms, intrinsic heterogeneity is a problem for structural essentialisms. Structures (and microstructures) are heterogeneous and so none are essential. This point is at the centre of the anti-essentialist consensus (Chapter 2), which is more appropriately viewed as an anti-structural-essentialist consensus. No single physical property is shared by all and only members of a biological kind. Any physical characteristic, any genotypic signature, any physiological structure is liable to variation. The biological world simply does not do uniformity.

Members of biological kinds do not share structural essences. They do, however, share histories. A history (or a set of historical relations) is the one and only thing that a biological kind cannot lose without ceasing to be what-it-is. Genes can mutate, physiology can change, all physical properties are up for grabs; but histories are not. On this view, when we say that two biological particulars are kindred, we cannot claim this relationship on the basis of shared properties, however often we use physical properties as epistemic handles for such claims. We cannot mean that they are ontogenetically similar, since they may happen to develop in divergent ways. What we mean is that those particulars posses some specific type of historical relationship—about which I will say more shortly.

Historical routes through selection, chance, and evolution play a large role in determining the existence and nature of present-day biological kinds. That chance and evolution furnish the world with its current biological kinds should be obvious. Some biological kinds persist through time because they are fit, others because they are lucky. In addition to affecting the existence of biological kinds, these historical interactions also affect the way the kinds are—what they are like. Interactions between kinds and their environment determine the fitness of particular variations on that that kind. Relevant fitness differences between variants within a kind conspire to shape subsequent generations of particulars of that kind. Of course selection is not everything. Many features of biological kinds are retained or lost due to drift. These chance events, too, impact the make-up of subsequent generations. When looking at a biological kind and asking after the present state(s) of its particulars, we look to their histories of chance, function, drift, and selection.

Ruth Millikan (1999a, 1999b) and Paul Griffiths (1996, 1999) have separately called attention to historical essentialism for biological species.[80] Though both move between natural kinds and induction-supporting kinds, their views provide, at minimum, accounts worthy of consideration for present interests.

With induction on his mind, Griffiths attempts to explain why we can expect historical processes to provide unity to members of biological kinds. He explains that heredity acts as a force, which he calls 'phylogenetic inertia', ensuring that organisms of shared descent stay relatively similar in their properties until some adaptation occurs. That the similarity is imperfect is not a problem for this view, since physical properties are non-essential.

> This *phylogenetic inertia* is what licenses induction and explanation of a wide range of properties –morphological, physiological, and behavioural– using kinds defined purely by common ancestry. If we observe a property in an organism, we are more likely to see it again in related organisms than in unrelated organisms. Since Darwin, this idea, much elaborated, has been the basis of comparative biology. (Griffiths 1999, p.220)

Millikan builds on this point to arrive at a partial account of intrinsic heterogeneity. Appealing to the imperfection of the biological copying mechanisms that link moments in phylogenetic histories, Millikan explains the poor fidelity with which generalizations ranging over those kinds hold.

> [H]istorical kinds are unlikely to ground exceptionless generalizations. The copying processes that generate them are not perfect, nor are the historical environments that sustain them in the relevant respects. (Millikan 1999a, p.55)

These accounts explain how a robust essentialist account of biological species as induction-supporting kinds can yield kinds that fail to feature in laws, fail to ground exceptionless generalizations, but nevertheless feature in reliable inferences and explanations.

Both accounts of induction-supporting historical kinds offer interesting explanations of the diversity and similarities within biological classes.[81] More relevantly, however, these

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[80] Elder (1995) presents an account of historical kinds, as well. However his account focuses on the structural and teleofuncitonal connections between members of kinds, not their historical relations.

[81] There are criticisms of the historical essences view as an account of induction-supporting kinds. Most notably, Chakravarrty (2007) notes that phylogenetic inertia does not *guarantee* that properties will be shared. He thinks that if inductions work for these kinds then they work because the kinds are HPCs, not because they are historical kinds. The attack appears misplaced. Phylogenetic inertia is part of the causal homeostasis that supports inductions. But we need not expect inductions to be perfectly guaranteed. In fact intrinsic heterogeneity should lead us to expect exception-*ridden* generalisations. Chakravarrty has found a virtue with the account, not a problem. See also the exchange between Boyd and Millikan (Boyd 1999b, Millikan 1999b).

authors also provide an argument for historical kinds as natural kinds. Griffiths is explicit on the matter. He argues that historical properties are essences, the only properties that organisms cannot lose without ceasing to be the kinds of organisms that they are:

> Although Lilith might not have been a domestic cat, as a domestic cat she is necessarily a member of the genealogical nexus between the speciation event in which that taxon originated and the speciation or extinction event at which it will cease to exist. It is not possible to be a domestic cat without being in that genealogical nexus. Furthermore [such kinds] have no other essential properties. (1999, p.219)

This is a specifically biological version of Kripke's famous claim about history as essence, nearly two decades earlier, though Griffiths does not notice the connection.[82] Millikan does not make this same claim, focussed as she is on the role of natural kinds in laws and explanations. She does however refer to historical essences as 'the ontological ground' of inductions ranging over the kinds, suggesting that she may view historical kinds as more than just convenient tools for biologists.

As I will illustrate in the next chapter, the historical kinds account is useful beyond Lilith and her ilk. Processes of drift, selection, and evolution give us far more than just species. Many categories of biological object are historical kinds. In the next section, I will examine the implications of the historical kinds view as an account of natural kinds.

3. Historical Kinds are Categorically Unique

Structural kinds and historical kinds constitute distinct types of kind category. Differences in category type underpin other interesting and relevant differences between the kinds. I will highlight the most relevant of these differences here. The first set concern the relation between essences and properties of the kind. The second set concern the possibility of change within the kind. Together, these features of categorical distinctness account for widely-recognized core features of the biological world often thought to preclude biological natural kinds.

Static physical essences can function as reliable causes with a degree of fidelity not possible with historical essences. A physical property, in a world governed by physical laws, can and will reliably cause other physical properties. Since the physical essence will occur in all members of a kind, so too will any properties that it causally supports. Physical essences may causally guarantee certain other properties, as when no supporting conditions need be present, or the essence may render other properties likely, as when context matters. In the

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[82] In *Naming and Necessity* (1980), Kripke famously argued, in a buried footnote, that the *only* feature of things that was truly essential was their origin. This is not quite the claim on offer here, since Kripke was talking about individuals and not kinds, but the basic point is similar. The belief is that the only feature of thing that cannot be faked, lost, or construed as accidental, is its origin.

case of the chemical elements, atomic structure reliably supports a host of characteristic properties. Atoms' weights, ductility, conductivity, malleability, and even colour can be traced to the atomic microstructure. Many of these remain stable across all possible physical conditions.[83] Even the dispositional causal profiles of atoms are tied to atomic structure. Atoms will exist in specific states under specific conditions, will react with certain partners, and so on. All of this is uniform across the kind because of static shared physical essences, in a world governed by static physical laws and regularities. The relative homogeneity of the characteristic properties of physical kinds is a result of the *type* of property that serves as their essence.

This causal link also happens to make many structural kinds ideal induction-supporting kinds. Projections over kinds hold for any property guaranteed by the essence. The strength of other projections is determined by the likelihood that any necessary supporting conditions are present. It is possible that the Essence-Properties-Principle, critiqued as unmotivated in Chapter 2, arose out of consideration of canonical natural kinds such as these. Rather than a constitutive feature of all natural kinds, the link between essence and properties may be an accidental feature of structural kinds.

Historical kinds lack the possibility of so strong a casual link between essence and properties. With historical kinds there is no static property that can serve as an anchor in reliably recurring causal events. The shared property is a history, which is not the sort of thing that is identical from one particular to the next. Each particular has its nature determined by a historical trajectory through time, but the specific way in which that historical legacy impacts a given particular will depend on the context. Since contexts vary, so too do kindred particulars.

A history is a curious thing. To say that two particulars are kindred in virtue of a shared history is not to say that they share their *entire* history, for then they would be numerically identical. Rather, kindred particulars share a long evolutionary history and diverge at some comparatively recent point. This divergence makes all the difference. The particulars will differ in starting conditions and will face different developmental pathways. They will likely even differ in intrinsic structure—one may acquire a DNA/RNA mutation, for instance. Precisely how they differ will depend on the details but that they will differ in some way is virtually guaranteed. Even in cases where the divergence occurs extremely recently, small developmental differences can yield large effects. Any set of twins can tell you that. The two California lemons in my fruit bowl share a trajectory beginning with the hybridization of citrons and sour oranges, including a trip across the Atlantic courtesy of Columbus, and

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[83] Some physical conditions will of course destroy the atomic structure (knocking off a proton or two) but at this point the properties are not expected to persist because the kind, too, has ceased to exist.

diverging at some point just before the planting of the seeds that yield the trees that grow the lemons. Between the divergence and now, much will have happened to yield two lemons that are distinct in at least some of their physical properties. This is why one is large and the other small. One has slightly thicker skin. One is mildly sweeter and better hydrated. Taken across an entire lemon grove, the heterogeneity in even a relatively genetically homogenous population will be surprisingly large. The heterogeneity of properties within a biological kind is a result of the historical nature of their essence.

Historical kinds thus do not happen to be great induction-supporting kinds. The strength of an induction is determined by the frequency of shared properties. Unlike structural kinds, the historical kinds of biology do not have the types of essences that guarantee characteristic properties. Biological kinds are therefore full of particulars that lack certain properties and so projections about those properties will occasionally fail. This is not to say of course that historical biological kinds cannot support inferences. They can and do. Rather, these kinds will not support perfectly general inferences in the way that structural and induction-supporting kinds may.

Members of historical kind will indeed have various features in common. It is no accident that both of my lemons are recognizable as lemons. It is not because I can see into their histories, but because those histories have rendered them objectively similar. In spite of their differences, they still have enough in common to be recognizably similar. That is similarity enough to ground certain weak projections from one to the other, or from this sample of two to a larger class. This is a point stressed by Millikan. She writes:

> Historical kinds are domains over which predicates are non-accidentally projectable: there are good reasons in nature why one member of an historical kind is like another, hence why inductions are successful over the kind. (1999a, p.55)

Though she notes the reduced strength of these projections, she is careful to ground this reduced strength in features of the very same mechanisms that make the projections possible in the first place:

> On the other hand, historical kinds are unlikely to ground exceptionless generalizations. The copying processes that generate them are not perfect, nor are the historical environments that sustain them steady in all relevant respects. (p.55-6)

The historical nature of biological kinds explains the existence and limited scope of their ability to feature in inductions.

The second striking difference between structural and historical kinds is the possibility of change to the kind. At issue is not whether a particular can change kinds but whether the kind itself can change over time. The question is whether the make-up of members of a kind at time t can be interestingly different at some later time t_{+1}. Structural kinds do not change in this way; historical kinds do.

Static physical essences cannot change, by their very nature. The microstructure that makes an Oxygen atom Oxygen will be the same 2000 years hence as it is now. It is conceivable that an atom with a structure hitherto unrealized in this world will at some point in the future come into being. But this is a new kind, not a change to an existing one. Particulars come and go, forming old, new, and different kinds, but the kinds themselves remain—whatever particulars have this or that essential physical structure. Perhaps it is this feature of physical kinds that has led so many to assume that unchanging natures are a constitutive feature of all natural kinds.

Historical trajectories, by contrast, are by their very nature developing things. A history is not a static thing, but an evolving one. Particulars of kinds with historical essences can physically diverge not only at the same time, as described above, but also across time. A particular on a historical trajectory at time t may be quite different from a different particular on that trajectory at time t_{+1}. Events will have cropped up along the way that impact its makeup. Changing kinds are the expectation within Kind Historicism.

In sum, I have provided an introduction to Kind Historicism and a sketch of its implications. In the next chapter Kind Historicism will be put into action, resolving a dispute about proteins as natural kinds. Thereafter, I will examine a role for Kind Historicism in resolving questions of biological individuality. After a detour to discuss induction-supporting kinds, in Chapter 6, I return in the concluding chapter to the implications of Kind Historicism.

4. An Aside on Realism

Pluralism is often associated with scientific anti-realism. The motivation perhaps stems from realists' commitment to the mind-independent structure of the world. This is summarized in Stathis Psillos' (1999) influential characterization:

> The metaphysical stance [on realism] asserts that the world has a definite and mind-independent natural kind structure … [this] thesis is a basic philosophical presupposition of scientific realism. It is meant to make scientific realism distinct from all those anti-realist accounts of science … which reduce the content of the world to whatever gets licensed by a set of epistemic practices and conditions. (p.xix)

But there is no sound reason why realism should be associated with either pluralism or monism, exclusively. Monist positions may be the most attractive to the realist, but there are viable realist options along both the category and taxonomic dimension. In order to see this, we must stress the distinction between 'definite' structure and 'singular' structure. The world can have a definite structure without that structure being monistic. It can definitely and really be the case that the world admits of multiple taxonomies, for instance. The difference is obvious when pointed out, but still seems easily forgotten.

TP is entirely compatible with realism about natural kind classifications. One might think that the world really does not have a *single* unique structure without denying that there is real structure. As an example, Anjan Chakravartty (2011) has recently advanced a realist TP, according to which natural kinds are any group of particulars based on shared properties. Since particulars have many properties, and since no single property is fundamental, one particular will belong to multiple natural kinds. The distributions of properties are real, and so are the kinds. According to Chakravartty, nature really does have joints. They are innumerably many and admit of no singularly best way to carve. This is a taxonomically pluralist but realist-friendly option. The only concession that a TP advocate needs to make concerns the uniqueness of kind membership. This brand of realism still permits belief in pre-existing order, it's just that the order is complex and admits of innumerable non-overlapping groups.

Interestingly, pluralism with respect to categories is hardly a problem for the realist. The category pluralist can be realist about the particulars, the relations between them, and the kinds that they form. She needn't make any anti-realist concessions!

5. Conclusion

Robert Wilson made the following remark about the emergence and dominance of pluralism in the philosophy of biology. I think he gets the motivation for pluralism right, but errs when characterizing the benefits pluralism offers:

> For philosophers, pluralistic views often mark a departure from traditionally dominant views within the philosophy of science … Such views are seen, I think rightly, as imposing a sort of straightjacket on the biological sciences, forcing their conformity with the physical sciences taken as a paradigm within the philosophy of science until the last thirty years … So one motivation for pluralism within the philosophy of biology might be characterized, in the most literal sense, as reactionary in rebelling against dominant traditions within the philosophy of science. *But pluralism carries with it a more positive view of the nature of biological reality, of the biological world as more complicated, various, and messy than even our sophisticated views of theories, explanations, and kinds have allowed.* Pluralism aims to more adequately capture this complexity. (Wilson 2005, pp.12–3 emphasis added)

Wilson's view is in turns helpful and misleading. He is correct to note that the view of kinds in the physical sciences is a poor fit for biology. Structural essentialism simply will not suit. The biological world is importantly different, and category pluralism makes sense of that difference. This does not lead, however, to his 'positive view'. Biology can be deeply different from the world investigated by the physical sciences without being hopelessly messy. It is complicated, yes; but it need not be taxonomically pluralistic, which is what Wilson and this pluralist challenge imply. Intrinsic heterogeneity in biology is an expected outcome of historical natural kinds. Kinds in biology can be unified, mind independent, monistic, and still be internally heterogeneous in kind.

II

Applying Kind Historicism: A World of Evolving Ontology and Sciences of Limited Means

I began with three questions:

Q1: What kinds of things populate the biological world?

Q2: What does the correct answer to Q1 entail for monism and pluralism?

Q3: What role should this biological ontology have in scientific practice?

PART I sketched answers to these three questions in the abstract, by way of articulating Kind Historicism, a theory of biological natural kinds. PART II will add substance to these answers by applying this theory to biochemicals (Chs. 4 *&* 5) before addressing the practical limitations of natural kind theorizing (Ch. 6). The second part of my discussion of biochemicals, in Chapter 5, will address the lingering problem of biological individuality.

PART II offers a rather drastic change of style. The opening chapters of this thesis dealt in concepts—their histories, forms, and relations. Science was discussed, but at a certain distance. This distance aided in the isolation of distinctively metaphysical and ontological issues. The chapters that make-up PART II are not so hospitable to these lines of enquiry. Rather than developing and exploring concepts, the task is to bring them into contact with scientific knowledge. In Chapters 4 and 5, I examine issues that have received some measure of philosophical attention: biochemical kinds and biological individuals. In each instance, philosophers have been interested in (what I call) epistemic forms of these issues—though occasionally vaulting metaphysical and ontological conclusions, too. I ask, in light of recent work on the epistemic questions, what can we say about the ontological and metaphysical ones? The final chapter is again of a different form. Chapter 6 comes in two parts. In each, I advise caution in cases where philosophers and scientists have been overzealous in arguments over the precise ontological status of (i) human races and (ii) cognitive modules. The point of that chapter is not to illuminate the natural kinds, but to show how questions of natural kindhood are often not relevant to scientific investigation. Because all three chapters constitute a shift in the style of argumentation it may be helpful to first recapitulate, in brief, the answers to the three central questions, before pressing forward.

Q1: What kinds of things populate the biological world? The objects of the biological world are those that exist in the ways that they do in virtue of Darwinian processes of conservation, selection, and change. That is to say that many of the objects commonly thought of as biological do indeed come in kinds. Those kinds are Darwinian. They are what-they-are in virtue of their unique histories; they have historical essences. A thing is a member of a biological kind if it is a part of this sort of Darwinian lineage.

From this account a few interesting metaphysical features of kinds follow: there will be many biological kinds; biological kinds are relational, and so two kinds can be 'closer' or 'further' depending on the evolutionary details; members of a single kind will be heterogeneous in their properties and so causally heterogeneous also; and the biological kind can change over time, including coming into and out of existence. These features, in turn, have interesting implications for science: biological kinds will be imperfect tools for scientific inquiry, owing to their heterogeneity; and biological kinds are often unknowable, since phylogenetic histories can be epistemically inaccessible.[84]

A complicating factor arises when we remember that biological objects are composed of physico-chemical kinds. These kinds are not Darwinian, they are physical—microstructural, to be precise. They are what-they-are in virtue of physical microstructures. Characterizing this relationship is one of the tasks for Chapters 4 and 5.

Q2: What does the correct answer to Q1 entail for monism and pluralism? This question is more complicated than it seems. Monism and pluralism are not straightforward notions. I have separated taxonomic monism/pluralism from category monism/pluralism. The account of kinds presented in PART I is categorically pluralist and taxonomically monist. The theory is categorically pluralist because biological kinds and chemical kinds are two very different types

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[84] Bapteste and Dupré (2013), discussed at length in Ch. 5, describe cases in which the evolutionary histories of microbes are not just difficult to determine, but impossible, owing to excessive gene swapping. Versions of this problem, of differing magnitudes, will crop up across all biological kingdoms.

of natural kind. One is historical, the other physical. The theory is taxonomically monist because the physico-chemical and biological kinds do not cross-classify. Each set of kinds is taxonomically monistic.

The complicating factor arises again. The compositional relationship between biological and physico-chemical kinds makes it difficult to evaluate their taxonomic status. Even if we agree that a biological object has some unique kind membership in virtue of its unique evolutionary history, we still must reckon with the fact that, qua physico-chemical kind, there is (in some sense) a giant pile of molecules that compose that biological kind, which themselves have a unique kind membership in virtue of their collective microstructure. This complication is not so obvious when thinking in terms of large organisms (few have the occasion to think of a tortoise as a singe biochemical mass), but it becomes much more clear when we think about smaller biological kinds, such as biochemicals. Exploring the compositional nature of biological individuals and its implications for monism and pluralism is one of the tasks for Chapter 5.

Q3: What role should this biological ontology have in scientific practice? A principle implication of the theory of biological kinds on offer is that biological kinds should not necessarily furnish biological science with its taxonomic categories. Manifest ontologies in science have many properties that biological ontologies do not, and the facts required to generate a biological ontology will often be epistemically inaccessible.

In PART II, I will discuss a number of actual scientific enquiries for which a biological ontology is not at all well-suited. The case of proteins illustrates the epistemic inaccessibility and heterogeneity problems, the case of races illustrates the need for pragmatic attention to classification, while the case of cognitive modules describes a wealth of kinds that fail metaphysical scrutiny but nevertheless provide good categories for science.

4

Biochemical Kinds[*]

Protein molecules are an interesting case for philosophy of science because they are at once the objects of biology and chemistry, an in-between status that leads to conflicting intuitions. The first is that, *qua* chemical molecules, their physical structure is fundamental. The second is that, *qua* biological objects, their physiological roles are important to recognise and understand. The conflict manifests in a number of ways, but the principal problem is the classification of proteins into kinds. Consider the lens crystallin protein, which forms the lens of your eye but also 'moonlights' as a number of functionally distinct enzymes. Common structural classification, like that used for simpler chemical molecules, will gloss over this biological diversity. Intuitive functional classification will separate these proteins and so fail to highlight structural similarities. Privileging one of these classifications over another appears at best subjective and at worst arbitrary.

Two issues arise. First, we might ask after the actual scientific practices of classification, focussing on how scientists can, should, or do address this problem. Caught up with this inquiry we find a mix of questions about the aims, norms, contexts, and limitations of scientific investigation. Call this the 'epistemic question' about protein classification. Second, we might ask after the status of proteins as natural kinds. Is there a natural or correct

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[*] This chapter is closely based on a previously published paper. I am grateful to the referees of that paper for their helpful comments and suggestions—Bartol, J., (2014) 'Biochemical Kinds', *British Journal for the Philosophy of Science*, Advance Access 24 Dec, 21pp.

arrangement of proteins into kinds, no such arrangement, or many? In case there are natural kinds, we want to know what sorts of kinds they are and how they relate to one another. This second line of inquiry asks after the ontological structure of the biomolecular world and the metaphysical relations therein. Call this 'the metaphysical question' about protein classification. Recent work on the epistemic question leads me to turn my attention to the metaphysical one. This chapter asks, in light of what we know about proteins and biochemistry, what can we say about nature's joints?

William Goodwin (2011) recently argued that the practice of protein taxonomy begins 'fundamentally' with structural considerations, but classifications are then adjusted ad hoc as dictated by specific phenomena and scientific interests. Considerations of function, chemical properties, or different levels of structure may alter specifics of the classification scheme. Call this position 'pragmatic pluralism' about classificatory practice. Goodwin resists this label, but my use of it will become clear as we proceed.

Pragmatic pluralism about taxonomic practice is fairly open as regards metaphysical interpretations. Nominalism is always an option, of course, but so too are all of the various realist interpretations. Pragmatic pluralism is compatible with taxonomic and category monism. One might insist that there is a single set of uniform kinds, but that these are inaccessible or do not facilitate scientific inquiry. A permissive pluralist metaphysics is also available, which reads the metaphysics straight from the practice, assuming there are a wide range of taxonomies and categories. Perhaps structure is the most useful, but occasionally we must sample from taxonomies based on function or reaction profiles. Finally, a number of less permissive interpretations are available, which posit a select few taxonomies or categories. Neither biologists nor philosophers hold out hope for a tidy ontological reduction to either biological or chemical kinds. This rules-out metaphysical monism. Nominalism and the two pluralisms remain.

On its own, Goodwin's account privileges no particular interpretation. Two other recent papers, by Matt Slater (2009) and Emma Tobin (2010), also call for pragmatic pluralisms but venture beyond the epistemic question and into the territory of the metaphysical. These authors proffer a highly permissive brand of pluralism. They begin by noting the need for two categories and taxonomies, biological and chemical, but then claim that even the chemical side of the protein case is pluralistic, citing the physical underdetermination between a protein's initial amino acid sequence (called 'primary structure') and its final folded three-dimensional state (called 'conformation'). This problematisation is consistent with many scientific accounts (e.g. Copley 2012, Wright & Dyson 1999, Dyson & Wright 2005). Slater adds even more plurality, explaining that there are multiple legitimate ways to determine protein function. If accurate, these accounts point to the conclusion that there are many ways that proteins can be members of kinds, and even more taxonomic arrangements; possibly as many ways as there are structural and functional properties. It is widespread pluralism about both categories and taxonomies (though these philosophers do not attend to the distinction).[85] This is what I mean by 'permissive' pluralism.

In section 3, I dismiss the 'permissive' bit of the pluralism, arguing that physical underdetermination is in fact not a problem. Microstructuralist accounts of chemical kinds are well equipped to treat higher-level chemical structures as constrained by the lower-level microstructure. The multiple-realization of function, too, is not a problem since an adequate biological kind classification is not functional but historical. The pressing issue remains the multiple-realisation of physiological roles by chemical microstructures.

With the conflict thusly framed, I dispense with the nominalist option as unmotivated and introduce my position, which is a far more restricted pluralism. I remind the reader of my duality of kind categories: the biological (historical) and the physico-chemical (microstructural). It is within this framework that I resolve the problems presented by Slater and Tobin. Though the disconnect between structure and function is instructive, it would be a mistake to identify biological protein kinds with their functions. There are a number of well-known problems with functional kinds, and function appears to be an accidental property of chemical structures rather than a necessary property of any kinds. This becomes clear when we conceive of biological protein kinds as Darwinian. On the proposed view, chemical kinds are best viewed as pieces or tools that are picked up, shuffled, and recombined and sometimes acquire physiological functions. These pieces are occasionally conserved through evolution. It is through this process that biological kinds emerge. Biological kinds are what-they-are in virtue not of their physical structure, but their evolutionary history. Within that history chemical structures have some influence on outcomes, but biological kinds are ultimately created and shaped by their histories of contingency, chance, and selection.

Separating the proposed duality from the permissive pluralist picture is important because the former is more fruitful. While the permissive pluralist is forced to view different classifications as alternate ways of describing the same world, the theory I offer describes two fundamentally different kinds and their interactions, on the model developed in Chapter 3. This account paves the way for more general discussions about the differences between the kinds of the biological world and those of the physico-chemical world.

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[85] It is odd that Slater (2009) misses the distinction, since he recognized it in a later work (Slater 2013).

Implications for metaphysical monism, on the one hand, and for biochemical practice, on the other, will come into full view in the concluding section. To begin, I will introduce the two intuitions and their conflict, which form the backdrop of my analysis.

1. Conflicting Intuitions About Kinds of Proteins

At base, the conflict between the biological and chemical is a clash of intuitions. Respecting both aspects of proteins comes at the expense of a single consistent classificatory scheme. While practical workarounds can and have been found, these might trouble the natural kind monist. I begin with the pre-theoretic conflict between these biological and chemical intuitions before expounding the precise nature of the relationship between these two sides of the protein world.

The biological intuition has a long history in the sciences that study proteins. This tradition emphasizes the importance of proteins in physiology. Though many of proteins' biological roles are newly discovered, their importance has long been recognized. The term 'protein' was coined in the mid-19th century from the Ancient Greek 'proteios' meaning 'primary' or 'in the lead', in order to emphasize their presumed essential role in microbiological processes. Proteins are the most profuse macromolecule, occurring in all parts of all cells. Though they carry out a wide variety of functions and take on a staggering number of forms, all proteins are created from amino acids linked in linear sequences and then folded into complex shapes, called 'conformations'. There are two varieties of protein. The first are fibrous proteins. These make up every tissue in organic bodies; common textbook examples include keratin and collagen. Second are globular proteins, which carry out important physiological roles as enzymes, antibodies, regulators, and cellular messengers. The importance of proteins in this regard should not be understated. Enzymes are necessary for the catalysation of nearly all organic chemical reactions and, as such, are involved in a wide variety of molecular biological processes—and this is not to downplay the biological importance of messengers, regulators, and antibodies.

Understanding protein function is a key part of understanding molecular biology. Not only do they comprise all organic bodies and play key roles in organic reactions, they have recently become the subject of re-focussed interest for their role in molecular evolution. Adaptations from the development of anatomy to alteration in metabolic processes involve changes at the protein level. As a result, a key tool in uncovering the progeny of extant physiology is the study of the proteins involved. Specifically, biochemists study the semi-autonomous 'domains' from which proteins are compiled. Conserved domains are shuffled, recombined, duplicated, and changed to carry out new functions. Tracking domains allows us to map the evolution of new traits. Understanding their physiology is key to understanding ancestral functions of current protein molecules, which is key to understanding the process of molecular evolution.

It is fair to say that not only are proteins fascinating for their chemical complexity, but also for their biological significance, on multiple fronts. Any description of protein kinds should respect this—so the intuition goes.

The chemical intuition, on the other hand, is an extension of standard thinking about natural kinds in science, which has long been structured by treatments of chemical kinds. Indeed, the gold standard case of a natural kind is a chemical one: gold. Nearly all introductions begin with this elemental example. Even Marc Ereshefsky's (2009) reference article 'Natural Kinds in Biology' introduces kinds not with a biological example, but with the familiar chemical:

> The traditional account of natural kinds asserts that the members of a kind share a common essence. The essence of gold, for example, is its unique atomic structure. That structure occurs in all and only pieces of gold, and it is a property that all pieces of gold must have.

Paul Griffiths (1999) similarly explains:

> My gold watch resembles your gold navel ring [...] because the atoms of which both are composed share an essence: their atomic number. (p. 209)

The received view of chemical kinds is microstructural essentialism, introduced in Chapter 3. Both Ereshefsky and Griffiths, referencing the atomic structure of gold, align themselves with this tradition. Simple chemical objects make ideal examples because they are neatly divisible and eminently familiar. More importantly, microstructure is a unique determinate of identity since any instance of a chemical kind cannot lose its microstructure without changing kinds. Microstructural essentialisms also hide the distinction between induction-supporting kinds and natural kinds, because they admit a clear physical reduction of their macro-level properties to some physical microstructure.[86]

Though elements provide the simplest cases, we can extend theories of chemical kinds to more complex molecules. Robin Hendry (2006) has recently shown how microstructuralist accounts can be scaled up from elements to molecules, arguing that just as the essence of Gold is represented by atomic number 79, so too might we identify the essence of carbon dioxide with its constituent atomic elements, represented by the formula CO_2. Again, this is a kind in both senses, since the microstructure is causally efficacious, explains the relevant properties of carbon dioxide, and is necessary and sufficient for being carbon dioxide.

Microstructure becomes less clear as we move up the complexity scale. The microstructure of CO_2 might be its constituent atoms, the atoms and their bonds, or the atoms, their bonds, and their spatial relations. Chemists and biochemists describe a range of 'levels' of structural

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[86] This point was elaborated in Chapter 3, section 3.

arrangements. The problem becomes considerably more complex when we get to proteins. Protein molecules are described at the level of primary structure, which includes just the linear sequence of amino acids; secondary structure, which describes stable recurring geometrical patterns in localised sections of the molecule; and tertiary and quaternary structures, which describe the geometric and bond structures of the whole molecule or the molecule plus bound partners, respectively. Forwarding a metaphysical thesis, the microstructuralist would do well not to attach to any one of these representations. For these are just that, representations, fallible attempts to capture the physical state of the protein. The physical facts that get included in a given representation are a function of goals and interests, but also of the context, since certain physical features are stable only in specific environments. Given present concerns, what is interesting about microstructuralism is the grounding of kinds in microphysical facts. How and whether we know or represent those facts is a separate matter. Rather than take a stance on which representation of microstructure is best, I will use the general phrase 'chemical structure'.

There are several reasons why philosophers might expect and want a theory of chemical kinds to extend upward to proteins. First, from a purely physico-chemical point of view, proteins are simply very large chemical molecules; they are macromolecules. They can be annotated and described in much the same way as smaller molecules, but on a much larger scale.[87] If microstructuralism can handle one step up the complexity scale, from elements to molecules, then what's a few more? Second, some may find it suggestive that microstructuralist individuation is a dominant method in protein classification today. When biochemists investigate proteins, they work largely at the levels of conformation and primary structure. This is the main way in which proteins are annotated and referenced. But the final and most philosophically forceful motivation behind extending the standard account of chemical kinds comes from the prospect of monism. As Slater explains,

> Following the lead of natural kinds essentialists of old, one might suggest individuating proteins (and other biological macromolecules) on the basis of their chemical structure. At first glance, this stance affords a tempting monism about biochemical taxonomy. (2009, p.852)

Though he goes on to reject this possibility, the prima facie appeal of monism is worth understanding. The issue evinces a longstanding concern with the homogeneity or heterogeneity of the world investigated by the sciences. There is great appeal in the thought that molecules, atoms, and macromolecules are all fundamentally the same types of thing. A monist might envision a single (enormous) hypothetical taxonomy representing the varieties

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[87] My point is not that proteins *are* described in the exact same way as smaller molecules, but that they can be. The chemical formula of a protein is far too long and cumbersome to be of any use in talking about proteins and so is not used. A higher level of description, focusing on component motifs and domains, is much more practicable.

of chemical types, from hydrogen to ununoctium, and on to molecules and proteins. Microstructuralism proffers one set of things with one type of kind membership conditions.

There are two senses of 'monism' and both are bound up with the appeal of microstructuralism. First, microstructuralism offers monism in the fashion after which molecules are naturally individuated. This is Category Monism. Scaled up to large molecules or down to single atoms, the kind category is the same. Microstructuralism holds that all chemical kinds are what-they-are in virtue of microphysical facts. Second, microstructuralism offers the promise of a single taxonomy. This is Taxonomic Monism. Every kind in the microstructuralist taxonomy is unique. There is no worry about one particular belonging to multiple incompatible kinds.[88] Both brands of monism are on the table. Slater is right to be tempted. I will return to the prospect of monism in the final section.

Notice that microstructuralism as a putative account of biochemical kinds also avoids intrinsic heterogeneity. Groups based on microstructure are by definition uniform in at least one important property: their microstructures. The questions are thus whether those microstructures are ever lost and whether these groups are distinctively biological.

Hopes for grounding protein identity in microstructure are not just idle metaphysics. The tradition has a corresponding scientific history. Scientists long presumed that chemical structure determined biological properties. The study of proteins was once dominated by reductionist ideology, which claimed that the function of a protein was determined by its three-dimensional structure, itself determined by the protein's molecular composition. This came to be known as the 'Sequence-Structure-Function' paradigm (SSF), a development of Emil Fischer's (1894) influential 'lock and key' model of enzyme function. This became a central principle for all proteins with the rise of physical chemistry in the early 20th century. Physical chemistry was reductionist. It sought to ground all chemical properties in atomic physical reactions.[89] On this perspective the key to binding is molecular shape. Binding requires that particular atoms on binding molecules be brought into close physical proximity; only then can the atoms form the weak covalent bonds that hold adjoining molecules together. The more atoms in the molecule, the more complex a shape must be assumed to

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[88] The point here is that no single object will be two incompatible kinds. An atom cannot be both hydrogen and oxygen—nor can it be both hydrogen and water, even though a water molecule might be composed partly of hydrogen.

[89] Fibrous proteins do what they do not through interactions, but though the joining of many proteins of the same type—often in a sheet or coil. The physical properties of the macro-structures fibrous proteins create (like hair or skin) are a function of micro-structural features of the proteins. William Astbury of Leeds, for instance, found that the elasticity of many fibrous materials, from hair to muscle tissue, was a function of their molecular composition (Hall 2011).

bring everything into alignment. This is how keys open locks.[90] Since denatured proteins can recover their shape, it was believed that the shape of a molecule was a perfect function of its amino acid sequence.[91] Thus sequence determines structure and structure determines function.

When SSF stood strong there was no problem of protein kinds, since, according to SSF, there was a straightforward link between molecular composition and physiological function. Whether individuated structurally or functionally, the result should have been the same. Unfortunately these canonical beliefs have proved false. Though various phenomena have caused doubts about SSF, multifunctional (or 'moonlighting') proteins are taken by many to be the nail in the coffin—so much so that one researcher recently declared, 'Moonlighting is mainstream: Paradigm adjustment required!' (Copley 2012).

Multifunctional proteins are a heterogeneous class. As the name suggests, these are cases where 'one' protein performs multiple functions. The relations that make these count as 'one' protein vary, but in general proteins are considered the same when they share an amino acid sequence. There are several molecular mechanisms that permit multifunctionality. In some cases, proteins with identical sequence adopt different folds in different contexts in order to carry out different functions. Differential folding serves to utilize different functional domains, associated with different tasks. In other cases, conformationally-identical proteins carry out different but related roles in the same physiological process (Copley 2012). Regardless of the particulars, proteins that share some chemical structure but differ in physiology have left scientists without a theory of protein kinds. It is unclear whether a protein is what-it-is in virtue of chemical structure, biological function, or something else entirely. The result, among other things, is a serious disjoint in competing classificatory techniques (see report from Carr et al. 2004) leading to poor understanding of when two proteins are or are not the same.

Though protein taxonomists still tend to classify microstructurally, microstructuralism alone does not undergird taxonomic practice. Using only microstructural classification results in unhelpful and counterintuitive classifications, such as cases where proteins that appear wildly different at the biological level are grouped together at the chemical level, and proteins that fulfil the same biological role yet are grouped apart due to chemical dissimilarity. As a result, microstructural classification is supplemented with biological considerations, when appropriate, to correct these irregularities. Thus Goodwin's concession: though he

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[90] The complementary relation between protein and binding partner is called 'specificity'. For a history of the idea of 'specificity', see Judson (1980).

[91] A particularly significant finding came from Mirsky and Pauling (1936), who found that a denatured protein lost structure and function, but regained structure and function when renatured.

wishes biochemical classification to be based on physico-chemical structure, biological facts must be accommodated, often ad hoc, by augmenting or supplementing structural classifications. He explains,

> While there is a fundamental, structural way of individuating proteins, there are also supplemental classifications introduced to address various biological interests. (2011, p.537)

Structural information about proteins may come close to a biologically meaningful classification, but it must be adjusted to highlight pertinent biological similarities or differences.

The point can be made salient with the example mentioned at the beginning: the multifunctional protein family known as 'crystallins'. Crystallins are the transparent structural proteins found in the lens and cornea of the eye. There are many varieties of crystallin but nearly all demonstrate some multifunctionality. In chickens and ducks αβ-crystallin forms the refractive surface on the lens of the eye, yet also occurs as a heat-shock protein and an enzyme, called a 'lyase'. This is mirrored in many other animals. In birds and crocodilians the crystallin that forms the lens also doubles as the digestive enzyme lactate dehydrogenase. The α-crystallin present in all vertebrate lenses also functions as a molecular chaperone and may have an enzymatic role in digestive processes (Copley 2012). Standard physico-chemical classification leads us to say that we have one protein, but intuitions about biological function lead us to conclude otherwise. These intuitions come out when Slater (2009) insists that protein kind classifications preserve 'important biochemical facts' about the molecules, something that structural definitions fail to do. Though the chemical facts are presumably explained, many functional (often physiological) ones are not. The desire for category monism is at odds with the desire to respect the biology.

Scientists can describe structural proteins with multiple biological roles, or biological proteins with multiple structures. But no single scheme will perfectly categorize both. Hybrid schemes are needed. Different taxonomies and different types of kind category are necessary under specific disciplinary circumstances. Though some communicative problems may result (Carr et al. 2004), these are presumably resolvable with more specific language or better databases for classification. Biochemists face no in-principle problem, having developed a rich epistemic system of interwoven classificatory practices, which change with contexts and aims. The situation is only problematic if we hold the belief that there is a 'correct' or 'natural' way to classify proteins—to carve nature at its joints—and that biochemistry should aim at this ideal, but misses for all its pragmatism.

In the final section, I will argue that scientific practice need not utilize a taxonomy of natural kinds. Yet without being normative about scientific practice, we can still ask what pragmatic pluralism in practice means for the metaphysical question. It is still possible to ask what structure of kinds is compatible with known phenomena and would support the pragmatic pluralism that characterizes taxonomic practice.

Pragmatic pluralists see multiple kind classifications as alternate and equally legitimate descriptions of the same entities. If there is no sense in which one such scheme is fundamental, or privileged, then there exist no grounds on which to say, 'this classification describes how the kinds really are'. From here, there are still a number of answers to the metaphysical question. One option is nominalism. Perhaps pragmatic pluralism reveals the poverty and scholasticism of natural kinds talk altogether (Hacking 2007). Not only is there no single way that the kinds are, perhaps there is no way at all! Another option is highly permissive category and taxonomic pluralism. Perhaps each type of kind category forms a different taxonomy of natural kinds, and we simply pick and choose from different natural kind structures as situations dictate. On this somewhat deflationary view, there are as many natural kinds as there are natural properties from which to classify. A third option is to accept a more conservative pluralism. Perhaps there are not innumerably many kinds, just a select few. Relying on the account of biological kinds developed in Chapter 3, I will argue for the third option: a restricted category dualism.

2. Against Permissive Pluralism

Setting aside for the moment the nominalist option, the two realist pluralisms have similar appeal. Both concede the force of the pre-theoretic observation that one classification will not do. The choice between the two is a matter of just how many types of classification might lay claim to naturalness or primacy. Many treatments of proteins emphasize the physical underdetermination of final conformation. These accounts draw attention to the number of distinct 'levels' of structural arrangement of molecules, suggesting that each might be a unique physico-chemical kind. Such descriptions strongly legislate for permissive pluralism. This is a red herring.

Imagine a protein family where a single sequence of amino acids results in a number of distinct final conformations under different conditions. Many such cases exist. Classification according to primary structure would yield one scheme of kinds; classification according to final conformation would yield another. These cases are frustrating to practicing taxonomists and appear to have implications for the philosophical discussion, as well. As Tobin claims,

> If two chemical kinds can have the same [microstructural] essence, yet are considered distinct at the macrostructural level, then the … microstructure would seem insufficient for macromolecular classification. (2010, p.53)

On the surface, physical underdetermination appears equally problematic as the disconnect between biological and chemical classification. The claim is that these cases recommend a collection of different physico-chemical kinds, appealing to many different levels of structural organization.[92]

Yet microstructuralism is well equipped to handle these cases. We can maintain that conformation is selected extrinsically from within a space of possibilities imposed by the intrinsic microstructure. Some comparatively simpler cases from chemistry will help clarify. Many molecules share a chemical composition but exist in different states at higher levels of structure. One such group are conformational isomers, called 'conformers', where one set of component atoms, with just one arrangement of bonds, can exist in multiple conformations. This happens in relatively simple molecules and is also a common feature of proteins. Conformational isomerism is a property of bond structures that permit movement, usually around single bonds, enabling multiple geometries. The particular geometry that obtains is a function of external forces; temperature is perhaps most commonly discussed. Certain possible conformations are stable under common conditions and these are the conformations that are recognized in practice. Given free reign over extreme temperatures, electrostatic forces, and other conditions scientists can bring about additional marginally-stable forms. The familiar n-butane (C_4H_{10}) is commonly recognized to have two conformers (trans and gauche), but these are just the most common and stable in our world. At least two more isomers are possible yet difficult to isolate and stabilize in the lab and even more intermediate forms might be possible across a wider range of conditions. But however many possibilities exist, they are finite, constrained by the bond structures that must realize them together with the laws of physics. Possible conformations are constrained by the microstructure. For this reason, conformers pose no threat to microstructuralism. Extrinsic determination of geometry should be viewed as the selection of one possible geometric state from an internally-constrained space of possibilities.

A second type of isomer might be thought more problematic, and indeed more similar to the troublesome protein cases. These are structural isomers: cases where the same component atoms, represented by the same chemical formula, can be arranged in unique bond structures, yielding unique geometric shapes. Chemists often regard structural isomers as being of different kinds. Consider again n-butane. In addition to its two conformers, n-butane also

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[92] The motivation for this perceived problem is thorny. I see two options: (1) Disconnect between lower-level structure and higher-level structure violates some *a priori* principal concerning the link between essences and the properties of a kind; or (2) Adopting different higher-level conformations constitutes a loss of the lower-level shared microstructure, thus introducing a new kind. Tobin and Slater seem to be pressing (1), which is of course a non-starter in view of Chapters 1 & 2. I will therefore charitably critique (2), but notice that the argument I present would work against 1, as well.

has a structural isomer, methylpropane. Both have four carbons and ten hydrogens, but n-butane is a linear structure and methylpropane is a branching structure. Like the conformers, the space of possibilities for structural isomers of a molecule are limited by the available arrangements of constituent atoms together with the context and laws of physics. Colloquially, the conformers and structural isomers are all called 'butane', but the International Union of Pure and Applied Chemistry (IUPAC) separates the structural isomers into two types. This is in contrast with its treatment of the two conformers, which are viewed as two instantiations of the same type, n-butane.

It is important to consider the IUPAC standard. The rules for dividing and grouping isomers are complicated, often tied up with concerns about nomenclature, but the relevant concern here is practical: n-butane and methylpropane behave differently, are independently stable in experimental contexts, and are used separately. Contrast this with the conformers of n-butane, which rapidly flip back and forth between conformations and exhibit relatively similar properties. In practical applications chemists simply do not work with pure solutions of a single n-butane conformer; nor would they need to, given the negligible difference in behaviour. Different IUPAC stances on conformational versus structural isomers reflect practical demands of science, not fundamental metaphysical differences. These classificatory norms have an important place in the practice of chemistry and are accordingly relativised to the contexts of the human pursuit of chemistry. Radically different contexts, new uses, or more stringent acceptability standards for difference could lead to different decisions.[93]

While IUPAC's practices do count against microstructuralism about classificatory practices (as a descriptive or normative claim), they should not count against microstructuralism as a metaphysical thesis. Both conformational and structural isomers exhibit the same type of relation between higher and lower levels of organisation. Though structural isomers admit a greater space of possible geometries, the relation is still one of internal constraint and contextual determination. The atoms in butane are subject to the electrostatic properties of the collective component atoms and within that space of possibilities the physical environment (understood as a number of various forces over time) determines which possible arrangement the molecule can actually take. The molecular essence provides a disposition to act this way or that, depending on relevant context.

There is no reason why this strategy cannot scale up to proteins. We can treat different conformations adopted by multifunctional proteins as a function of extrinsic factors, constrained by the possibility space imposed by the physical microstructure of the macromolecule.

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[93] Notice for instance that the stance on n-butane is relativized to human timescales.

If I may now anticipate an objection, the reader might justifiably wonder why we cannot extend this strategy to explain the underdetermination of physiological function. If conformation is constrained intrinsically and determined by environment, why not say the same thing about physiology? We might think, for instance, that just as a structure contains the potential for many conformations so too any given structure contains the potential for innumerable physiological roles. The roles that get selected are a function of extrinsic determining factors. Indeterminate intrinsic physical microstructure at the lower level, plus context, equals determinate outcome at the higher level. On the face of it this seems very much like the problem of isomerisation with a larger space of possibilities and a lower likelihood of actualization. But there is an important difference. In the case of isomerisation, the phenomena at the higher level are not multiply-realised at the lower level. Any molecule that is n-butane or methylpropane is necessarily C_4H_{10}. Nothing could be one of these structural isomers yet originate from a different underlying microstructure. The same cannot be said of physiological roles. Phenomena at the biological level are multiply realisable at the structural level. The lens crystallin role may be filled by the αβ variant, but so too could it have been filled by a number of other crystallins. This possibility is clear from the large numbers of species utilizing different crystallins in their lenses. Many molecules are suited to this biological role. Molecular structures are surprisingly functionally flexible. The molecule that gets the job is the one that happened to have been evolutionarily conserved, which is a matter of great chance and contingency.

A stronger (and more loaded) way to say this is that in all possible worlds the chemical structure we recognize as n-butane is realized by C_4H_{10}, but there are many possible worlds in which the biological role 'lens crystallin' is realized by structures other than the αβ-crystallin protein. While there are commonalities between the underdetermination of conformation and the underdetermination of physiology, the difference lies in the existence of multiple-realizability in the opposite direction.

The conflict is much deeper than an observed incongruence between microstructural and biological classification, since this weak observation would also lead us to claim that microstructure cannot account for final conformations, which also appear quite different from the microstructures that bear them. Much more strongly, the claim is that microstructure lacks the bidirectional relations with physiology that are in place between microstructure and conformation.

Without the problem of physical underdetermination, there is no push left toward the highly-permissive pluralist interpretation. Classification need only accommodate the microstructuralist, on the one hand, and the biologist, on the other. But further elimination is not possible. The multiple-realization problem shows that it would be folly to attempt to privilege one of these considerations over the other. It would be no more than a trading of intuitions over the relative significance of biology versus chemistry. Any such decision would be metaphysically arbitrary. The phenomena are best respected by a dual theory, comprised of biological kinds and chemical kinds. Though chemical kinds are well described by microstructuralism, biological kinds are more difficult. Philosophers and scientists have tended to think of protein biology through the lens of function. Though tempting, functional kinds will not suffice. Not only are there well-rehearsed difficulties with functional individuation (Slater (2009) covers some of these with regard to proteins), the functional flexibility of molecular structures, the multiple realization of biological functions, and the evolutionary contingency of function suggests that function is an accidental property of molecules, not an essential property of any kinds. Instead, I suggest conceiving of biological kinds as historical kinds. I will now unpack this account while defending it against the nominalist option.

3. Against Nominalism, Toward a Duality of Kinds

With the structural underdetermination problem dispensed with, two answers to the metaphysical question remain: nominalism and restricted pluralism. The most plausible case for nominalism about protein kinds derives from the observation that microstructuralist classification can neither explain nor capture certain characteristic properties of proteins. Granted, chemical structure can explain certain of the physical properties of proteins, but it cannot explain everything about the biological functions. It cannot tell us, for instance, why a certain biochemical performs the specific multiple functions that it does. For a nominalist, this limitation forces scepticism about proteins as microstructural kinds and probably about proteins as kinds altogether. Both of these conclusions are misguided.

To introduce my position, consider the following extended analogy:

> Take a solid gold necklace, a solid gold ring, and a solid gold electronic connector pin. It is perfectly acceptable to tell a story about atomic structure according to which the gold of the jewellery and the gold of the electronic pins are all members of the same kind; we might take this to be a story about why all of this gold is indeed the same kind of thing. This story would be one about natural kinds.
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> Now suppose you were asked about the other kinds in this scenario: viz. gold jewellery and gold electrical components. Your account might include some facts about the gold from which they are created, including facts that account for its ductility, conductivity, malleability, and colour, which explain why gold makes useful electrical pins and attractive jewellery. Yet these facts would not tell us why humans chose to make jewellery and electrical pins, nor would they tell us why we chose to make these things from gold rather than palladium, silver, cadmium, or platinum. These facts would not tell us why these very different kinds of things happen to have been made from the same material, nor would these facts tell us

> much about the uses to which jewellery and electrical pins are put. In short, the physico-chemical facts about gold are helpful, but they do not tell the full story. A better account would include any number of historical, economic, and anthropological facts. These facts are about humans, not nature, and for this reason we do not call kinds like jewellery and electrical pins natural kinds. If they are kinds then humans, not nature, make them so. Yet the fact that the microstructure of gold cannot account for the existence or form of the kinds 'jewellery' and 'electrical pins' would not prevent us from saying that gold was a natural kind.
>
> Consider now a more difficult case: αβ-crystallin. Recall that in addition to serving as the lens in ducks and chickens, αβ-crystallin also occurs as a lyase enzyme. If we wanted to tell a story about how duck-lens protein, chicken-lens protein, and lyase protein were all similar, we would appeal to their physico-chemical microstructure, much as we did with gold. A shared microstructure is why all instances of αβ-crystallin are generally taken to be instances of the same kind. We might take this to be a story about natural kinds.
>
> Now suppose you were asked about the other kinds in this scenario: the kinds 'duck-lens protein', 'chicken-lens protein', and 'lyase enzyme'. You could appeal to some facts about the αβ-crystallin molecule, explaining why it happens to be well suited to refracting light and binding various substrates. These facts tell us why αβ-crystallin makes effective eyes and also why αβ-crystallin makes useful enzymes. Yet, just as we saw with jewellery and electrical pins, these physico-chemical facts about αβ-crystallin will not give us the full story about these kinds. They do not, for instance, tell us why αβ-crystallin is used to make duck-lenses, rather than ε-crystallin, τ-crystallin, or α-crystallin. Though helpful, the physico-chemical facts about αβ-crystallin do not tell us everything about the various uses to which it is put.

I take it that no one will believe my tale to have proved that gold is not a natural kind. Rather, the point is that we would not take our inability to account for the existence of the kinds 'gold connector pin' and 'gold jewellery' as evidence against gold's status as a natural kind. By parity of reasoning, I suggest, we should view biological facts about chemical kinds to be non-problematically beyond the pale of microstructural kinds.[94] We should not take the incongruence of chemical and biological classification to count against the natural kind status of the chemical kind αβ-crystallin. The correct move is to retain the microstructural chemical kind and search for a second set of kinds. In the gold case the second set of kinds were artefacts, in the protein case the second set of kinds are biological kinds.

The kinds 'gold jewellery' and 'gold electrical pin' are not natural kinds. They are human kinds. How about lenses and lyases? We should view them as evolutionary or historical kinds. Are they *natural* kinds? Like others, my argument for their naturalness will be left implicit, an appeal to the naturalness of natural selection.

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[94] Morange (2012) provides an excellent discussion of the limitations of both chemical and biological explanation. He claims that biological—specifically evolutionary—explanations provide a sort of historical explanation that fills in the sorts of details left out of a chemical explanation, such as why a molecule performs this or that function.

4. Biological Kinds, Chemical Kinds, and Their Relations

The microstructuralist versus historical kinds debate has traditionally been rehearsed in the context of biological species (Griffiths 1999). According to proponents of historical biological kinds, the only essential properties for species are their unique histories. It is these histories of selection and chance that have made them what they are. Compared to species, biochemical molecules are a more instructive case, owing to their comparative simplicity. Unlike species, the case of biomolecules makes it easy to see how microstructural and historical kinds relate. This can be seen already from the gold analogy. Gold is a chemical kind that can be picked up and used by humans in the service of creating new kinds of things. These take on a life of their own, independent of the materials from which they were originally created. We view the chemical kind 'gold' as a tool or part. Human kinds like jewellery and electrical pins are created with or from these tools, by design or by happenstance, and are subsequently maintained or changed by innumerable forces, both intentional and accidental, using new and different chemical kind tools. If we wanted to ground the stability of these human kinds in spite of structural and functional changes, we would need to take recourse to their trajectory through human history. It is this unique history that has shaped the contemporary kinds.

We can think of proteins the same way. Swap gold for chemical macromolecules, rings for enzymes, and humans for evolution and you have an account of biochemical kinds. There are chemical kinds that get picked up, used, and changed, by selection and drift in the service of biological kinds. Different chemical kinds come in and out of this process as genes mutate. Different functions emerge and disappear as contexts change and selection pressures emerge. Through all of this change the closest thing to a constant is the biological protein's historical trajectory. Current chemical microstructures and current physiological functions are simply the latest stage in an on-going history.

Conceiving of proteins *qua* biological kinds as essentially historical entities helps avoid the intrinsic heterogeneity problem faced by the microstructuralist account. Over time a set of genes coding for a protein will mutate, leading to change in protein structure. Many of these will have no impact on the protein's physiological function yet are stabilized over time. How are we to conceive of these? Should we say that the protein has changed kinds? Other mutations may be more severe, inducing physiological changes slight enough to register as 'change', but not enough to remove the protein from the proteome altogether. In case of functional alteration, should we consider it to be a new protein? Biological species pose these same two problems. They exhibit change in genetic and morphological structures and also gain, lose, and alter behaviours during and between generations. These problems strain microstructural accounts.

In the case of species, the reply to both of these worries is to reject the microstructuralist account. The same is true of proteins. The microstructure of a biological individual does not make it what it is. The microstructure is just one part of the biological individual. The parts, like the whole, change. We can think of the chemical kinds from which proteins are compiled as sets of often interchangeable parts, with varying effects on functionality. We can likewise think of the function of a biological protein kind as just another property, subject to sporadic change and change in response to force. Neither of these are 'essential' properties of the biological kind.

So when we ask: Are proteins that share a function but differ in structure the same natural kind? How about proteins that share a structure but differ in function? The answer will be: It depends on the historical details.

5. Implications for Biological Individuals

Attention to evolution recommends one additional change: a refocusing of particulars away from whole molecules and toward evolutionarily-conserved units. The common sense focus for a theory of biological protein kinds is the protein molecule itself. Intuitively this seems rather simple: why wouldn't you focus on the spatially delimited molecule? This sort of physical delimitation is often a good strategy when it comes to chemical kinds. But a *prima facie* problem should give pause: Biomolecules are often ever-changing composites, made up of smaller proteins and amino-acid residues. Though these parts converge onto one chemical molecule, they will almost certainly be of different evolutionary origins. It is unclear where and when one molecule ends and another begins, and it seems that solving this problem by appealing to the entire composite as a single molecule runs afoul of the historical kind theorist's appeal to evolutionary history.

To solve the mereological quandary we might borrow a trick from certain discussions of biological organisms and draw physical limits according to whatever composition is required to achieve physiological integration. Yet this appears to inherit the general problem of finding a mind-independent sense of 'function'. If physical composition is judged against functional integration, then there must be a privileged sense of physiological function. But philosophers have long-strained to find any such notion. Slater (2009) has already shown how difficult it is to find the function for a given protein molecule. Which of many actions and interactions we take to be the function is a matter of explanatory context. An appeal to physiological integration will not work.

Historical kinds are whatever individuals have been conserved over time, or have resulted from the historical processes of which they are the current terminus. This approach would thus suggest focus on conserved domains, rather than spatially-delimited molecules.[95]

Taking conserved domains as the individuals is not without precedent in practice. Biochemists do not explicitly conceive of conserved domains as the individuals, but it is nevertheless conserved domains, not whole molecules, that serve as the focus of work on molecular evolution. Biochemists recognize that one spatially delimited molecule may contain amino acid strings of unique evolutionary origins. Thinking of conserved domains as potentially distinct from the whole molecule is necessary in order to explore the evolutionary history of protein molecules and establish cladistic relationships.

This approach need not face the mereological problems imposed by the need for static physical constraints, or the function problem imposed by the need for physiological integration. This approach is not challenged by the existence of molecules that contain conserved domains of different historical origins, since it regards these as separate individuals and so they are able to be members of different kinds. This allows us to say that, in many cases, different biological kinds converge into a single chemical molecule with a single set of physical limits, to participate in the same or different physiological performances. A full discussion of these mereological issues continues in the next chapter, when I examine the problem of biological individuality in greater depth.

6. Implications for Monism and Scientific Practice

The treatment I offer requires conceiving of proteins in a very different way. A single chemical molecule may contain multiple biological individuals. Moreover, the same biological kinds will often exist on different chemical kinds. This shift in focus leaves the door open to a form of monism about taxonomies. Proteins consist of two different types of objects, with similar extensions. There is no cross-classifying the same object. In the chemical case, we have whole molecules characterized by physical microstructure. In the biological case, we have conserved domains characterized by evolutionary relations. Though we must admit a duality of kind categories, there is a strong sense in which they do not categorize the same objects. But taxonomic monism is just a door prize. The more important implication is that this avoids a potentially unsavoury consequence of permissive pluralism. On that view, one will have to concede that there are many different but equally natural ways

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[95] It is common to call these sequences 'domains', but since that term is also used to refer to units of function it is perhaps better to call them 'conserved domains', reserving 'functional domains' for the other use.

to categorize the same protein molecules. Instead, we have an account of two different types of kinds and their relations.

What is lacking in this account is a single taxonomy of biochemical kinds. It is not that we have multiple taxonomies of biochemicals, of course, it is rather that biochemical kinds appear not be kinds at all. Biochemicals are at the nexus of two kinds (of kinds); but this is no skin off the nose of the monist.

Though this position is overtly category dualist, notice that the chemical side of the protein case appears consistent with other chemical kinds. One type of chemical kind—that described by microstructural essentialism—seems perfectly equipped to describe atoms and molecules of all shapes and sizes. The protein case offers no reason to suspect that there are limitations to the scope of microstructuralism within the world of chemical molecules. Insofar as this is the monism behind the chemical intuition, the desire appears stated.

Some philosophers of science might be troubled that my theory of protein kinds diverges radically from actual scientific practice and that my theory cannot take the place of current taxonomies. While we should allow scientific knowledge to guide investigation into kinds, it is certainly not the case that scientific practices should straightforwardly dictate metaphysical conclusions. Nor is it the case that the conclusions I offer should be taken to recommend the revision of scientific practice.

Epistemic barriers constrain classificatory practices. These are a function of the means of acquisition of human knowledge and so should not constrain classificatory metaphysics. To take a simple example, we do not know the evolutionary histories of most proteins; this would preclude my biological classification. But a more subtle point is also worth considering. In order to begin an investigation of evolutionary origins, proteins must first be carved up into operational types. Those types should be carved according to their evolutionary relationships, but that would be putting the cart before the horse. In order to investigate the evolutionary history of a protein type, we need to have marked off that type to facilitate investigation. The best option is to use structure. One might try to classify in a way that approximates physiological similarity or phylogenetic relationships, but even this would be grounded in the relevant structural similarities. It is for precisely these reasons that biochemists use structure as a primary investigative tool in the understanding of physiological function. Structure provides the only currently accessible epistemic handle for thinking about proteins. The tools and techniques of biochemistry are accordingly built around structure. This is how I interpret Goodwin's finding that biochemical classification is 'fundamentally' grounded in structure. He writes, 'one of the enduring goals of biochemistry has been to explain the function of proteins in terms of their structure' (2011, p.534). It would be wrong to read this as commitment to reductionist metaphysics. This is simply a response to epistemic barriers.

Supposing we could perfectly refine a biological taxonomy, perhaps based on god's eye view of evolutionary history, it is still not clear that this would provide the sort of taxonomy that scientists need. Natural kind taxonomies are insensitive to the contexts of investigation, whereas actual taxonomies need to be pragmatically tailored. While metaphysicians want their results to hold over all possible worlds, across all possible conditions, real-life scientists tend to work in just one actual world, and even then in a fairly circumscribed range of actual conditions. It is perfectly acceptable if they fine-tune their taxonomy to this world and those conditions. Yet when we set practice to one side we can see that, when it comes to biochemicals, nature has two sets of joints.

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Biological Individuality

In day-to-day conversation I have no trouble picking out individual things. I separate my chair from the desk at which it sits, I differentiate my clothing from my body, and I have no trouble separating my friend Sarah from her twin sister Dani.[96] Most of this work is done effortlessly by language that reflects spatial boundaries, such that I need not put much thought into a lay theory of individuality. But the biological world throws up a number of challenges to the lay theory.

The human gut is home to catalogues of microbes that aid with digestion and other metabolic processes. The microbes and I are functionally integrated. Are these symbionts part of me? I have no intuition one way or another. The dandelions in my garden reproduce by cloning themselves, making parent and offspring genetically identical. Selection sees these as one individual, but to my eyes they appear as many. Which is it? Is there a correct answer to be found? A theory of biological individuality is needed.

A concept of the individual has several roles to play. Concerning biological individuals, certain of these roles are practical or epistemic. In demographic surveys we need to know which things to count. In modelling evolution we need to differentiate between things increasing in size and groups of things are increasing in number. Individuality as it bears on these practical issues has received a lot of attention from philosophers of biology, particularly

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[96] Dani is also my friend, even though this sentence is ambiguous on that point.

in the last few years (Clarke 2010, 2012, 2013, Clarke & Okasha 2013, Ereshefsky & Pedroso 2013, Godfrey-Smith 2013).

Epistemic work aside, individuality also performs some metaphysical and ontological heavy lifting. Individuality differentiates between collections with many members and individuals with many parts; individuals and collections relate to their properties in different ways; parts and members bear relations to one another in different ways; and the status individual versus collection is thought to have implications for causal powers.[97] A collection of philosophers also think that ontological facts about individuality bear on social and ethical issues, such as those surrounding abortion (Lee et al. 2014, Smith & Brogaard 2003).[98] The metaphysical side of biological individuality asks whether certain collections of biological matter form one thing or multiple things

These metaphysical questions of individuality are questions about how to negotiate part/whole relationships. Though biology furnishes us with catalogues of challenging cases, the general problem is not unique to our field. In analytic metaphysics and ontology the problem even has its own name, rarely uttered by philosophers of biology: The Special Composition Question (SCQ).[99] SCQ asks: under what conditions do objects combine to constitute other objects? Answering SCQ would tell us not only what biological things form wholes, but also what it is about those things that marks them off as ontologically different from mere collections of parts.[100]

Unfortunately, the literature of SCQ is far from conclusive. Few proposed answers have gained traction; none come anywhere near consensus. A philosopher of biology cannot simply look to those mereological theories, find the correct account, and then start carving up the biome. Nevertheless, looking at SCQ is the best way to regiment the metaphysical discussion of biological individuals. At the very least, this perspective makes plain exactly what is at issue, metaphysical and ontologically, and what sorts of account may be applicable. I describe three forms that answers to SCQ may take: universal, moderate, and nihilistic compositionalism. It is clear that philosophers and biologists seek a 'moderate' form. According to the moderate compositional intuition, we must separate the 'real' composite

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[97] On the second and third points see the concise overview in (Varzi 2007). The final point is discussed below.

[98] These accounts focuss on the part/whole relationship of mother, foetus, and associated biomass.

[99] This comes from Peter van Inwagen (1990).

[100] van Inwagen actually distinguishes three questions: The General Composition Question (GCQ), the Special Composition Question (SCQ), and the Inverse Special Composition Question (ISCQ). GCQ asks about the relationship between wholes and parts, SCQ is specifically about the relationship amongst parts that compose a whole, ISCQ asks about the property instantiated by an object in case it is composed of parts. The discussion in this chapter deals mostly with SCQ, but will overlap occasionally with GCQ. I ignore the difference for present purposes, but see Hawley (2006) for a detailed treatment of the varying forms that answers to these questions must take.

entities, like organisms and proteins (perhaps), from the apparently composite entities, like flocks of birds or transient molecular compounds. The intuition is strongly shared among philosophers of biology who want to secure some realism about the higher-level objects to which they appeal.

A second shift will further these goals. Rather than a shift in perspective, it is a shift in examples, away from the standard examples—organisms—and toward other biological composites. Here I discuss biomolecules. Biomolecules demand an account of individuality for all the same reasons as organisms. Through this lens, it quickly becomes clear that many accounts of biological individual will not provide general answers to biological composition, but are rather answers to the question: 'what is a life?', or 'what composites form singular lives?', focussing as they do on physiology, reproduction, or other apparent features of living things.

This chapter critically evaluates two theories with the potential to satisfy the moderate compositional intuition, both of which tie individuality to features of the world rather than properties of life. In each case it is unclear whether the account is meant as an ontological or conceptual clarification. The charitable interpretation is conceptual, and so I view my project as evaluating the possibility of extending these accounts to provide a rich metaphysics of biological individuality. The first theory grounds individuation in causation (Bapteste & Dupré 2013, Dupré 2007, Wilson 2000). According to this account, individuals are the relata of causal relations. I will argue against adopting this view as a metaphysical account. Features of the world can be lumped together operationally for the purpose of causal claims, but the accuracy of these claims says nothing about the compositional status of the objects that feature in them. Our causal theories of the world can be accurate even if our ontology of objects is not. The second theory emerged in a landmark paper by Ellen Clarke (2013). For Clarke, individuality is tied to mechanisms that determine the level(s) at which selection is happening. I will argue that this account is compatible with Kind Historicism. Since Clarke's account is aimed at modelling future evolution, it must be modified slightly in order to include the genesis of past and current biological individuals. Once this is done, what remains is a theory according to which individuals are whatever composites have participated in Darwinian processes.

Once modified (or 'ontologized'), Clarke's theory becomes Kind Historicism. Both claim that individuals are composites participating in and resulting from Darwinian processes. We therefore do not need a separate theory of biological individuality. It is the kinds, I will claim, that help us reward the real collections with a special ontological status. This move is extra appealing when we remember that many putative individuals in biology can fail to participate in evolutionary processes (e.g. sterile organisms). If we attach individuality to kind membership, not particulars, then these putative individuals are not a problem. They are still individuals in virtue of being members of kinds that are the result of long-run Darwinian processes. Kinds therefore perform the ontological heavy lifting sought after by the moderate compositionalist intuition. A theory of individuality seems superfluous. Which clumps of matter have some ontological status over and above the other clumps? The natural kinds!

This chapter marks the beginning of a shift in focus, away from ontology and toward scientific reasoning, in pursuit of **Q3**. The shift reaches its climax in the conclusion. Here and in the next chapter, I speak somewhat tongue-in-cheek about quests to award 'badges of ontological merit'. What I am criticising with this phrase is not the idea that there are metaphysical or ontological facts pertaining to biology. To the contrary, I think that Kind Historicism helps illuminate these facts. Rather what I am criticising is the tacit assumption that those ontological and metaphysical facts should dictate scientific categories. My criticism of this assumption comes out largely in the next chapter. First, in this chapter, my goal is to simultaneously show how Kind Historicism relates to individuality while also downplaying the import of ontology facts of individuality.

To stress the importance and independence of the pragmatic approach to questions of individuality, I begin by briefly introducing questions of individuality and separating an epistemic side of the debate. I then discuss individuality as an ontological concept before evaluating the causal and Darwinian approaches to individuality. After explaining how Clarke's (2013) Darwinian approach to individuality is ultimately an application of Kind Historicism, I conclude by reflecting on the possibility of using natural kinds to secure physico-chemical individuals, as well.

1. Why Individuality? Which Individuality?

In both biology and in its philosophy a great many scholars have turned their attention to the question of individuality. The problem is an old one. In writing-up the *Beagle* voyage, Darwin mused about the nature of biological individuality, noticing that some organisms seem to be 'incomplete' without the presence of others (19 May 1834; in Darwin, 1913). T.H. Huxley (1852) worried about biological individuality, identifying individuality with microphysical (genetic) similarity. Julian Huxley rebuked his grandfather's position, believing that it could not handle monozygotic twins. The younger Huxley preferred to think of individuals as self-maintaining integrated systems:

> [L]iving matter always tends to group itself into these 'closed, independent systems with harmonious parts.' Though the closure is never complete, the independence never absolute, the harmony never perfect, yet systems and tendency alike have real

> existence. Such systems I personally believe can be identified with the Individuals treated of by the philosopher (1912, p.ix)

And debates about individuality are still in vogue among biologists. Following the comparatively recent discovery of a giant genetically-homogenous fungus in Michigan, the pages of *Nature* were awash with debates about the nature of biological individuality.[101] Upon the completion of the ENCODE project, one (of many) controversies centred round the individuating criteria used for genes and functional genetic elements.

Those worried about biological individuality often have in mind the growing number of bizarre biological phenomena that challenge our common-sense notions about what makes a thing *a* thing, rather than a collection of more basic parts. Anyone who has seen a yogurt advertisement in the last 10 years will know that our digestive system is home to untold numbers of tiny microbes, which, among other things, help us to digest our food and maintain normal metabolic function (see discussion in O'Malley & Dupré 2007, Dupré & O'Malley 2007). Are the microbes part of us? Are they different individuals inside of us? Perhaps the inside wall of the gut in fact marks an outer surface of the human, such that we are really long tubes in the void of which an ecosystem of microbiological life thrives.[102] The animal kingdom is full of these cases—and so is the literature on biological individuality. But symbiosis is not the only problem. Problems of individuality also arise when we consider 'superorganisms' such as the 8 square kilometre fungus in Oregon (Ferguson et al. 2003), or the 6000 tonne 40 000 trunk tree named Pando in Utah (Grant 1993). This collection of mushrooms and forest full of trees are believed to be single individual organisms, respectively, because they are connected via complex root systems and are genetically identical. In the late 1970s biologist Dan Jenzen caused a stir when he announced that populations of parthenogenic[103] organisms were in fact single individuals. Janzen (1977) described the clonal offspring of female aphids as 'pieces' of a rapidly growing parthenogenic individual; he described fields of dandelion clones as giant trees that spread laterally across the valley floor, rather than vertically toward the sky. Other troublesome cases have included lumps of algae and lichen, viruses, and the microbial populations that form biofilms[104] (Ereshefsky & Pedroso 2013). These cases are puzzling because they share some but not all of the hallmarks of prototypical individuals, such as physical connection (Hull 1980), genetic identity (Janzen 1977), or physiological integration (Wilson & Sober 1989). It is a complex question whether shared physiology, shared genetics, shared evolutionary fate, or something different altogether

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[101] Nature 356, April 1992.

[102] This possibility is discussed by Dupré (2007).

[103] Parthenogenesis is a type of asexual reproduction absent the fertilization of an embryo. In Janzen's cases what is relevant is the existence of a genetically homogenous population.

[104] A 'biofilm' is a collection of microorganisms that are connected by a self-produced extra-cellular 'slime'. They can be composed of one or multiple species.

combines parts together to form biological wholes. Beyond these cases there are many and more bizarre forms of life that have perverted attempts at reaching a consensus definition the biological individual.

It is surely the case that certain practical and epistemic issues hang on the definition of individuality on which biologists settle. So it is important that philosophers examine how biologists do or should conceive of individuals, and what the implications of various such conceptions might be. However it is an open question whether there is a corresponding metaphysical or ontological dimension to these issues. Not only is it unclear whether the pragmatically-selected definition could/should/does line up with where the individuals *really* are, it is unclear even whether it makes sense to talk about the *real* individuals, at all. Certainly the tone of debates about symbionts, clones, and superorganisms suggests there is a 'correct' answer to be found; and the word 'ontological' appears throughout discussions of biological individuality. However this alone does not establish that there is a genuine issue.

Debates about biological individuality are nearly always fought over organisms. It is not just that organisms are the proto-typical examples of biological individuals; it would seem that, for many, the search for an account of individuality will end if and when we find an adequate account of organism. To some extent this is a function of the practical and epistemic side of the individuality debate, which arises within the context of evolution and is therefore discussed primarily using organisms. So what is the ontological problem of the biological individual? There are two possible answers. On the one hand, the problem might be a matter of determining what is an individual life, or an individual living organism. On the other hand, the problem might be a matter of addressing an intuition about composition according to which certain things 'really' come together to form biological wholes and certain things do not. In this chapter I examine the latter question. My defence of this choice is simply that it forces us to include biological objects that are evolved but are not intuitively alive, such as biochemical molecules. This choice is also motivated by a desire not to discharge philosophical work to a positively baffling notion such as 'life', and by my extreme scepticism that 'living thing' picks out a category about which anything intelligible can be said.

1.1 Individuality as a Conceptual Tool

Faced with mycological monsters like the giant mushroom of Oregon, or symbiotic sycophants like our gut flora, biologists need a conceptual tool for separating organismal parts from the (apparent) functional whole—and not simply to satisfy curiosity. The way in which we conceive of individuality will have big ramifications for the way in which we model evolutionary systems.

A fundamental activity in evolutionary biology is counting. Sounds easy. But the trouble is that we do not always know what to count. Place me in an animal shelter and ask me to count dogs and I will have your answer in no time. Place me inside of Pando and tell me to count trees and I will need to first find a good armchair. Decisions on this matter have big implications. In addition to the obvious ones, such as affecting the number of individuals we take to be present, conceptions of individuality also impact reasoning about and modelling of selection and evolution. Individuals are bearers of fitness, non-individuals are not. It is individuals over which our evolutionary reasonings range. If individuals of type-a are fitter than of type-b, then we expect individuals-a to be present in greater numbers than individuals-b in the next generation. In fact this claim has meaning only in light of a concept of individuality. The terms featured in evolutionary theorizing are all relative to individuals: generations, traits, and phenotypes are all ‘ … of individuals’.

Pando helps make the problem concrete. Suppose that one of Pando's 40 000 ‘stems’ (trees?) acquires a mutation during mitosis, which leads to faster root growth and hence increased reproduction. This heritable growth pattern is an increase in fitness and so, *ceteris paribus*, selection is happening. For a scientist modelling Pando as the individual the selection will be invisible, lost within Pando's aggregate fitness. For a scientist modelling stems as individuals it is clear that within-Pando selection is happening.[105] Both conceptions map onto something important. In one sense there really is tree-level selection happening, which will impact the future composition of Pando. In another sense, this selection really is just one part of Pando's fitness, which must be balanced against all of the other bits of Pando if we are to understand Pando's future.[106]

Arguments about individuality often proceed by testing definitions against special cases, like those listed above. Thus recent reviews and articles reject extant theories on the grounds that they fail to capture the individuality of niche multicellural taxa (Herron et al. 2013), plants (Clarke 2012), or biofilms (Ereshefsky & Pedroso 2013). Just as single trees require a different individuality concept from Pando, so do plants require a different individuality concept from biofilms. This appears to support a relaxed common-sense pluralism about biological individuals. Much like pluralism about natural kinds and species taxa, pluralism about biological individuals is driven partly by the observation that different branches of biology and different investigations need different concepts, and partly by the failure to find a single theory that unifies our pre-theoretic use of ‘individual’. There are at least 13 definitions

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[105] A more detailed version of a similar example (using aphids) can be found in (Clarke 2013).

[106] You might think, as Clarke seems to imply, that averaging will misestimate the fitness. This is true only if fitness is taken as a static measure. If we measure instead change in fitness over time and average the changes in fitness over time from various parts of Pando, the result should be an accurate representation of the acceleration in fitness.

common in the literature, which carve up the biological world differently (Clarke 2010). The biological world contains a myriad of types of physical organisation. Not only are there huge differences between mammals, bacteria, reptiles, or archaea; there are also huge differences *within* these groups. It is this heterogeneity that frustrates consensus definitions of individuality. This, combined with recognition of the diverse explanatory demands placed on the concept, make pluralism quite tempting. It also makes the ontological question all the more interesting.

1.2 Individuality as Ontological Merit Badge

The ontology of the material world throws up a *prima facie* challenge for the realist. We do not experience a world of fundamental particles or isolated atoms; the world as we experience it is made up of composite things. The challenge is to give an account of when composition occurs; that is, when little things combine to make larger things. For philosophers of science the debate concerns the existence of groups over individuals, organisms over parts, and so on. For mereologists this debate concerns all physical objects above fundamental particles. The content differs but these discussions are substantially the same. The task is to determine how it might be that wholes exist consisting of parts. This task carries a concomitant burden: providing an account that distinguishes the clumps of parts that really form wholes from those that merely appear to do so. There is an intuition, and it is a strong one, that some things really do go together and other things really do not. It is the former things, the intuition goes, that our ontology should reward. Those things deserve badges of ontological merit. This intuition is 'moderate compositionalism' and I think it explains the drive behind ontological approaches to biological individuality.

The label 'moderate' sets this compositionalism apart from two more extreme forms: nihilism and universalism. Nihilism is the claim that composition never happens, that the world just is a distribution of fundamental particles. We can imagine why this is not an attractive option for philosophers of science. Universalism is the claim that composition always happens, that it is automatic that for any number of things there is another thing that is their composite. Universalism is harder to resist, for it would seem to be implied by the truth conditions for conjunction. If A exists and if B exists, then A+B exists. Universalism does permit us to claim that some lumps of atoms do form real wholes but this comes at a cost, for we have to admit that *all* collections of atoms do so. So universalism allows me to claim that the bundle of particles that makes up my cat Ellie[107] really do form a thing that is Ellie, but only if I am willing to admit that there are innumerably many other things that are those same

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[107] No, I did not forget the name of my cat. Joey, from the introduction, is one of two. Ellie was my second cat.

particles plus one or two more from somewhere else in the universe. To stick with the stock mereological example, there is some thing that is Ellie and the Eiffel tower. Both really exist and so too does their conjunction.

Moderate compositionalism lies in-between. The belief is that sometimes, in certain cases, under certain conditions, we have genuine wholes composed of parts. The trick is to come up with the conditions—then we can start conferring the badges.

Biologists appeal to a great number of higher-level organisations as real things. Organisms are the best example, but more abstractly some may want to think of populations, hives, oosts, herds, troops, kin groups and families as real objects. In fact, biologists do nothing *but* appeal to higher-level composites—they do not much care for fundamental particles. Certain philosophers of biology have sought to advance an anti-reductionist description of these higher-level individuals as genuine entities, not arbitrary collections of particles. Sometimes this discussion takes the form of physicalist anti-reductionism (Dupré 1993), sometimes of strong emergence.[108] Regardless, the claim is that certain collections of things deserve recognition as *real* collections. Implicit in this claim is the belief that other collections do not go together in this same way.

1.3 Individuality and Biology

Discussions occasioned by Pando, biofilms, the giant mushroom, and symbiosis are instances of the moderate compositionalist intuition. These debates appear to be more broad only because they represent a move beyond providing conditions for the possibility of composites and now address distinctions between composites at different levels. But this is only a small extension. Whatever the conditions are that allow me to sort the genuine collections of matter from the apparent ones, those same conditions allow me to claim that Pando is one such collection but that normal forests are numerous such collections. When we are giving badges for ontological merit, it doesn't matter whether we are sorting the fundamental things from the composite things or the single composite things from the collections of composite things, the desire in both cases is to sort the real from the apparent, decorating the former.

Moderate compositionalism is just as much at home in the context of biomolecules as it is in the case of organisms and other higher-level groups. A survey of the biomolecular world reveals phenomena very much like those that occur at higher levels. For one, protein molecules often join forces, sometimes for prolonged periods of time. These compounds are

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[108] British Emergentism, most closely associated with C.D. Broad, often focussed on biological examples of this sort. Today this is often read as a commitment to non-reductive materialism.

so common that they are often considered to be a final 'level' of structure for each partner, called the 'quaternary' structure.[109]

Additionally, the non-rigid and occasionally 'transient' (Mittag et al. 2010) nature of chemical bonding should suffice to raise mereological worries about biochemical molecules. Far from being groups of balls and sticks as we frequently picture them, biomolecules can be considerably more loose collections of free-moving atoms and electrons. The emerging view of molecular interaction among those who study 'disordered' proteins is of a loose aggregate of atoms and a sheet of electron density. The electrons do not bind to the target molecule but move dynamically to create a weak force of attraction. As a more general problem, that same thing occurs within molecules—proteins and otherwise—according to at least one influential definition of 'bond' (Bader 1990). On this view, bonds are not rigid links between atoms, but peaks in the molecule's aggregate electron distribution. Since anything in close physical proximity to the molecule will affect the energy of the system, we face a *prima facie* demarcation problem. Absent stiff bonds to help us decide what is and is not part of a molecule, we need some way of distinguishing the molecule from everything that surrounds and interacts with it.

There are many parallels between biomolecular composition and organismal composition. Both feature vague collections of interacting parts at multiple levels, which persist for various periods of time. Both exist as physiologically integrated composites of heterogeneous origins and opaque boundaries. Both appear to have persisted through generations *as* integrated composites. The moderate compositionalist intuition is as applicable here as anywhere in biology. If anything solves the composition problem then it will solve it for the entire biological world, not just for organisms.

Expanding focus to include biomolecules will not somehow provide *the* definition of individuals, as authors claim of certain other test cases. What it does, however, is highlight the fact that certain proposed solutions to the individuality problem are in fact solutions to a different problem altogether: the problem of defining an organism, or life. Superorganisms and symbionts can easily be viewed as cases where questions about individuals just are questions about individual lives. When implicitly taking this form, treatments of individuality hinge on immune systems (Pradeu 2012), reproductive bottlenecks (Maynard-Smith & Szathmáry 1995), and various other physiological marks of living beings. These are non-

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[109] There is interesting social epistemological work to be done to explain why we conceive of these as two separate things coming together and not two pieces forming a whole. Similarly, there are more basic mereological issues to do with protein origin. The amino acid residues of which a protein is composed can come from very different genetic origins. They may come from different regions of DNA, which may not even lie on the same chromosome. Again, the norms governing when we consider these amino acids to have formed a protein are complex.

starters—unless, of course, organisms are somehow the only genuinely compound things in the biological world.[110] Though popular, I do not consider these approaches to be applicable to the present discussion.

2. No Merit Badges for Causal Agency

There is much that can be said for what makes something an individual. As a first pass we can say that individuals are not properties; they are things that bear properties. This is Aristotle's definition of substance, later picked up and modified by Locke. On this conception, individuals are the sorts of stuff of which things are predicated but are not themselves predicated of anything. To describe them, as Locke found, is impossible; we are only describing lists of their properties.[111] This no more than gestures at a solution to the composition problem, since something predicated of a whole may well be shorthand for predication of some sub-set of the parts of that whole. Substance only gets us so far.

In a development along these lines, many philosophers of biology proceed with the claim that individuals are anything that has causal powers (e.g. Bapteste & Dupré 2013) or the similar claim that biological individuals are the locus of agency (Wilson 2005, Wilson & Barker 2013). I will treat these two criteria as equivalent and call the resulting theory the 'causal node' account.[112] This account is strongly tied to non-reductive physicalism about biological groups, but I will not dwell on this connection.

The causal node view is very helpful in fleshing out our pre-theoretic claims about individuality. We do seem to think that groups that act as a whole are good candidates for being treated as wholes. These things are referred to as wholes when reference to their parts seems redundant, given that they constantly behave as wholes as far as we are concerned. Often, causal features of a system only become intelligible when we conceive of some aggregates in that system as wholes. So the causal node restriction on individuality gives us a property of a compound entity to which we can appeal in talking about compound individuals. Though it is certainly true that this view provides useful epistemic shorthand, it is far from clear that the view can secure an ontological ground for composition.

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[110] Van Inwagen (1990) claims just that … and more! He thinks that there are no composite objects that are non-natural and that the only natural composite objects are unified by giving rise to life. So organisms (and maybe viruses?) are the only composites in the world. Everything else is fundamental particles appearing to form compounds. I will discuss this view below.

[111] There is a contemporary (though classically empiricist) view, in the philosophy of physics, that holds that individuals are *only* bundles of properties. This is the 'bundle' view.

[112] Getting rid of the word 'agency' is helpful. The causal node view is not the same as the view that certain biological things exhibit intentionality (as 'agency' might accidentally indicate). The view on offer here is rather that individuals are the sorts of things that can serve as nodes in a causal chain or network.

If this view provides criteria for composition, it does so by claiming that the piles of matter that really go together are those piles that exhibit causal powers *only as wholes*. Ultimately, the only way for this view to get off the ground is with a pre-existing assumption of the non-supervenience of the higher-level composites whose ontological status is sought in the first place. The argument is either circular or requires rejecting reductive materialism—and I am not about to concede that.[113]

It should be uncontroversial that behaviours and interactions of organisms are best described at the organism level. This requires treating the organism as a whole rather than as a collection of parts. When we are describing the grazing habits of a goat we do not describe the cellular interactions in the gut, their effect on dopamine receptors via neurotransmitters in the brain, which lead the goat to drop its head an munch away. Instead we talk about the goat getting hungry. Explanations are a lot more helpful when we can treat the situation as though there is a thing that is a goat and that this goat has goat-level behaviours and goat-level interactions. As an explanatory strategy, attributing agency and existence to the goat is invaluable. But explanatory utility does not an ontological status buy.

The ontological status of the goat as a badge-deserving individual hangs ultimately on its ability to be an agent of causal change over and above the causes of its parts. This position is frequently expressed as the claim that causation must exist as more than the sums of the individual causal powers of the parts:

> We assume that real entities are those that have causal powers; complex entities are real if they have causal powers that are not merely aggregates of the causal powers of their parts. Organisms, for instance, can do things that none of their parts can manage on their own. Similarly functional proteins have capacities – catalytic, structural, etc. – that are not exhibited by any of the amino acids of which they are composed. (Bapteste & Dupré 2013, p.380)
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> [T]here are may different kinds of things in the world, from physically simple things like electrons or quarks, to very complex things such as planets, elephants, or armies. Many or all of these things, in my view, have equal claims to reality. As the basis of this position is the idea that many or all such entities have causal powers that are not simply consequences of the way their physical components are fitted together. (Dupré 2007, p.12)
>
> [T]here is a whole hierarchy of increasingly complex things that really exist, and that have causal powers that are not reducible to the mechanical combination of the powers of their constituents. (Dupré 2007, p.15)

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[113] Noticing a similar dependence on non-reductive materialism in the context of special science laws, Callender and Cohen (2010) also reject this brand of metaphysics (somewhat more briskly than I do) claiming, 'If we have reason to believe anything in science, it's that macroscopic entities are constituted by microscopic ones and their relations. If we insist on this, as we do, [the account given by Dupré and others] won't do all of the things we want our metaphysics of science to do' (p 5).

These claims are grounded in observations of the explanatory utility of appealing to wholes over parts. If these epistemic observations are to be anything more than suggestive about the wholes that feature in explanations, some meat must be put on the bones of this position.

We can all agree that novel properties may arise in compound entities. Even the ontological reductionist can be happy with this claim, since it says nothing about the novel property being *more than* the sum of its parts. Weight is the easiest example. If we add the weights of the components of a composite we arrive at a summative weight. We call this the weight of the compound. It is novel in the sense that the sum is not contained in the list of weights added to obtain it; but this does not entail that the summative weight is somehow greater than the sum of its parts. It exactly is the sum of its parts.

Perhaps weight is too easy, since it is summative and relatively unrelated to causation. Take instead the claim about proteins from the first quote, above. Bapteste and Dupré are correct: The catalytic properties of a whole protein are novel. This is true in the sense that if we listed the properties of each of the composing amino acids, nowhere in that list would the catalytic role appear. The amino acids have many causal properties, many of which will not manifest once they aggregate into a single protein. This is certainly true of any conformationally-dependent properties, which will be increasingly truncated in systems that permit less freedom of movement. Does this aggregation simultaneously permit other causal properties? Surely it does. The question is whether these are numerous properties of some pieces or singular properties of the whole.

The causal node theorist will claim that the new catalytic property is a property of the protein. They must mean by this not just that no single piece has the catalytic property, since nobody would claim otherwise. Instead they mean that the catalytic ability is not simply a manifestation of the aggregation of some subset of the causal actions of the parts. I claim, in contrast, that the amino acids exhibit individual causal properties, which, in aggregate, constitute causal events visible at the protein level. The visibility at this level does not entail that some object at this level deserves a badge of ontological merit.

The issue is not whether there is causation happening at the higher level. Several popular theories of causation are equipped to handle causation at macroscopic levels. The account preferred by causal node theorists is the interventionist framework made popular by James Woodward (2003). Interventionism is an undemanding account of causation in the sense that it places few constraints on what counts as a 'genuine' cause, adopting a metaphysically deflationary attitude toward any connections between causally associated variables. For an interventionist, X causes Y just in case, given stable background conditions, following some intervention on X the value of Y would change.[114] This account has become popular for conceptualising causation in biology (Waters 2007, Woodward 2010). But the interventionist framework alone cannot support the sort of ontological claims needed here.

Notice that the interventionist is uncommitted to the existence of causation at any particular level. Causation exists at whatever level of description it is appropriate to make these sorts of claims. So we can talk about electrons causing changes at the sub-atomic level, proteins causing catalysis, baseballs breaking windows, and economic policies causing recessions. Anytime we can define two variables and claim an interventionist relation between them, we have described causation. On this view, providing a causal explanation just is providing information about a case of causation.

The interventionist picture places no constraints on the relata of a causal relationship. Though many causal theories speak of 'events', Woodward speaks even more generally about 'variables'. In the statement 'X causes Y', X and Y can be any multi-valued variable. Often these variables will be properties of the things in our causal chain, but they can also be Boolean representations of events' occurrence or non-occurrence. In the baseball case, X can stand in for the position of the baseball in space time and Y can stand in for a property of the glass, broken or whole. To claim that the baseball causes the window to break is to claim that a change in the value of the spatial location of the ball would result in a change in the value of the state of the window.

Simple cases like this seem to vindicate the causal node view. In spite of the fact that the baseball is composed mostly of isoprene and the glass of silicate, we predicate the causation of the wholes, not the molecular parts. But many cases are not so hospitable. Switch now to the case of a protein catalysing a reaction. In this case, X is presumably a state-space description of the protein molecule, while Y (catalysis) does not belong exclusively to any of the reactants. The antecedent variable Y can be described as a property of all reactants, or of the local system. A change in the state space of the protein [X] may result in the disappearance of the reactants [Y1], or it may result in a new state of the local system [Y2] (viz that the system now contains a different arrangement of particles in space). Does the second description make the local system an individual? How is this to be negotiated? The causal relationship could also be broken down into components. Perhaps X causes first a change in binding [Y], where Y= a range in the peak electron densities within one domain of one reactant. Later X causes a lowering of reaction potentials [Z], where Z= the state-quantity of rate-limiting free energy of the local system. Notice that I've left the left side of the expressions static (X=protein molecule) but I could as easily break it down along similar lines. In each case of binding the

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[114] See Woodward (2003, p.98) for an explanation of 'intervention'.

relevant variable is not the whole protein but the peak electron densities of some sub-set of one domain. So where are the individuals?

The interventionist framework can subsume any group under a single variable where interventions causing changes in that group reliably result in changes in some other variable, under at least some circumstances, however narrow or unlikely. For this reason, the framework does not supply the sorts of composites that the causal node theorist desires.

In general, causal claims are a poor guide to an ontology of entities. History is full of cases where accurate causal claims turn out to have been based on a misunderstanding of the entities involved. Woodward (2010) discusses a few such cases, explaining that we should be realist about the causal structure, not the particular relata we invoke in representing that structure. I do not mean to claim that the groups referred to in interventionist style causal expressions are not somehow interestingly different from the groups that cannot feature in these expressions. Baseballs have some basis in our experience of reality that my cat and the Eiffel Tower do not. That is evidenced by their ability to feature in successful causal explanations. I suspect, however, that the moderate compositionalist intuition at play in discussions of biological individuality will remain unsatisfied in the face of composites permitted by the interventionist framework. Certainly it would not deliver intuitive verdicts on symbiosis, superorganisms, or other popular cases.

So far I have established only that the issue is not simply whether or not causation is happening at higher levels, since composition is unhelpfully unproblematic on metaphysically and ontologically deflationary (or uncommitted) schemes. The causal node theorist must find some grounds on which to argue that the levels of specific composites are better or more accurate representations of the structure of the world.[115]

One attempt appeals to our ability to unify explanations by predicating them of a single whole. Protein catalysis could be described as a series of disconnected causal relationships predicated of domains and sub-sets of total molecular structures. But this picture does not allow us to see that all of the protein domains are domains *of a protein*. One might wonder whether these are distinct events of objects located in close spatial proximity, or if they are parts of a larger event of a single object. The only way to conceive of these as parts of *one* event is to predicate them not of a collection of disconnected atoms or domains, but of an integrated whole protein. But the argument quickly reveals itself to be circular. If we need to find a protein-level cause in order to ground the claim that the protein is an individual and

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[115] I made this same point, targeted instead at mechanistic theories of causation, in (Bartol 2013b). This criticism is compatible with (if not enhanced by) the causal pluralism advanced by Dupré (2013). It is that argument that leads me to believe that his causal node view is intended not as a metaphysical account, but rather (what I call) an epistemic or pragmatic one.

not a collection of atoms, then we cannot use a claim about the protein as an individual to secure the existence of the protein-level cause over and above the atom-level causes.

This circularity objection can be motivated from a second perspective. The appeal to higher-level composites faces the very mereological conjunction and perspectival problems that it is meant to avoid. For any two really existing causal capacities, there will be a third thing that is the conjunction of those capacities. This will be true even if the third thing is a gerrymandered composite. The only way to avoid the gerrymandered composite capacity is to take recourse to pre-existing beliefs about where the individuals are and tie capacities to that level. Take the grass-munching goat. The neurotransmitter activity, on the one hand, and the musculo-skeletal reaction, on the other, really exist separately. Their conjunction, too, exists. But this is okay because we think the conjunction really does exist and we call this 'eating grass'. We can understand the two disconnected phenomena as a *single* phenomenon when predicated not of neurons and muscle tissue but of a whole goat. Now think of a gerrymandered conjunction. Take the chewing of the grass and the running away of the nearby ants. Both really exist and so does their conjunction. While we might grant that these two sets of causal capacities are connected in some way, the moderate compositionalist does not want to say that they form some single larger causal capacity. This is because that single larger capacity would be a capacity belonging to a bizarre combination of goat-jaw-muscle and ant neurophysiology. The only way to save our ontology from these Franken-individuals is to appeal to pre-existing notions of what the individuals really are and to then predicate our causal capacities of those individuals. That would beg the question.

One further option remains, but it is not particularly attractive. In order to address the moderate compositionalist intuition, the causal node view may hold that there is some feature present at the protein level that is not present at the lower level, and that this feature, whatever it is, ensures that some type of causation obtaining at that level is unique to that level such that the higher-level cause is not merely 'the sum of its parts'. This claim is a statement of non-reductive physicalism. Perhaps there are cases that motivate a rejection of reductive physicalism—some hold out hope that mental activities necessitate a re-thinking of that position—but even *if* these cases exist, the cases at issue in biological individuality do not seem to be among them.

Surely most philosophers of science are reductive physicalists[116], believing that phenomena at (e.g.) the protein level are ultimately the product of the atoms and interactions at the level of component atoms, their bonds, and their environment. The ability of a protein to catalyse a

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[116] It is extremely important that reductive physicalism is understood as an ontological claim and not a claim about explanatory or methodological reduction. These three forms of reduction are too often conflated. See (Brigandt & Love 2012, van Riel & Van Gulick 2014)

certain reaction is not magic; it is a matter of certain atoms with certain properties in a certain spatial relation under certain conditions. In this sense, then, the capacities of the protein very much *are* the collective dispositional properties of the atoms of which it is composed. The causal node theorist has to deny this, believing that there is something to the protein that is not just its components. It is very difficult to imagine what this amounts to.

Even if we grant that there is a thing that is the protein (as a composite object), it would still be the case that the protein is entirely inert, causally irrelevant to the catalysation.[117] All of the catalytic actions are performed by the constituent atoms alone. If we had all of the constituents of the protein in the right organisation and context then we would achieve catalysis. Adding one additional abject that is 'the protein' does not change a thing. It cannot be the case that the protein is a joint cause of the catalysation along with its constituents, for if that were the case then we would have to deny that the mere presence and spatial arrangement of the constituents was sufficient for the catalysation effect. For this to be true the protein as an additional object would need to play a non-physical role in the catalysation (non-physical because its physicality just is the physical constituents that are apparently insufficient). This strikes me as absurdity.

3. Merit Badges for Kindness

Philosophers who discharge notions of individuality to agency or causality do so for good reason. What they recognize is that we can talk about individuals with friends and family without problem, but that among philosophers the common sense concept becomes complicated. The simple, easy to apply everyday concept of 'individual' is, upon closer inspection, neither simple nor easy to apply—as is so often the case when we probe common sense too deeply. The causal node view is helpful because it happens to run roughly parallel to the common sense application of the term 'individual' and because it fits with biologists' appeal to higher-level composites in scientific explanations. But it cannot do much more work than that—it cannot play a role in biological ontology. In these final sections, I will argue that in fact no new account of biological individuality needs to do this ontological work. We already have a tool for the job: Kind Historicism. I will also claim that the Kind Historicism approach to biological individuality is compatible with Ellen Clarke's (2013) account.

Guiding both the case for the causal node view and the objections against that view as an ontological guarantor are intuitions about the status of 'genuine' biological individuals like organisms and proteins. While I share the compositionalist intuition, I think that awarding

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[117] A good run-through of these overdeterminer style arguments can be found in Trenton Merricks' *Objects and Persons,* (2001 chs. 2 *&* 3).

ontological merit badges for individuality is the wrong move. What we want is to say that Ellie is an individual that does not include any architectural features of Paris, or that a cell is an individual in a way that the cell and a stray amino acid are not. There must be a tool that tells us that some collections of biological matter form wholes. And there is: Some collections of matter are such that they form particulars that are members of biological kinds. Ellie and the cell are each clumps of matter that are the present result of long-run historical processes. That is the source of their ontological merit.

3.1 Individuals as Targets for Selection

Clarke (2013) has identified a workable, consistent, and universal definition of biological individuality, by focussing on the two mechanisms required for selection to operate at a given level of organisation. Some mechanisms must (i) limit an object's capacity for within object selection; and (ii) increase its capacity to undergo between object selection. At first pass, there are serious differences between Clarke's theory of individuals and my own account of individuals *qua* members of kinds. At a deeper level, however, the views share a core insight: that if anything performs composition, it is the historical process of selection.

The novelty in Clarke's approach consists in the recognition that individuation mechanisms are multiply-realisable. This explains the plurality of individuality concepts in use—each attach to a set of selection supporting mechanisms specific to one realm of biology or another. Any collection of biological matter at a given level of organisation is an individual *iff* it possesses some mechanisms that satisfy the conditions for the possibility of selection at that level. Because conditions sufficient for selection can still be insufficient for selection *only* at that level, individuality can exist in multiple levels of individuals for the same set of things.

Clarke's account shares a central insight with my theory of biological kinds: that the composition of biological objects is tied to their involvement in natural selection. Yet we look in different directions. In the previous chapter I stressed the role of historical trajectory through time in shaping biological objects. For Clarke, the focus is on biological objects that will alter the make-up of populations of future such objects. While I emphasize the existence of change and stability over time, Clarke has a practical interest in identifying the present sources of future change. I face backward and Clarke faces forward, owing to our very different goals.

The practical epistemic orientation of Clarke's project creates a second difference: a focus only on things that are selected. Not even all common-sense organisms will count as individuals, on Clarke's account, since it is possible that an organism could lose one of its individuating mechanisms. We can imagine an organism that moves from sexual reproduction to asexual parthenogenesis (this is common in plants and insects) constituting a loss of the mechanism providing for between-object selection.[118] The individual will have moved up one level, to the clonal group. The former individuals are still part of selection, just not a direct part. They still reproduce, of course, and their current form will still be a product of past selection at various levels. But they are rightly overlooked by Clarke's theory because they are overlooked by selection, moving forward. On a practical approach, the causal relationship between the current generation and the next in non-selected objects is unimportant. It still exists, but is overlooked. It is overlooked because it is irrelevant to selection. Changes in the current generation *would* yield changes in the future generation, if they existed; it is simply that there are no such changes. The next generation will be the same as the current—or at least as similar as genetic homogeneity allows. So stasis in the first generation yields stasis in the next. The relationship is present, but inactive. Inactive causal relations maintaining stability can be overlooked by a model of evolution, since stability is the default assumption.[119]

Biomolecules offer additional cases of non-selected but reliably recurring biological objects. They are complex integrated composites that have persisted through time *as* composites and whose current forms are functions of their histories. Some of these biomolecules will have participated in selection events at the level of their vehicles. Cells, tissues, organisms and other objects acted on by selection will bring whatever population of biomolecules they contain along for the ride. We may also grant that at least some biomolecules will participate in selection at their own level. Even still, most will exist in a form that is more accidental than selected—that is, most will not exist at a level of selection. Like the hypothetical parthenogenesis case, changes in the world population of biomolecules is not usually a result of selection between biomolecules of different fitness, but is rather the result of the selection of an entire population of biomolecules—either the reproduction of an organic body in which those molecules are contained, or a population of bodies containing a population of molecules. An epistemic forward-looking tool for conceptualizing evolution can overlook these biomolecules since their fate is controlled at a higher level.

Clarke shares the central insight that composition of biological objects is tied to natural selection, and her neglect of the historical nature of this relationship and of the status of non-

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[118] Sexual reproduction guarantees genetic variance, which provides a basis for selection between organisms at that level. Asexual parthenogenesis results in genetic clones, removing the basis for selection at that level. Selection then moves up to the level of populations of clones, rather than the organisms making up that population. From the standpoint of selection, all clones are 'one' organism. Parthenogenic organisms were at the heart of Janzen's (1977) classic paper, which in many ways initiated the contemporary debate about these issues.

[119] Brandon and McShae (2010) claim that the default assumption of evolutionary theorizing is not stasis but a tendency toward growth and complexification. This is true only insofar as the starting point is heritable variance. The situation I describe has heritability but lacks variance. The default assumption here is thus (relative) stability.

selected but recurring biological objects is a reflection of the epistemic demands of evolutionary biology. It is therefore not the case that her account differs importantly from mine. To the contrary, for understanding ontology there are good reasons for focussing backward and for including non-selected biological objects; for modelling selection there are good reasons to look to the present sources of future change. If we must award badges of ontological merit, satisfying the moderate compositionalist intuition, there are compelling reasons to focus on the lumps of matter that are united by selection. When we do so, we arrive at Kind Historicism.

When we claim that a given biological object is an individual, grounding its compositional structure through biological conservation, we do not necessarily mean that *that* object is a conserved individual. It may well have no offspring. It may be sterile. It may fail to participate in the individuating mechanisms characteristic of its group. Any number of accidents may prevent that particular from being a target of selection or a subject of conservation. What we really mean when we say that a given object is an individual is that it is a member of a kind of object that is selected or conserved or has the right kinds of individuating mechanisms or what have you. The kind membership is doing all of the work. Notice however that this does not necessarily work for any view of individuality. Many of the conceptions of biological individuality cited earlier in this chapter focus on the possession of particular structures or tendencies or abilities that are possessed by the particular, not the kind.

There is a plausible objection to my extension and 'ontologizing' of Clarke's theory. The persistence of non-selected composites as composites is rather accidental. They simply hitch a ride on some selected creature's back. Because selective forces are not acting on any feature of these composites, but rather on features of other composites to which they are attached, it might be objected that things like biomolecules do not deserve to count as having been compositionally guaranteed by historical selective forces. There is nothing about the molecules themselves that ensures their stabilized recurrence as composites. There are no facts about the molecules even that ensure their compositions. This is provided by their hosts, the recurrence of which stabilises the composition over time.

Were this objection to stand, not only would individuality be limited to selection-apt organisms, it would be limited to a sub-set of that class. Many populations that posses individuating mechanisms do not possess them intrinsically. The mechanisms may be a feature of the environment. Clarke (2013) acknowledges this in a footnote:

> I remain agnostic, at this stage, about whether individuals must possess individuating mechanisms intrinsically –within their own skins, so to speak. Perhaps it is sufficient, especially in the early stages of a transition, for the

> mechanisms to exist in the environment, so long as they are stable enough that their effect is heritable. (p. 427)

As an example, it may be that the mechanism preventing within-object selection is simply a reliable feature of the environment that renders within-object differences fitness-irrelevant. A new environment can mean that existing features of individuals now do or do not confer fitness, where previously the opposite was true. Surely such objects are being selected 'accidentally' if any are, for the features that permit selection are not features of the object at all. Yet it would be odd to start sorting individuals by determining which were sufficiently responsible for their own selection. Insofar as Darwinian forces are blind to selective culpability, so too for theories of individuals and natural kinds.

In sum, the theory I have described ties individuality to selection, such that particulars are individuals to whatever compositional level is the present result of past selection, or will be the future result of present selection. This should seriously change existing conceptions of the sorts of individuals we expect to find. First, this approach will not yield static or unchanging individuals. The pieces of (e.g.) organisms that are replicated from one generation to the next are constantly changing. Individuals therefore feature ever-changing lists of parts. Second, there will not be a singular privileged level of individuality. We will not find, for instance, that *the* individual is the microbe, or the cell, or the human. All of these things are individuals, inside of and around each other. There are individuals full of individuals, filling yet additional individuals. Finally, as discussed in Chapter 4, the Kind Historicist approach to individuals will not provide a workable system for science. We simply will not know what pieces of biological individuals have/will participate in sub-sequent generations. The precise compositional content of biological individuals, on this view, is therefore both dynamic and (generally) epistemically inaccessible.

5. Conclusion: A Bombshell in Two Bullets

This chapter has focussed on biological kinds at the expense of physico-chemical kinds. This is largely because there is not a strong chemistry or physics analogue to the biological and philosophical focus on biological individuality. Compositional problems arise equally strongly with chemical molecules, but these are generally addressed by mereologists and ontologists, not philosophers of science and chemists. But the moderate compositionalist intuition is surely as strong with chemical molecules as anywhere. We want to say that the Hs and the Os in a water molecule really go together, but those Hs and the Os really do not go with the Fe molecules in the Eiffel Tower. Compositional problems in physics and chemistry are the standard compositional problems of the SCQ.

The account I present here will not work outside of biology, tied as it is to Darwinian processes. I mentioned at the beginning that there is no consensus answer to SCQ, and that remains true. Historical individuality will not provide a generalized solution to that problem. Nevertheless, one might think that the *form* of the answer is generalizable. Why not ground physico-chemical composites in their kinds? Perhaps there are two *categories* of individuals, just as there are two categories of kinds. But this type of solution will not do. Microstructuralist theories of kinds do not support claims of individuality.

In the biological case, the boundary problems that appear from the individual perspective disappear on the natural kinds perspective. The physical composition of a particular in a kind is whatever composition has evolved.[120] The account is one of demarcation and individuation through historical process. For physico-chemical objects, however, boundary problems remain even on the natural kinds perspective. Recall that for physico-chemical kinds I have adopted the received view: microstructural essentialism. This account makes sense of the fact that an individual atom can come together with other atoms to form molecules, but that these atoms are simultaneously parts and wholes in their own right. The view provides nested layers of individuality. Even when a molecule is part of some much larger composite (like an organism) the molecule nonetheless still exists as a genuine individual molecule, owing to its microstructure. But this claim that some microphysical properties are essential for kind membership does not in itself help draw boundaries. Boundaries around individuals will be boundaries around microstructures, but neither come to us pre-delineated. A molecule has a microstructure, but so too does that molecule and its local environment. A lone hydrogen atom has a microstructure, but so too does the composite that is the hydrogen plus the Eiffel Tower. The microstructuralist account of natural kinds has no in-built demarcation mechanism.

The microstructuralist has two options, but neither are particularly favourable. One might first supplement microstructuralism with a mechanism for demarcation. The obvious choice would feature an appeal to physical boundaries. A sceptic might press this reply, however, noting that physical connection is at best a statistical notion at the molecular level since weak forces obtain between one atom and any others within certain physical proximity. Perhaps this is a bullet the microstructuralist is willing to bite. A second option is for the microstructuralist to bite the bullet on the demarcation problem, surrendering to universal compositionalism. On this view, the natural physico-chemical kinds are all things—and their combinations. This would mean that the *only* badge-deserving sub-set of the class of universal compositions would be the biological kinds!

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[120] See Chapter 4 for a discussion of the epistemic inaccessibility of Kind Historicist ontology.

The first bullet will be preferable for many, but I suspect that the second is more defensible. A definitive account on this matter requires a far richer account of atomic constitution than I am equipped to give. The problem is interesting because it turns the tables on our thinking about kinds. Where formerly the standard examples were chemical and the biological were thought the outliers, it may be that the class of chemical kinds is unwieldy and the biological kinds are neat, tidy, and discrete by comparison!

6

Looking for (Scientific) Answers in all the Wrong (Philosophical) Places

In the previous chapter, one of the minor messages was an advised caution during quests to award 'badges of ontological merit'. When it comes to biological individuality, a pragmatic approach is better suited to the contexts in which questions of individuality arise. There is a common assumption that there is a link between ontological status and scientific utility. This assumption leads philosophers (and scientists) to expend philosophical effort seeking to determine what things *are,* in a rich ontological sense, in the hopes of aiding scientific understanding and manifest ontology.[121] These enquiries are as misguided as the assumption on which they are based. This assumption and these enquiries are the topic of this chapter. Instead of focusing on individuality, I here examine two cases where classification is at issue, where authors trade arguments about the status of certain classes. Those classes are, first, human races and, second, evolved cognitive modules. During the development of Kind Historicism, in PART I, I explained why historical kinds reveal the gap between an ontology of natural kinds and scientific classifications leveraging induction-supporting kinds. In keeping with that message, I now advocate shifting focus away from determining *the* status of classes and toward focussing on their robustness, objectivity, and ability to support scientific prediction and explanation.

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[121] On 'manifest ontology', see section 2 of the Introduction.

The overarching goal of these two different discussions is the same: I hope to show why ontological status is irrelevant to each debate, motivating a focus on induction-supporting kinds. The particular motivations for each discussion, however, differ slightly, and thus warrant brief outline. In the case of human races, discussion of natural kinds has recently given way to discussion of 'biological meaningfulness'. I want to show why this is little more than a change of language, and that focussing on ontological status, in any guise, is a mistake. I wish to move the debate in a more productive direction by focussing on supporting induction rather than meeting unmotivated *a priori* ideals. I will not long linger on the question of races as natural kinds because the question is irrelevant. In the case of cognitive modules, on the other hand, I wish to guard against a misapplication of Kind Historicism. I will thus argue at length that cognitive modules are not natural kinds, with reference to the complicated relationship between cognition and the neural architecture by which it is supported. Thereafter I will explain why cognitive modules may yet be good kinds for science. I will conclude each case with a discussion of the possible motivations behind seeking scientific answers in philosophical ontology.

1. Race

Racial classification is perhaps the most impactful classification of humans in modern times, and so it is unsurprising that scholars have wondered about the nature of racial groups. Like any biological kind, intrinsic heterogeneity precludes races as traditional structural kinds. This has been documented at length (e.g. Root 2003, Zack 1993, cf. Gannett 2010, Spencer 2012). In light of the disrepute of natural kinds in biology and the overwhelming evidence against races as structural natural kinds, debate has shifted to a new concept: 'biological meaningfulness'.[122] But this shift is lateral, not progressive. Biological meaningfulness attempts to establish ontological status in a new guise. In this short discussion, I would like to focus on this concept as it occurs in one recent exchange (Hochman 2013a, 2013b, 2014, Sesardic 2010, 2013, Spencer 2012, 2014) in which 'biological meaningfulness' is functioning as an unhelpful ontological merit badge, distracting attention from more relevant scientific questions about the robustness of racial classification and more interesting socio-ethical questions about the use of racial classifications.

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[122] Some philosophers still defend races as natural kinds, but their view of kinds is what I would call 'induction-supporting' kinds. E.g. (Andreasen 1998, Kitcher 1999, cf. Kitcher 2007). The latter work by Kitcher modifies his earlier view, building socio-ethical concerns into a conception of 'natural' (induction-supporting) kinds.

The race debates are huge, and I will not attempt a survey here.[123] Instead, I focus on this recent debate, which culminated in a heated exchange in the pages of *Studies in History and Philosophy of Biology and the Biomedical Sciences* (Sesardic 2013, Hochman 2013b, 2014, Spencer 2014). At the forefront of these debates is Adam Hochman, who defends the claim that races are not biologically meaningful. Sesardic and Spencer call attention to the opaqueness of this notion. Building on their views, I will show that the 'meaningfulness' Hochman critiques is not tracking induction-supporting kinds, nor is it akin to objectivity. For Hochman, meaningfulness is doing some ontological work, carving nature's joints. There are thus three issues.[124] The first concerns the status of 'race' as an induction-supporting kind. The second concerns the biological objectivity of races. The third concerns the ontological status of races. The current debate is a mess of all three. Races are demonstrably meaningful as induction-supporting kinds in science at present. These kinds support inductions because they have some objective basis in biology, making them meaningful in this second sense, too. I will argue that these two senses of 'meaningfulness' are the two that matter for the present concerns of biologists and philosophers. Whether races are natural kinds, or individuals, or species, or subspecies or any other badge-deserving set should not keep us up at night. I will conclude with some speculations on the appeal of ontological merit badges.

As a final note, there are so many nuances to this issue that I must ignore. I cannot discuss for instance the relationship between 'common sense' racial groups and more robust racial groups, the non-biological (social) forces that link certain races and certain traits, the best way to define races for scientific purposes, and the many social and ethical implications of using racial categories in science. These are all important issues that warrant attention. In this discussion, when I claim that worries about ontological status are a distraction in the race debate, it is these sorts of issues from which I believe enquiry is being distracted.

1.1 The Meaning of Biological Meaningfulness

Hochman (2013b, 2013a, 2014) claims that races are not biologically meaningful. He claims that for races to be biologically meaningful they would have to meet the criteria for subspecies. Before examining the unusual claim about sub-species, it is helpful to ask what

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[123] For comprehensive analysis of the race debates concerning kinds, meaningfulness, and all manner of ontological and scientific categories, see (Gannett 2010, Spencer 2012), both of whom urge a move away from ontological worries and toward pragmatic focus on objective likeness relations (also, in the case of Gannett, socio-political issues divorced entirely from classification and kinds).

[124] Quayshawn Spencer (2014) imposes a helpful distinction into the ongoing debates, which partially maps to the three-part distinction I use here. He identifies 'the philosophical race debate', which is about the nature and reality of common races, and distinguishes this from 'the biomedical race debates', which are about whether races are useful categories for biomedical and genetics research. The former is related to the naturalness and metaphysical takes on race. The latter is a specific instance of the induction-supporting kinds take on races.

'biological meaningfulness' ought to mean. There are two possible senses of 'biology'. Either races are meaningful to biological science, or they are meaningful to the biological world. I will interpret the former as a matter of investigative utility (induction-supporting kinds) and the latter in terms of having some objective basis in the biological world. Races are meaningful in both senses. Hochman would not view these interpretations as meaningful enough, insisting that they meet his *a priori* ideals.

Scientists demonstrably find racial classification to be informative. Racial categories are useful as both conceptual and communicative tools in many branches of science. When members of a race reliably share some specific set of trait and properties, they meet the conditions for induction-supporting kinds. This means that racial classification can provide information about members of that race. And it does. There is a reason that anthropological understanding often begins with race. In biomedical science, too, races are used as proxies for deeper physiological differences (see Risch et al. 2002). Pigliucci and Kaplan (2003) identify a number of ways in which human racial sub-groups are specified that find uses in different sciences. We know for instance that people of east-Asian descent are very likely (~80%) to have polymorphism(s) in the enzyme(s) responsible for alcohol metabolism, which result in a general intolerance for alcohol.[125] When assessing risk for cardiovascular diseases and endocrine disorders, family doctors are taught to begin patient histories with gender and race, since these are excellent predictors of risk. Races are meaningful in the same way that many other classes can be. Medicine uses generalizations about tall people, fat people, people with habit *x* or condition *y*. These are all kinds that support inductions. This is not racism or prejudice; it's science—statistics, generally.

Yet we find claims such as the following, quoted here disapprovingly in Sesardic (2013):

> Consider typical statements made repeatedly by leading racial constructionists that race is biologically 'meaning- less' (AAA, 1994; Fish, 2002, p. 138; Gould, 1996, p. 379; Marshall, 1998, p. 654; Rose, 2002; Schwartz, 2001), that 'race as biology is fiction' (Smedley & Smedley, 2005), that 'race is the phlogiston of our time' (Montagu, 1964, p. xii; similarly Hirschfeld, 1998, p. 36), that 'race' is a concept like unicorn (Fish, 2002, p. 138), that 'the reality of human races is [. . .] destined to follow the flat Earth into oblivion' (Diamond, 1994). (p. 287, citations in original)

These cannot be claims about the usefulness of race. These authors are presumably not ignorant of the use of racial groups in modern biomedical and anthropological research. These must be claims about the reality or objectivity of races. This is the second possible meaning of '*biologically* meaningful', which in these debates is called 'racial naturalism'.

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[125] There are a number of variants to the enzyme, which are variously and compoundly shared by persons of Asian descent. The alcohol dehydrogenase enzyme variants convert alcohol into the toxic acetaldehyde, resulting in a variety of unpleasant side-effects (Li et al. 2009, Peng et al. 2010).

Racial naturalism is the claim that races have some basis in the way the world is, rather than the way we perceive it. This goes hand-in-hand with the scientific utility of race. Racial categories are able to support inductions in several contemporary sciences because they are reliable indicators of certain properties. They are reliable (when they are) because races exhibit a reliable and measurable stability with regard to specific sets of properties. This is at least partly because the sorts of ancestral relations that races are intended to track will, in certain cases, support some measure of restricted phenotypic similarity.[126] It need not support widespread or universal similarity, only reliable similarity in the respects germane to the generalisations in which it features. We know that people of east-Asian descent are reliably similar in some set of ways tied to the metabolism of alcohol. We know that Black men are more often than not similar in some set of ways tied to prostate cancer.[127] A number of accurate generalisations can be made about a number of races, grounded in objective features of the world. Insofar as race is a tried and tested scientific tool, racial naturalism is vindicated.

As a matter of clarification, racial naturalism and scientific utility do not require perfect uniformity among members with respect to the trait(s) over which predictions range. Rather, they require only that the trait(s) be shared with a sufficiently high frequency. So long as that frequency is better than chance, and absent better predictors of the trait(s), race will find use in science as a category predictive of the trait(s). In this regard it is important to notice that, when races *are* used to support predictions, those predictions are statistical. The modal relations between classes and the traits predicted of them is one of statistical likelihood, not necessity. This is as true of racial categories as it is of any induction-supporting kind in the biomedical sciences.

Perhaps racial naturalism, though useful and objective, is not 'biologically meaningful' enough. It puts races on the same plane as categories like 'brunette' or 'mesomorph' or 'tall person'. Perhaps members of races have a few more properties in common than these groups, but all represent groups whose members non-accidentally share some number of properties beyond the property in virtue of which they are grouped. A critic might insist that biological meaningfulness requires that races are some extra special type of category. Following criticism (Sesardic 2013) this is the line pursued by Hochman (e.g. 2013b, p.285). After rejecting the type of naturalism and objectivity I have just outlined, he goes on to explain that,

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[126] Sometimes these generalizations will be stable for reasons not connected to biology. It may be that relations between a race and a trait are stabilized by social forces, as in cases where racial categories track socio-economic class. Even still, notice that this stability is still objective (it's merely contingent on social rather than biological factors) making the class suitable for induction.

[127] Though some of this increased risk has to do with social and economic factors, at least some also owes to inherited genetic factors common to sub-Saharan African descent (Batai et al. 2012).

barring a stronger level of meaningfulness, racial groupings are not 'good categories to do science with'. This final claim is strange. It is at odds with all of the science that uses races reliably, none of which are discussed by Hochman. Perhaps races are bad scientific categories for social or ethical reasons, but Hochman's argument is thoroughly scientific and metaphysical, not ethical.

Hochman has set the bar high for races. He asks that they fulfil the criteria for sub-species membership—which, he demonstrates, they do not. Having ruled-out Hochman's interest in either utility or objectivity, I must conclude that he believes that vindication of racial categories requires that they achieve some higher ontological status, and that sub-species is a stand-in for this status.[128] Before asking about these motivations, I will briefly digress to review the cases for races as natural kinds.

1.2 Races as Ontologically-Privileged Classes

Races are not structural kinds, for the same reason that any biological class is unlikely to be a structural kind: there will be no shared morphological, phenotypic, or genetic structure that binds races together. This point has been made by Root (2003) and Zack (1993).[129] Since my goal in this section is to claim that ontological status is irrelevant here, I will allow only brief digression into the topic of races as historical kinds.

Recall from Chapter 3 that, for Kind Historicism, historical kinds are relational. Two particulars can share large swaths of evolutionary history, diverging only recently, or they may share little history, diverging millennia ago. In cases like species and proteins this still leaves us with recognizable (if often epistemically inaccessible) groups, since speciation events are rare and, post speciation, particulars cannot (generally) re-breed into kinds to which their descendants formerly belonged. Races are not so hospitable. At the very least, widespread interbreeding rules-out any notion of the few canonical races as genuine natural kinds. Kind Historicism suggests things will be much more complicated than that.

The point of Kind Historicism is not to innumerate the actual kinds, but to emphasise the way in which biological particulars are historically rather than structurally related, how

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[128] One might think that Hochman merely views 'subspecies' as establishing an appropriate level of objectivity relative to the explanatory demands placed on them. If that were the case, however, then we would expect a discussion of the uses of race in science that shows why a certain level of objectivity or robustness is required and why (only) subspecies status provides that level. No such pragmatic worries are in evidence in his account.

[129] Genetic identity within races is impossible; but some high degree of genetic similarity might be thought to constitute a shared essential genetic microstructure. Absent some baseline for genetic relatedness, however, it is hard to determine what degree of similarity is similarity enough. Hochman (2013a) attempts to stipulate the amount of similarity but, as Sesardic (2013) comments, this smacks of arbitrariness. At any rate this is entirely incommensurate with standard approaches to structural kinds.

historical relations do not provide the relative homogeneity enjoyed by structurally related particulars, how historical kinds will often be unknowable, and why all of this precludes the use of historical kinds in science. With race this message becomes all the more important. In addition to the normal problem of intrinsic heterogeneity, interbreeding builds in further variance. The massive and widespread interbreeding of different populations also means that historical relations between and among races will be too complicated to track. Races as natural kinds is not a helpful topic of discussion.

Is this a problem for Kind Historicism? I think not. First, this finding re-enforces the point that natural kinds (particularly historical kinds) are poor tools for scientific investigation. Second, the messiness of race on the Kind Historicist account accords with contemporary biologists' general expectations of races. We expect racial histories to be messy, interweaving, and complex. Finally, this account of race helps explain why racial categories have not found greater use in science. They work well for certain predictions, but the number is very few given the number of traits humans possess.

1.3 On the Motivations for Awarding Merit Badges

Returning now to the *Studies* debate, Hochman has asked whether races meet the criteria for 'subspecies', claiming that this is the only condition under which races are biologically meaningful.[130] This utterly bizarre assumption has been critiqued by Quayshawn Spencer (2014) and Nevin Sesardic (2013) on the grounds that nobody defends the view that races *are* subspecies (most defend/critique racial naturalism).[131] One further comment is worth adding. Hochman's move is bizarre not only because the connection between 'race' and 'subspecies' are difficult to motivate, but also because 'subspecies' is a practical conceptual tool of biological science, not a principled metaphysical concept and not a measure of realism or objectivity. Subspecies pick out a specific type of cluster that is useful for (a small number of) modes of biological understanding.[132] That it is useful for understanding guarantees some objective similarity, but this cannot be the reason that Hochman focuses on it, for then he would have to concede that any use of races in science vindicate their naturalness. Why *only* subspecies? His response, that any other view is 'not substantive' or 'not natural' enough, fails to convince (Hochman 2014).

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[130] Hochman calls *this* view racial naturalism. Sesardic and Spencer call it a straw person.

[131] Darwin (1859) had thought that races might be subspecies, but debate has moved on since then. Andreason (1998) has an account of the epistemic utility of racial categories that appeals to cladistics and species/subspecies membership. Unlike Hochman she is explicitly pragmatic.

[132] As Mayr explains, in his 'Of what use are subspecies?': 'subspecies [is] not a concept of evolutionary biology but simply a handle of convenience for the clerical work of the museum curator' (Mayr 1982b, p.594). Along the same lines, David Hull (1998) explicitly argues that it does not matter whether races are subspecies because subspecies is an out-of-date pragmatic concept, not one that tells us anything of importance about the groups classified.

None of Hochman's critics seriously engage his claim about subspecies. I too will abstain. It is more fruitful to stay at the metaphilosophical level and ask why he thinks that some extra-strong criterion of meaningfulness is appropriate. We are limited here to speculation, but from the context it seems clear that Hochman imagines some connection between a highly biologically meaningful category and the appropriateness of that category for scientific research. Had Hochman concluded that, *because races fail to meet the criteria for subspecies they are therefore not subspecies*, there would be no issue and likely no response from Sesardic, Spencer, or myself. But Hochman thinks he has proved much more than this. He claims to have shown that races are not 'good categories to do science with' (2013b, p.285), that they are not 'scientifically respectable' categories (2013a, p.331), and are not 'biologically meaningful'. Given this, and because Hochman is demonstrably uninterested in scientific utility or biological objectivity, the only remaining option is to interpret him as holding race to an ontological-pragmatic standard, believing that subspecies status somehow establishes that races are sufficiently real to vindicate their use.[133]

As Lisa Gannett has lamented, this tendency to be unsatisfied with biological objectivity and to seek answers in ontology runs throughout the race debates:

> You might say that while scientists are about the 'real,' we philosophers are about the 'really real.' We assume we best contribute to debates about genetics and race by providing or withholding assent to the legitimacy of biological race concepts by metaphysical appeal to what is '*really* real'. (2010, pp.364–5)

Gannett is here complaining about enquiries focussed on natural kinds, but I think in light of the arguments presented here, it is fair to apply her point to 'biological meaningfulness', as well. She continues, speculating on the motivation behind these approaches:

> Attempting to capture basic motivations, we might say that theorizing about race as a natural kind is associated with the expectation—an expectation nonscientists share with scientists—that race as a category of classification furnishes an authoritative taxonomy, a taxonomy that by depicting fundamental divisions in nature is conducive to fulfilling far-ranging explanatory aims. (2010, p.376)

I think this is right, and it is this type of taxonomic authority that Hochman wishes to deny to racial categories.

In Chapters 1 *&* 2, I discussed several unmotivated assumptions about the role of natural kinds in scientific enquiry. These stem principally from supposed links between essences and characteristic properties of kinds and supposed levels of homogeneity in outward properties of kinds. Gannett (2010) explicitly chastises several discussions of race and natural kinds as inappropriately appealing to these types of assumptions. Her point is that there are more important issues to address. I agree, and would add that if scientific utility is the goal then

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[133] Spencer (2012) discusses a number of older discussions of race (pro and contra) that share this belief.

natural kinds are the wrong focus. Thinking about the present debate, concerning 'meaningfulness', appeal to ontological status is even more confusing. For, unlike kinds, there is no tradition in philosophy assuming a link between 'meaningfulness' and utility (erroneous or otherwise!). Here I think we see a deeper motivation, what Gannett calls 'appeal to what is *really* real". Hochman demonstrates the philosophical pre-occupation with determining what certain categories *really* are, under the assumption that this will tell us something about their scientific use.

In sum, future race debates should more carefully consider the purpose of debating the status of races, whatever that status might be. If the goal is to understand the use and role of racial categories in science, then we need a detailed analysis of the sorts of enquiries in which race features. Only then might we come to understand how much objective similarity races must exhibit to fulfil that role. Until such time as a requisite degree of objectivity, or similarity, or 'meaningfulness', has been established, we cannot stipulate the conditions required for racial categories to feature in science. Under no circumstances does it make sense to stipulate *a priori* what those conditions might be. If, on the other hand, the goal is to determine what type of category racial categories represent, then by all means we may ask whether races are natural kinds, species, sub-species, breeding populations, individuals, sets, universals, particulars, types, or tropes. But these enquiries should not be thought to have any implications for scientific classification (or ethics!) absent some compelling further argument linking ontological status to scientific validity.

I turn now to the second topic: evolved cognitive modules. There, too, philosophers and scientists have appealed to natural kinds as partial vindication of scientific utility.

2. Cognitive Modules

Does human cognition come in natural kinds? The human *brain* certainly does (it is full of chemicals, for instance). But what about *cognitive processes*?[134] Here the evidence is not so clear. Classes of cognitive phenomena are often classified functionally, like object recognition, choice, speech, or sensory perception. Functional kinds are useful for science, but will not survive metaphysical scrutiny. Yet we are also told that certain of these cognitive abilities have

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[134] There is an interesting overlap here with certain treatments of 'ontology'. Interesting philosophical and (later) psychological work has been done to determine which categories the human mind requires in order to make sense of the world. Though this tradition appears in Aristotle, contemporary versions date from Kant (1781), who offered the following categories: *quantity, quality, relation,* and *modality*. Kantian skepticism entails that these are necessary features of understanding rather than fundamental features of the world. However later ontologists, like Chisholm (1996), offer similar projects but claim to escape the conceptualism that plagued Kant, describing the real ontology of world by transcending human understanding. See (Thomasson 2013).

evolved. It is a cornerstone of Evolutionary Psychology (EP) that the contemporary human mind is composed of cognitive processes that date to our Pleistocene ancestors. Arguments in favour of these evolved cognitive 'modules' appeal to evolutionary conservation in a manner reminiscent of Kind Historicism. The claim is that the theory of evolution, applied to human psychology, yields the inescapable conclusion that our modern minds are comprised of pieces of minds past, which had very different ancestral functions in much the same way that modern organs (like eye lenses) are built from pieces of organs past (like digestive enzymes), which had very different ancestral functions.

As if anticipating Kind Historicism, the founders of EP describe modules as 'kinds invented by natural selection during the species' evolutionary history to produce adaptive ends in the species' natural environment' (Tooby & Cosmides 1995, p.xiii). Elsewhere they tie the scientific value of cognitive modules to the fact that they 'carve nature at its joints' (Cosmides & Tooby 1997). Other philosophers and cognitive scientists have also suggested that the scientific usefulness of cognitive modules derive in some way from their status as natural kinds (Fodor 1983, Gray 2001).[135] A hopeful proponent of EP might encounter Kind Historicism and think it an apt account for justifying the natural kind status (and utility), of cognitive modules. Of particular interest is the way in which historical essences are thought to have practical utility for EP; something I have argued is not the case with protein molecules, or, for that matter, with historical kinds in general.

In what follows, I will introduce cognitive modules and critically examine claims to their historical pedigree, arguing that they fail to be historical natural kinds. I will then show why cognitive modules need not possess this badge of ontological merit to justify a role in psychological science. Even if cognitive modules had no Darwinian histories at all, they could still be scientific kinds so long as they are robust and empirically identifiable. This entails however that we must leave the 'evolutionary' part of Evolutionary Psychology behind.

Again, there is much that must be bracketed in this discussion. I cannot discuss the dubious assumptions about human genetics used in EP (see Dupré 2008), the role of contemporary stereotypes in EP explanations (Fehr 2012), the problem of alternative explanations, and various criticisms that engage the non-evolutionary content of EP (Buller 2005 offers a comprehensive critique); and I will discuss only briefly the epistemic limitations EP faces.

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[135] For Fodor, natural kinds are those categories that can be bound variables in proper laws. As a result, he wonders whether all modules are a natural kind—which is not exactly the question I pursue here. See his (1974). Gray (2001) examines Fodor's (1983) claim, focusing on the shared properties of modules. Other philosophers have looked to the evolutionary construction argument to support claims about modular homologies (morphological similarities) as natural kinds (e.g.) Wagner (1996). I restrict my discussion to the natural kind aspirations of Leda Cosmides and John Tooby, the founders of Evolutionary Psychology.

2.1 The Case for Evolved (Cognitive) Modules

Several recent traditions in philosophy and psychology identify 'modules' in the mind and brain (Carruthers 2004, 2006, Cosmides & Tooby 1987, 1992, 2013, Fodor 1983), where 'modules' are isolable components.[136] I might have a decision-making module, a face-recognition module, a language acquisition module, and a geometric module. These modules will themselves consist of sub-routine modules, and may also be part of larger modules. On certain accounts, known as 'massive modularity' hypotheses, the mind is nothing but an enormous system of nested modules (Carruthers 2006, Pinker 1997, Cosmides & Tooby 1992).

The main philosophical argument for the existence of modules appeals to the piecemeal nature of evolutionary conservation.[137] Since evolution builds things by recycling and reissuing whatever parts are available, the argument goes, modern minds will likely have been compiled out of functional parts of old minds. These are the modules. The modular construction process is the same one that builds organisms out of bits of physiology developed for earlier organisms, or biochemicals using genes for other biochemicals.[138] This very general argument could be run for any evolved biological object. Evolution always works by modifying and re-arranging bits of all shapes and sizes, and when it comes to the mind or brain, we call (certain of) these 'modules'. The proponent of modularity imagines a mind built in much the same way as, for instance, the eye. Just as the lens of the human eye was fashioned from an older protein used in digestion and heat-shock response, so too is the cognitive system tasked with searching for my iPhone fashioned from older cognitive powers used in (e.g.) foraging and predator detection.

No movement is more closely associated with the evolutionary argument for modularity than EP. For EP, the mind is a massive set of conserved modules. EP starts with the claim that our Pleistocene ancestors, like humans of today, faced a set of reliably-recurring problem situations:

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[136] This tradition emerged with Jerry Fodor (1983), but there are now as many definitions of 'module' as there are module theorists. This diversity is not of present interest.

[137] There are also empirical arguments for their existence. Neurophysiologists may claim that neural architecture is set up in discrete isolated systems. Psychologists may claim that certain cognitive processes are isolated from others. Neither of these establishes the *evolutionary* nature of modules, only their contemporary existence. There are two additional *a priori* arguments for modularity. First is the claim that it is evolutionarily advantageous to construct minds out of modules rather than try to construct giant general-purpose algorithms. Second is the similar claim that it is computationally intractable to use one problem-solving strategy to solve multiple problems in real time (see Cosmides & Tooby 1987).

[138] A very good, if overlooked, account of the evolutionary argument for modularity can be found in (Clark 1987). Clark compares the inelegant process of evolutionary construction to overworked computer engineers, who 'kludge' together functional but inelegant solutions to the problems they face using whatever bits of code happen to be lying around.

> Our hunter-gatherer ancestors were, in effect, on a camping trip that lasted a lifetime, and they had to solve many different kinds of problems well to survive and reproduce under those conditions: hunting, evaluating plant resources, cooperating with others, avoiding predators, dividing resources among kin, selecting fertile mates, deterring sexual rivals, avoiding infectious diseases, detecting alliances, avoiding incest, learning grammar, negotiating dominance hierarchies, and managing aggression, for example. (Cosmides & Tooby 2013, p.203)

These problem situations are diverse. Citing this diversity and leveraging a computational tractability argument, EP theorists argue that no single problem-solving strategy would have suited all of the tasks involved in caveman life—or at least no single strategy will have done so well enough to avoid extinction. It is much more likely that early humans developed problem-specific cognitive strategies. These are the modular building blocks of contemporary minds.

> Evolutionary psychologists therefore expect (and find) that the human mind contains a large number of information-processing devices that are functionally specialized and therefore domain specific, with different devices activated by different kinds of content (snakes versus smiles, food versus mates, cues of social exchange versus cues of aggression). (Cosmides & Tooby 2013, p.204)

These modules are picked-up reordered and put to new uses in new situations, such that our present cognitive abilities are fashioned out of the original set of Pleistocene modules.

EP builds its research methodology on this foundation, claiming that we can gain insight into present cognitive processes by modelling them on the processes ancestors must have possessed to solve problems in the Pleistocene. Since it is from Pleistocene modules that our present cognitive processes are constructed, we should study cognition by de-constructing it into component modules—by carving it at nature's (and history's) joints.

Within EP the evolutionary argument is presented as pertaining to cognitive modules. The memorable claims of this field concern mechanisms like mate-choice, kin-detection, or object recognition. Less often do we read about conserved neurological structures. Yet EP must require commitment to neurobiological modules too since these are the vehicles for the evolution of cognition (Cosmides & Tooby 1992, see also Fodor 1983). The resulting view is of cognitive modules that supervene on neurobiological modules. This supervenience relation is our entry-point to a critique of cognitive modules as natural kinds.

Before discussing the link between cognition and the brain, a bit of terminology is in order. In what follows, I use the common metaphorical language of 'wiring' and 'networking' of the brain. Similarly, I will talk about cognition as 'programs' or 'algorithms' that 'run' on the brain. These computer metaphors are controversial, and rightfully so. They can mislead (see Carello et al. 1984). Here, however, it is convenient shorthand, which communicates the distinction between cognition and neurophysiology in (what I believe to be) a non-problematic way.[139] Neurophysiology is the wet-ware. It is partly inherited. It is physical. Cognition is the software. If it is at all inherited then it is inherited to some degree via neurophysiology. It is not physical. It 'runs' on the neurophysiology. Sticking with this language emphasises the way in which cognition must be enacted through neurophysiology.

2.2 Neural Plasticity and the Evolution of Cognition

What does the argument from piecemeal design prove? According to modularity theorists, it proves that bits and pieces of early modules will be recycled into present day modules. Bits of what, exactly? According to EP, it proves that *cognitive* bits and pieces will be recycled. It is not immediately clear that this is true. The argument from design certainly suggests that bits of brain structure get used and re-used, but whether those bits support bits of cognition is a big question. Evolution does not work directly at the non-physical cognitive level. This makes the claim about cognitive modules a more difficult one to prove.

Like proteins and organisms, brains will be composed of many historical kinds. There will be molecular historical kinds, like neurotransmitters; there will be cellular historical kinds, like neurons. Neurophysiologists also describe conserved aggregates of these. The brain is a nested hierarchy of historical kinds. All of these will be used as parts from which to build new adaptations at higher levels. Though the module theorist may claim that whatever pieces get reused and recycled are 'modules', this view strips 'module' of its meaning—they would not want neurons, cells, or proteins to count as 'modules'. Nevertheless this line of reasoning supports the claim that the brain *may* contain some neurobiological things of the size and scale described by philosophers and neuroscientists under the label 'modules'. If so, then those physical modules would be historical natural kinds. But these are not the cognitive kinds required by EP.

Nowhere in the brain will we see conserved *cognitive* modules. Cognitive algorithms only emerge when the brain interacts with the world. Yet, assuming physicalism about mental events, we would still want to say that cognitive algorithms are (at least partly) in the brain. What then would it mean for a cognitive algorithm to be Darwinian? Early EP assumed that the evolution of a neurobiological module just was the evolution of an associated cognitive module (Cosmides & Tooby 1992) but the widespread phenomena of neural plasticity complicates this assumption.[140] I will now explain why neural plasticity precludes cognitive modules as historical kinds.

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[139] In other work, I have used this distinction between levels of description in cognitive science to highlight the empirical gulf between cognition and neurophysiology. If the reader doubts the analytic utility of separating cognitive algorithms from neurobiological realizers, see (Bartol & Linquist 2015)

[140] Some authors have been overzealous in taking plasticity to disprove EP and modularity hypotheses. According to Buller and Hardcastle (2000), developmental plasticity is the brain's general-purpose

Neural plasticity is the widespread and well-supported claim that the architecture of the brain changes during the life of an organism in response to various stimuli. Plasticity comes in a variety of shapes and sizes. Some occurs at the cellular level, as new synaptic connections are formed in response to learning. Some occurs at the level of individual cortices, as areas are enlarged in response to excessive stimuli. Some occurs at the whole-brain level, as entire systems are disconnected and re-wired. A few examples should suffice.

Much of the work on plasticity comes from cases of injury, where the loss of certain bodily functions can have a profound and lasting impact on our neural architecture. Perhaps none so noticeable as the effect of motor control on the brain. Repeat stimulation of particular motor regions ultimately leads to enlargement of those areas. But loss of stimulation is even more interesting. When the motor region controlling a particular part of the body loses its signal (say from amputation) that region very quickly quiets down. Remarkably, however, it soon begins to function anew, receiving stimulus from other nearby regions of the body. In monkeys, it was found that cortical regions associated with the motor control of an amputated digit quickly re-emerged, receiving input from areas associated with intact adjacent digits (Kaas 1991).

Plasticity is also responsible for alteration in the way in which certain tasks are performed. A much-publicised research project recently found that heavy users of marijuana engage an atypical pattern of brain systems in order to complete working memory tasks (Kanayama et al. 2004). It is well known that marijuana use impairs synaptic activity in brain regions associated with spatial reasoning and working memory.[141] It appears that heavy users of marijuana eventually 're-route' these tasks to other areas of the brain in order to supplement the impaired regions. They perform working memory tasks equally well as controls but do so using a different neurophysiological strategy.[142]

These and other findings show that the cortical regions were always 'wired' to receive a range of different signals and perform a range of different tasks. Those regions simply do not perform many such tasks until needed. The presumption is that conditioning 'assigns' certain regions to certain functions, but that functions can be subsequently re-assigned to any qualified bit of neural anatomy—and most of your neural anatomy is overqualified. In this regard, neural plasticity is frequently associated with neurological development. The

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problem solver, and so plasticity fashions neural regions on-the-fly during development, obviating the need for conserved modules. This line of attack, though popular, misses the mark. A 'general purpose problem solver', like plasticity, does not eliminate the possibility of specialised cognitive modules. Like a good operations manager, plasticity might 'wire in' one of many possible specialist cognitive modules. See (Dellarosa Cummins & Cummins 1999).

[141] The prefrontal cortex and anterior cingulate.

[142] Investigators did not seek to determine whether the cognitive process engaged in the task changed as the neurophysiological strategy changed.

developing brain in children can be viewed as a plastic system that gradually builds functional neuro-anatomy in response to stimuli. There are many different ways in which brains control bodies and execute cognitive functions. Which ways our brains do this is partly a matter of conditioning.

Plasticity shows that the link between biological modules and cognitive algorithms is complex. Functionally equivalent cognitive algorithms can supervene on different biological systems. Cognitive demands can alter biological systems. The environment can bring about changes in biological mechanisms via cognitive ones, or it can alter cognitive algorithms without changing their biological realizers. There is no simple characterization of the relationship between the cognitive and the biological. This is precisely the issue. Present-day cognitive algorithms will supervene on various bits of neurophysiological machinery, but these bits are cobbled together by plastic cognitive procedures, not by evolution. There is no reason to suspect that cognitive algorithms will respect the borders of any conserved neurobiology, or that a given module will always support the same cognitive function.[143]

To say that cavemen had cognitive systems that we have re-purposed is misleading. Cavemen had certain brain structures that developed during their lifetimes in response to local and developmental stimuli, simultaneously shaping and being shaped by cognitive procedures suited to the problem scenarios of caveman life. Contemporary 'descendants' of those brain structures are further modified during our lifetimes, shaping and being shaped by cognitive procedures suited to the problem scenarios of contemporary life.[144] As philosopher Chuck Ward (2012) explains, 'the functional architecture of our brains (and so our minds) is in significant part a product of fairly recent cultural development, [and so] those features are not adaptations to the environment of our Pleistocene ancestors' (p.21).

We can think of cognition and neurophysiology the same way we think of molecular-physiological function and the biochemicals, collections of structures, etc., that execute that function. In Chapter 4, we learned that structurally and historically different proteins can carry out the same functions, just as structurally and historically kindred proteins can carry out distinct functions. There is no necessary link between structural or historical similarity (or identity) and function. Functions were described as dispositional properties of kinds, the

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[143] The criticism I am offering is compatible with that offered by Dupré (2008), who appeals to the developmental plasticity of human beings in order to criticize the notion that our brains are directly inherited from Pleistocene brains. Dupré's point is that contemporary understanding of molecular evolution (and heredity) suggests that our physiology is in larger part a response to recent development than Evolutionary Psychologists suggest. The molecular mechanisms Dupré discusses are among those thought to lie behind intra-generational neural plasticity.

[144] In fact, it is more complicated even than that. Brain structures are not inherited, the genes partly specifying them are. Enter all of the complications surrounding genetic expression (Dupré 2008). I have simplified the matter here in a way that shortens the prose, but should not harm my point.

realization of which is highly contingent on context and development. Functions thus do not demarcate natural kinds, nor are they necessary properties of natural kinds; function and kind membership are independent. This account works for modules, too. Plasticity tells us that the same bits of human brain can sub-serve different cognitive functions, just as two different bits of human brain can subserve the same cognitive function. Cognitive algorithms are properties of vast networks of neurobiological assemblies. These properties arise only for specific network arrangements, though not for singularly specific network arrangements. They are contingent not only on the presence of the arrangement by which they are enacted, but also on the developmental and environmental contexts that occasion the arrangement in the first place, via plasticity. Cognitive software and neurobiological hardware develop together during the lifetime of the organism. They are no more natural kinds than any developed property of an individual is a natural kind.

2.3 The Fate of Evolutionary Psychology

A lot hangs on claims about cavemen. Though some EP theorists are more interested in the idea that the mind contains isolatable components, and so make less reference to the Pleistocene (e.g. Carruthers 2004), all EP theorists rely heavily on the evolutionary argument. Cavemen claims are only supported by the evolutionary argument[145], and so it is these claims that have to go. Where does this leave EP? Modularity theorists must give up the idea that cognitive algorithms are the historically conserved pieces from which present minds are constructed. However this does not entail that cognition cannot be carved up into isolatable pieces and grouped according to functional equivalence. Natural kinds are off the table, but induction-supporting kinds are not. In this final section, I will sketch an argument for the possibility of cognitive modules as induction-supporting kinds.

The fact that cognitive algorithms are not natural kinds does not tell us anything about their usefulness. It is hard not to notice that, in spite of plasticity and variance, cognitive algorithms in the population do exhibit a certain clumpiness. This is no accident. The problem-solving environment faced by contemporary humans presents certain reliable problems, which admit of a varied but finite number of solutions. If the solutions are finite and if their number is manageable, then cognitive algorithms may well form useful similarity-based groupings, or ‘induction-supporting’ kinds, of the sort described in Chapter 1.

What I am describing here is a form of ‘canalisation’ applied to cognition. Canalisation refers to the way in which certain phenotypes exhibit robustness in the face of genotypic and

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[145] Other claims about modules, such as being isolable and domain specific, draw support from additional empirical evidence.

developmental variation.[146] The variety of possible starting conditions and developmental events behind phenotypes are not necessarily reflected in an equal variety of end points. The same phenotype can result from a number of developmental origins.

We can expect canalisation of cognition for two reasons, or because of two constraints. First, a cognitive algorithm must be able to execute whatever task it is for. Though many possible algorithms can solve a given problem, that number is still finite. Second, an algorithm must run on a human brain. Powerful though our brains are, these physical systems impose an obvious constraint: We cannot run a cognitive program for which we lack compatible wet-ware.

Consider a simple example of the first constraint. Here is how I perform multiplication involving the number nine:

$$9n=((n-1)*10) + (10-n)$$

I do not know why I do it this way, but I do; and it works. This is how I started multiplying by nine as a child, and I have never grown out of it, in spite of the knowledge that there are easier ways to do it. My algorithm does not work particularly efficiently, especially once the numbers get higher than ten, but it is reliable enough to have survived. Few of my friends perform the operation this way. Most have memorized the multiplication tables up to a point and perform a simple recall. Others use a more intuitive combination of multiplication and subtraction [(n*10)-n]. These are the only options; everyone will use one of these processes. As long as the algorithm must solve the problem of multiplication by nine, it is limited to a few possible forms.[147] The same is true of any cognitive process; it can take only as many different forms as will reliably execute its function.

These teleological constraints imposed on algorithmic operations are a logical imposition, and there is a biological analogue to these constraints, which is more in line with traditional applications of the term 'canalisation'. There are only so many possible organic structures that will address certain adaptive challenges. Limbs, body plans, and sensory organs each evolved multiple times separately, with minor variations, because these are good solutions to certain adaptive challenges, and, for many phylogenetic groups, they are the only options available given inherited biology. We can apply this same thought to the functional set-up of the brain.

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[146] The concept of canalisation comes from Waddington (1942). He used the concept to describe the apparent robustness of certain phenotypes against environmental and genetic perturbation. For Waddington, canalisation is a result of selection. I am here stretching the initial concept, which is entirely biological, to add the teleological algorithmic constraint.

[147] On a fine enough grain the number of possible strategies might be much larger. The simple process of [(n*10)-n] might break down further, since it does not actually eliminate multiplication. '(n*10)' might become (10+10+10…), and so on. This does not seem to matter for the creation of similarity-based groups, since most of the generalizations that hold for the compact algorithm also hold for the broken-down algorithm. But it is an interesting question how much of this type of variation might exist within cognitive algorithms and the degree to which it matters for psychology.

Applied to a plastic brain, there are only so many 'wiring' set-ups that will support the sorts of cognition that the environment demands of us. Plasticity may be able to route cognition via many channels, but not just *any* channels. This means that not only will cognitive algorithms be limited to those that can execute the requisite function, but *that* set is further limited to those that can run on available (or possible) neurophysiology.

Combined, the algorithmic and biological constrains make the case for a sort of canalisation of cognitive processes.[148] Canalisation of traits positively correlates with selection pressure. So we might expect the number of possible cognitive processes for at least some problem-scenarios to be rather small. There are only so many processes by which to spot kin, identify faces, or represent geometry. And some further sub-set of these possible processes is such that a given human mind can execute it. The possible workable and reasonably efficient 'modules' are likely few enough to permit some recurrence of cognitive strategies in the population and recurring cognitive strategies are excellent categories for scientific enquiry.

To be clear, the possibility of robust similarity-based groupings is not challenged by neurobiological variation underneath cognitive equivalence. Canalisation is a measure of robustness against variation, including variation in neural hardware. Even if equivalent cognitive algorithms run using different neurophysiological strategies, this should not prevent researchers from lumping those cognitive algorithms together for the purpose of explanations and generalizations at the level of cognition. Canalisation predicts classes of functional equivalence, not physical identicalness. Functional equivalence is not enough to establish historical kinds or other natural kinds, but it certainly is enough to support induction and explanation.[149]

This explains some of the empirical success claimed by EP without vindicating any of the claims about cavemen.[150] On the received approach, EP successfully identifies cognitive algorithms in the modern population and uses this finding to vindicate the central EP belief in cavemen cognitive modules. That belief justifies the search for those algorithms in the first place and in turn frames Panglossian explanations about why an algorithm happens to run this way or that. On the canalisation and similarity-based kinds view just proposed, the cavemen claims of EP are justifying things that do not require justification and explaining

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[148] Notice that this is very different from Ariew's (2006) attempt to identify innateness with canalization. He describes innate psychological traits as the realization of canalized neurophysiology. This is only the biological constraint, not the algorithmic. Also, I make no claims about 'innateness' as I view the concept as unhelpful. See (Griffiths & Machery 2008) on Ariew and (Linquist et al. 2011) on the concept of innateness.

[149] Mistakes enter the frame only when scientists attempt to make generalisations about neural architecture using groupings of cognitive algorithms, for then we would hide neurobiological variation underneath cognitive similarity. So long as 1:1 reduction from cognitive algorithm to neurobiological realiser is resisted, such mistakes will be avoided.

[150] See Cosmides & Tooby 2013 for a list of EP success stories

things that do not require explanation. Why does the algorithm exist? Because there is a problem in the world that it addresses or solves. Why does it take the form that it takes? Because it settled within a space of possibilities imposed by the problem it solves and the biology by which it is executed. If a further story needs to be told then we can appeal to the history of the agent in question to explain why the algorithm settled on a particular solution within that constrained space of possibilities—but this story is going to be practically inaccessible and has nothing whatsoever to do with the Pleistocene. The same can be said of appeals to cognitive modules as natural kinds. Scientific success with modules does not entail that they represent joints of nature enduring from humans past; they need only to be robustly and objectively similar.

How then does the EP theorist generate hypotheses about candidate modules? Cognition is terribly messy. It is difficult to pull cognition apart into isolatable chunks in such a way that does not do violence to the cognitive phenomena being described. EP methodology suggests a way to circumvent this problem. Rather than starting with present-day cognition, they think about what simple adaptive problems might have been faced by our ancestors and then search for those mechanisms in present-day human minds. This methodology is untenable in view of the arguments offered here. A new mode of hypothesis generation must be found. In replacement I can offer only old-fashioned empirical cognitive psychology, aimed at pulling apart problem-solving operations into their component parts.

3. Final Thoughts on Races, Cognition, and Induction

In this chapter I have discussed two cases in science, which, *prima facie*, are hospitable to a Kind Historicist account of natural kinds. My goal was to forestall such applications of my theory not because I believe races and cognitive modules are not natural kinds, but because I think that natural kind status is irrelevant in these cases. I hope to have shown how worries about the ontological status of scientific classifications can lead astray. To avoid this, philosophers and scientists must critically evaluate the ontological status they seek, be it 'natural', 'meaningful', 'individual', 'natural kind', etc., and ask after links between that status and scientific utility. If no such link exists, then it is time to re-evaluate our philosophical projects. Another way to phrase this message is this: 'What follows from the fact that thing *X* has (or lacks) status *N*? Often, the only claim such a discovery licences is the claim that '*X* is (or is not) *N*'. If we want to pass moral or scientific judgement on *X*, we need some link from *N* to science or ethics. Such links are not easily found.

A naturalised metaphysics of biology is not helpful in providing kinds for scientific investigation. For certain philosophers of science, this may appear to invalidate my project altogether. But that would be hasty. A naturalised metaphysics of biological kinds is helpful for understanding philosophical observations such as intrinsic heterogeneity, theoretical pluralism, and the general inapplicability of standard metaphysics of science to biology. And so it is to these philosophical issues—often called ‘biological exceptionalism’—to which I now turn in my concluding discussion.

Conclusion

I promised at the outset that Kind Historicism would shed some light on biological exceptionalism. It is now time to deliver on that promise. Doing so should serve as a helpful recapitulation of my position while also pointing toward future work and highlighting implications for philosophy of biology and metaphysics, yet unexpounded. A secondary goal of this discussion is to leverage my position, Kind Historicism, over a family of broadly pluralist approaches to biological kinds—versions of which appeared in Chapters 1, 3, and 4—by showing how Kind Historicism provides a superior account of biological exceptionalism (henceforth simply 'exceptionalism').

It was talk of exceptionalism that first piqued my interest in the metaphysics and ontology of biology[151], however I must confess that, at the time, I had only a vague sense of what the term meant. I was perhaps not alone. Little has been written about exceptionalism.[152] The term is used in connection with the natural kind problem, intrinsic heterogeneity, theoretical

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[151] During my MA at the University of Guelph, (which until then was supposed to be all about political philosophy) in a room full of philosophers and biologists, there was a sense that biology was somehow different from the other sciences. This sense grew as the semester wore on, fuelled by the frequent juxtaposition of biological and non-biological treatments of kinds, classification, laws, and causation. After a project on the gene and its causal powers (see Bartol 2013a) I began the present investigation into kinds.

[152] A Google N-gram of 'Biological Exceptionalism' returns no results (searching books between 1800-2015). A JSTOR search returns a handful of results mentioning 'exceptionalism' in legal or moral contexts. An informal survey of colleagues supports the interpretation I use here.

pluralism, and the problem of laws in biology. Now, when colleagues mention 'exceptionalism', I take them to refer broadly to the fact that *biology is somehow interestingly and importantly different from non-biology.*[153] I submit that Kind Historicism and the conceptual framework introduced to support it constitute a vindication of this sentiment. There are thus two tasks required of my account. The first (and simpler) task is to explain what the word 'biology' means in this statement of exceptionalism. 'Biology' might be sciences; it might be the world; it might be both. The second task is more difficult; it involves explaining what it might mean for biology to be 'interestingly and importantly different'.

To begin, I will interpret the 'biology' in exceptionalism as both the biological world and the sciences that study it. There are exceptional elements to both, and they should now be familiar. On the side of the biological world is *intrinsic heterogeneity*. This thesis has discussed at length the ways in which biological objects are broadly heterogeneous, and the problems this causes for certain metaphysical enquiries. Classes of non-biological objects are not so troublesome. On the side of the sciences is *theoretical pluralism*. It is certainly an unusual feature of biology that scientists are so overtly pluralistic with their theories of classification. I have not yet discussed the way in which biologists are pluralistic about causal claims and generalisations but this, too, is a form of theoretical pluralism and thus a fact to be accounted for as part of exceptionalism. Theoretical pluralism and intrinsic heterogeneity describe ways in which biology is interestingly and importantly different from non-biology.

Kind Historicism predicts intrinsic heterogeneity as an empirical consequence of the metaphysics of biological kinds. Intrinsic heterogeneity in turn precludes the use of biological kinds as induction-supporting kinds. This necessitates theoretical pluralism. I will make this argument first in the context of kinds and classification before suggesting how the argument might be extended to laws and generalisations.

This thesis has been about the metaphysics and ontology of biology, not biological practice. I have discussed natural kinds at the expense of induction-supporting kinds and I have investigated intrinsic heterogeneity at the expense of theoretical pluralism. It is now time to bring together insights from around the thesis in order to situate Kind Historicism, a thesis about the metaphysics of biological kinds, relative to biological exceptionalism, theoretical pluralism, and the induction-supporting kinds of the biological sciences.

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[153] Exceptionalism has an ethical version, according to which things made from biology (e.g. cells) are on a different moral plane than things made from non-biology (e.g. chemicals). This is not what I mean, here.

1. Biological Exceptionalism

The term 'biology' in biological exceptionalism might refer either to a slice of the world or to the sciences that study that slice. To determine which of these meanings is best suited to exceptionalism, we should ask whether any deeply interesting or important differences might be picked out by exceptionalism under each possibility. Many use 'biology' in the latter sense, referring to the collected biological sciences. Not only are these different from the chemical or physical sciences, they are also different from one another. An observer might even wonder what connects the biological sciences at all, given their diverse subject matter, methods, and aims. If exceptionalism uses 'biology' in this sense, then it either identifies exceptional practices as interesting in their own right, perhaps for sociological reasons, or it identifies exceptional practices as interesting because they reflect exceptional features of the world with which biological scientists must grapple. If it is only the former then exceptionalism is philosophically uninteresting. The latter is more promising. An account of exceptionalism should illuminate exceptional practices by showing how they relate to exceptional features of the biological world.

Biologists do many things differently—from the rest of science and from each other. We have seen already how biological classification and taxonomy vary internally and are different from classification and individuation in the physical sciences. This is 'theoretical pluralism'. Philosophers of biology have also noted the ways in which causal claims (or ascriptions of laws) are different in biological contexts (Brandon 1997, Hamilton 2007, Haufe 2013, Mitchell 2000, Woodward 2001); this is a theoretical pluralism of a different stripe, which I will discuss shortly. These practices are 'exceptional', in the literal sense of the word, but there must be more to exceptionalism than this. For suppose that biologists are just bad at taxonomizing, individuating, and finding causes. If they were doing this poorly compared to physical scientists, then focussing on these differences would not be subject matter for a philosopher, but a psychologist or sociologist.[154] Many would claim, of course, that biologists are not wrong, merely different. Perhaps the epistemic culture of biology has channelled the practices in one direction while the epistemic cultures of the physical sciences have gone a different route. One might talk about the types of people that go into these sciences, or about the relatively late popularity of biology as a science, or about the perception of biology as a 'softer' science than physics or chemistry. There are many facts that might have led biologists to develop unique ways of doing things. Again, however, if it is only these sorts of facts that make up exceptionalism then exceptionalism is not a topic for philosophy of science. It may

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[154] Branches of philosophy such as social epistemology might have something to say about this. My point here is simply that philosophers of science interested in biological exceptionalism would be wrong to look primarily at practices for this reason.

be interesting to sociologists of science, psychologists, and even to social epistemologists, but this version of exceptionalism does not help us understand the unique challenges the biological world poses to those who study it. I suspect there are more to these differences than socio-historical factors.

Exceptional biological practices are necessary features of any biological science, not just contingent features of the current epistemic and social landscape of present biological science. Biological practices *must* be exceptional because the biological world demands it. Exceptional practices reflect an exceptional world. The point of this concluding discussion is to outline how biological kinds make the metaphysics of the biological world exceptional, and how that exceptionality demands theoretical pluralism as a feature of biological science. I begin with the biological world before discussing its scientific investigation.

1.1 Intrinsic Heterogeneity and Kind Historicism

Philosophers of biology have obsessed over classification and kinds in the biological world. One thing is nearly-universally agreed: The natural kind approaches from physico-chemical kinds do not work for biology. These approaches anchor kindhood in shared physical properties, but biological objects lack such properties. This is intrinsic heterogeneity. It is a major reason why we think biology is exceptional.

In this section I will consider two responses to this state of affairs. First is a broadly pluralist approach to biological kinds; second is Kind Historicism. While both putative accounts point to a metaphysical difference between the biological and non-biological sciences, kind historicism has the additional virtue of explaining the intrinsic heterogeneity of biological classes.

Most discussions of natural kinds assume that particulars are kindred in virtue of shared physical properties, called essences. While traditional ontological treatments of kinds do indeed require shared essences, the idea that those essences must be physical properties intrinsic to the particulars is unmotivated. As I suggested in Chapter 2, the intrinsic physical essence is perhaps a coincidental feature of the stock examples of natural kinds: physico-chemical kinds. Simple chemical kinds, like gold, and simple physical kinds, like electrons, have shared physical structures in virtue of which certain atoms are gold atoms and certain particles are electron particles. This view is often called ‘microstructuralism’, or ‘microstructural essentialism’. But microstructural essentialism is not the only essentialism. The kind historicist recognizes that essences need not be physical. According to Kind Historicism, particulars can be members of kinds in virtue of *historical* essences.

I have discussed at length the widespread heterogeneity in the biological world. Within-class heterogeneity among the properties of biological particulars has led philosophers of biology to all-but abandon hope for a microstructuralist account of biological kinds. All physical properties of biological things are liable to vary, and therefore no set of essential physical properties will be found to unify kinds. This is how microstructural essentialism fails in biology. There are two responses to this failure.[155] First, we might adopt some brand of pluralism about biological kinds, finding inspiration in the plurality of taxonomic systems developed by biologists in the face of heterogeneity. The second option is Kind Historicism.[156]

There are many pluralist options that fall within the scope of the first response. What they have in common is a general appeal to the many objective and non-arbitrary taxonomic systems in biology as possible natural kinds. Call these all 'biological pluralist' accounts. Responses within this tradition will always be categorically pluralist, supporting multiple types of kind category. Any of the diverse approaches to taxonomizing species, for instance, are seen by the biological pluralist to offer (potentially) unique kind categories. Responses within this tradition may also be taxonomically pluralist, believing that self-same biological particulars are members of distinct kinds. One might believe that a single organism is a member of one kind because of interbreeding abilities, another because of morphological similarity, and yet another because of genetic similarity. Biological pluralism is a metaphysics of biology that interprets the disunity manifest in theoretical pluralism as a metaphysical disunity.

One virtue of biological pluralism is that it points to a metaphysical difference between the biological and non-biological world. Where the non-biological kinds admit of category and taxonomy monism, the biological kinds admit of category and (on some views) taxonomy pluralism. But we should not take this to be an account of biological exceptionalism. I have argued that approaches within this tradition should be viewed as offering realist induction-supporting kinds, not natural kinds (Chapter 1). This should soften the temptation to view biological pluralism as a sufficiently 'deep' metaphysical account of exceptionalism. Here I offer a separate point for consideration: The biological pluralist accommodates intrinsic heterogeneity but does not explain it and, as a result, offers a comparatively weaker account of biological exceptionalism.

Heterogeneity is not a consequence of the biological pluralist view, it is a feature of the biological world with which the view fails to conflict. Biologists have found ways around intrinsic heterogeneity, identifying shared features and properties of organisms that provide workable taxonomic distinctions. But these properties work as handles for taxonomy (and thus for kinds) only for circumscribed sub-sets of the sum total of biological particulars. The

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[155] Setting to one side nihilist and constuctivist responses.

[156] Kind Historicism is also pluralist, of course, but it is pluralist across biological and non-biological. Within biology Kind Historicism is monist, whereas this broadly pluralist option is not.

biological pluralist endorses some or all of these as natural kinds, and in so doing accommodates intrinsic heterogeneity. That is not the same as accounting for it, which would require that heterogeneity is an excepted outcome of the position. Suppose we asked why biology is characterised by intrinsic heterogeneity. At best, the biological pluralist can say that the biological world is heterogeneous because its kinds are heterogeneous. This is an embodiment of heterogeneity, not an account of it. Consider by contrast the following account of how intrinsic heterogeneity is a consequence of Kind Historicism.

Kind Historicism predicts heterogeneity as a consequence of the nature of biological kinds. When kindred particulars share a history, they do not instantiate the same property. Rather, they bear a set of relations to one another. They cannot instantiate the same property because the nature of individual histories precludes co-instantiation. The history of one particular will necessarily be different from the history of any other, for to have the *exact* same history is to be the same token particular, rather than a kindred pair thereof. For any set of historically related particulars, there will be some divergence in individual history, even if only moments before conception/synthesis/mitosis/etc. These differences in history underpin differences in particulars. In Chapter 3 I developed this point with reference to lemons. Kindred particulars will, at the very least, develop separately. Individual developmental differences will result in different properties. Think of identical twins, whose history diverges only from conception onward. Genetic and epigenetic differences at conception compounded by environmental and developmental differences over their lifetimes result in organisms that have many unique properties, in spite of being closely historically related. Most kindred particulars will diverge further back in their histories, measured in generations. Divergence over generations also results in differences in properties. Thus members of the same branch of a family differ immensely. In each instance, the nature of the biological kind category predicts heterogeneity. Insofar as biological kinds are fundamentally historical, they are necessarily heterogeneous.[157]

Notice that the same type of account also explains why widespread heterogeneity is *not* a feature of non-biological kinds. In microstructural kinds, kindhood is anchored by a static physical property (or set). Physical properties, unlike historical relations, can be instantiated by multiple particulars. Since the causal relationships in which physical properties are engaged are governed by invariant physical laws, identical essences will participate in identical causal interactions yielding identical properties. Even if the causal features of the essence are

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[157] Some may think 'necessity' is too strong. One might imagine a possible world in which natural genetic variation is absent, as is variation in the environment. In such a world biological kinds would be homogenous. But that world is not our own; nor is it one containing biology in the first place, since it lacks the conditions for evolution.

dispositional, the fact that the essence itself does not change means that it will reliably support the same effects in the same conditions.[158] So, just as an account of the features of biological kinds predicts intrinsic biological heterogeneity, that same type of account predicts the relative homogeneity of physico-chemical kinds.

I submit that this explanation offered by Kind Historicism is more substantive than that offered by the biological pluralist. While Kind Historicism predicts intrinsic heterogeneity, the pluralist can only cite heterogeneity as a feature of her view. The pluralist observes heterogeneity as empirical reality and builds a view around it, believing that intrinsic heterogeneity indicates a (metaphysical) heterogeneity of biological kinds. The Kind Historicist, by contrast, offers an account of kinds that predicts heterogeneity in biological properties. This link from Kind Historicism to intrinsic heterogeneity extends through to theoretical pluralism, since theoretical pluralism is a response to intrinsic heterogeneity. The biological pluralist might think that it is a virtue of her account that it aligns with the kinds used in biological practice, reflecting theoretical pluralism. However this is no place for natural kinds; induction-supporting kinds are the appropriate tool. I have said little about how induction-supporting kinds actually arise in biology, so this next section will sketch just such an account.

1.2 Theoretical Pluralism and Kind Historicism

The motivation behind biologists' pluralistic approach to taxonomy is not metaphysical; biologists are not offering an implicit metaphysical thesis about the unity or disunity of biological kinds. They have little interest in addressing issues of metaphysical monism or pluralism, whether categorical or taxonomic. Rather, biologists are pragmatically pluralist about their classifications because that is the best way to get stable groups over which inductions and generalisations can range. Historical kinds cannot do this; enter pragmatically tailored induction-supporting kinds.

I noted in Chapter 3 that microstructural kinds provide chemistry with induction-supporting kinds. For reasons just outlined, simple microstructural kinds are highly-similar under similar conditions. This makes them ideal candidates for inductions and explanations, and so the chemical elements lie at the heart of chemical classification. The same is not true of historical kinds and so biologists need to find ways of erecting kinds that support their inductions. But how? At first blush, we might think that scientists should simply group particulars using whatever properties they want their inductions to engage. If we want to talk

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[158] It is still possible for non-biological kinds to differ. Any 'accidental' properties not related to the microstructural essence may vary. We tend to forget this since most examples of microstructural kinds are described such that all of their obvious properties are direct results of their essential microstructure, lacking any 'accidental' properties.

about body plans then make groups based on body plans. If we want to talk about locomotive strategies then make groups based on mechanisms of locomotion. If we want to talk about ecological niches then make groups based on niche. This approach quickly falls apart. There would be a combinatorial explosion of non-translatable kind classifications, equal to the number of properties mentioned in inductions. Claims about biological kinds would always be relative to a classification system in which they were made, in which claims of only that type (e.g. morphological, phylogenetic) hold true. This would not facilitate understanding of the biological world. As Hacking (1993) has argued, taxonomic systems relativized to very specific enquiries would result in Kuhnian incommensurability. Knowledge about the kinds would not be gained because there would be no way of translating knowledge from one enquiry, using its taxonomy, into the taxonomic system of another enquiry.[159]

Instead of adopting taxonomies for each shared property, scientists seek a smaller number of classifications that work in slightly broader investigative contexts. These classifications are based on proxies. By 'proxy' I mean some single property or property set that stands in as a good guarantor of other shared properties of scientific interest. In Chapter 3 I discussed how Millikan (1999a) and Griffiths (1999) make a compelling case for using phylogeny as a proxy for the classification of organisms. Interbreeding, too, provides a good proxy for other similarities between organisms, as does morphology. These properties, when shared, tend to indicate the presence of other shared properties over which generalizations might range. However the fidelity with which their presence indicates other shared features depends on the organism, environment, and supporting conditions in question. Genetic similarity may guarantee morphological similarity in some contexts but not all. Interbreeding ability may guarantee certain genetic similarities in some types of organism but not others. It all depends on the details. Different proxies will better suit different investigations and different groups of biological particulars.[160] Thus biologists studying plants, algae, and fungi tend to use a different taxonomic approach from those engaged in zoological classification. Part of the task of biological taxonomy is determining the scope of applicability (or usefulness) of any given scheme.

This pluralistic and selective use of proximate classification systems is a 'theoretical pluralism' regarding taxonomy. Multiple distinct representations of the taxonomy are required to capture all of the similarity/dissimilarity relations in which biologists might be

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[159] Hacking is attempting to prove here that taxonomies of natural kinds must be hierarchical. I have reservations about his argument. We should notice that in many sciences, non-translatable taxonomies are still such that investigators can transfer knowledge, since there are sufficient overlaps. Hacking seems to imagine radically distinct taxonomic systems, rather than the partially distinct systems of (e.g.) species classifications. See Khalidi (1998) for discussion.

[160] It is an interesting sociological and psychological question as to how scientists navigate knowledge transfer from one system to another. They nevertheless seem to do just that.

interested. Kind Historicism explains why this is necessary (see above); but why does it work? Why do certain proxy handles for taxonomy reliably indicate the presence of other properties?

It is here that the utility of a robust theory of induction-supporting kinds comes to the foreground. Consider HPC theory, introduced in Chapter 1. According to HPC, these proximate kinds work, when they do, because the proxy property is a reliable indicator of certain stable causal structures in the world that give rise to some set of shared properties. Following Griffiths (1999) and Millikan (1999a), for example, we can explain that phylogeny works as a system of induction-supporting kinds because evolutionary relatedness guarantees the presence of certain causal mechanisms, which ensure some measure of similarity among kindred particulars. This is Griffiths' 'phylogenetic inertia' (see Chapter 3).

Notice that this account says nothing about *natural* kinds. Kind Historicism has nothing to say about why pluralistic induction-supporting kinds work, because these kinds operate independently of whatever the natural kinds are. One might think it a virtue of the pluralist account of natural kinds that it endorses (some) of these induction-supporting kinds as *natural* kinds. Quite apart from worries about the meaning of 'natural kind' (Chapter 1), we can ask whether this endorsement amounts to an explanation of inductive success. It does not. At best, the biological pluralist can claim that certain kinds from biological science support induction because they are natural kinds, but then we are left wondering why these kinds do not *always* support inductions. If that worry is answered with reference to the imperfect likeness relations among biological kinds, then we are back at intrinsic heterogeneity, of which the pluralist has no internal account. A better account of induction makes reference to the relatively stable features of the world that support inductions and the connection between the proxy classification and those causal structures. In this regard I think the HPC approach is most helpful.

As a final point, we should not forget that supplementary classifications exist outside of biology, too. Chemists rely on supplementary classifications to represent likeness relations not captured by the elemental classification. Some of these build upon the elemental classifications, such as *metal*, *non-metal*, and *metalloid*. Others are independent of the elemental classifications and operate at a higher level of organisation, like *acid* and *base*. Yet others are relativized to realms of enquiry, like *poison*. Pluralistic taxonomic systems are a must for scientific enquiry. What makes biology different is that its 'fundamental' natural kind classification is not capable of being nearly as fundamental as that of chemistry.

1.3 Laws and Kind Historicism

Theoretical pluralism about biological classification is just one corner of theoretical pluralism in biology. Just as biologists develop alternative taxonomic systems, testing their scope and applicability, so too do biologists develop biological rules or generalizations and attempt to determine the scope of their applicability. Biology journals are full of 'relative significance controversies' (Beatty 1995), cases where biologists seek to determine the 'extent of applicability' of biological generalizations. Common examples include the Krebs cycle, which most but not all aerobic organisms use for the metabolism of carbohydrates, or the lac operon model of gene regulation, which appears to be only one of many mechanisms that organic systems use to regulate genetic expression. In each of these debates, generalisations initially believed to apply to the whole of biology have had their scope gradually eroded, until they fall in status from law to imperfect generalisation.

A number of philosophers have argued that there will never be any distinctively biological laws (Beatty 1995, Fodor 1974, Millikan 1999a, Mayr 1982a, Smart 1963, cf. Ruse 1973, Sober 1997).[161] This lack of laws in biology is yet another feature that marks biology as somehow importantly different from (e.g.) physics or chemistry, which feature a number of perfectly invariant generalisations. Although the discussion is ostensibly about biological practices of generalisation, the suspicion is that something about the biological world lies behind this state of affairs. The suspicion is that generalisations are not applicable to biology. In the case of theoretical pluralism regarding classification, I argued that plurality in scientific practice should be seen as a necessary result of the historical nature of biological kinds. I shall now extend this argument to the problem of laws and generalisations in biology.

A 'law' in this context is an exceptionless generalisation that is counterfactual supporting. The first condition ensures that the law applies to all cases, the second condition ensures that the truth of the law is not contingent upon local facts and would therefore hold across all possible changes in environment. Biology is inhospitable to both conditions. First, all generalisations in biology appear to admit of exceptions. Second, even an exceptionless generalisation (if such a thing obtained) would fail to be counterfactual-supporting.

Any student of biology will tell you that biology contains generalisations. They will tell you how gametes always segregate in 50/50 ratios, how certain genes *x* cause phenotypes *P*, or how sex ratios will always reach equilibrium. The biology student will also tell you, however, that these generalisations are not perfectly general. Exceptions to each abound. But this should not be surprising; the existence of at best imperfect biological generalisations is an expected outcome of evolutionary processes.

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[161] I say 'law-like generalisation' to focus attention on cases where a generalization ranges over some specified set(s) of kinds. Brandon and McShae (2010) have proposed a law of biology that does not fit this picture. Pace Beatty (1995), there is room to argue that this is not a law of *biology*. It may be a mathematic generalization that, in this instance, applies to biological objects that instantiate its requisite pre-conditions. At any rate, my concern in this section is the idea that perfect generalisations do not generalize over biological classes.

Sometimes, evolutionary forces tend to favour the predominance of certain states of affairs, such as the three mentioned above. Perhaps selection acts against organisms that segregate genes irregularly, that have gene *x* but lack phenotype *P*, or whose populations have a predominance of one sex over the other. In these cases, evolutionary outcomes give rise to stable causal regularities, which biologists capture with causal generalisations. However just as easily as evolution makes these regularities, it can break them. Changes to a biological system, whether in the form of mutation or adaptation or environmental change, can interrupt otherwise stable causal relationships. An organism may develop a mutation leading to 30/70 meiotic division. A mutation may introduce gene *z*, which mutes *x*'s power to produce *P*. A population may lack free competition among individuals, permitting an unbalanced sex ratio. None of these cases will necessarily result in decreased fitness, and at any rate their very occurrence (never mind prevalence) is enough to weaken the scope of relevant generalisations. This is how evolutionary forces can both bring about generalisations and introduce exceptions that limit them. For this reason, even if an exceptionless generalisation did obtain, it could not be a law. Suppose it really was the case that all organisms with gene *x* had phenotype *P*. This fact and its corresponding generalisation would be contingent on whatever features of the world brought it into being, such as a supportive environment and a lack of genes *z*. Since it is possible that those features of the world will change, introducing exceptions, the exceptionless generalisation is not necessarily exceptionless, only contingently so. It is therefore not a law.

John Beatty (1995) makes precisely this point in his classic argument against laws in biology. His focus is on the *contingent* nature of biological generalisations, explaining why any biological regularity is contingent on evolutionary outcomes.

> [T]he conditions that lead to the evolutionary predominance of a particular trait within a particular group may change, so that the predominance of the trait declines. Somewhat more colloquially: what the agents of evolution [drift, selection, mutation, etc.] render general, they may later render rare. Two sources of this kind of contingency are mutation, and natural selection in changing environments. Suppose that relative hairlessness owes its prevalence to the fact that it was favored under particular circumstances by natural selection—relative hairiness being selected against… Is there anything naturally necessary about the circumstances under which relative hairlessness was favored—something that could not change? (222)

The answer to his (rhetorical) question is: 'of course not!'. Whatever conditions brought about relative hairlessness in humans could just have easily changed, bringing about relative hairiness. There is nothing necessary about those conditions or their effects; they are contingent features of the evolutionary landscape. This contingency does not prevent us from making generalisations about relative hairlessness, which does after all obtain in most human populations; but these generalisations are imperfect. They do not constitute laws, since they could have been false (they will not support counterfactuals concerning changes in conditions of selection) and they will likely not be *exceptionless* generalisations, since even in a world that has tended to favour relative hairlessness, hairy mutants will crop-up (e.g. hypertrichosis).

In their most basic form, scientific generalisations are statements ranging over kinds and operating on properties possessed by members of those kinds. The example from Beatty is a claim about the kind 'human' and the property 'relative hairlessness'. Generalisations of this form are confronted with the problem of intrinsic heterogeneity. For reasons outlined above, it is possible that any member of 'human' might lack the property of being relatively hairless. But what about laws? Suppose all of the hypertrichosis patients in the world died, making 'all humans are relatively hairless' an exceptionless generalisation. Is it a law? No. It is an exceptionless generalisation at present, given the present humans that make up the kind 'human', but humans of the future will necessarily have different historical essences from both one another and from humans of present. It is possible that some of those histories may include an interchromosomal insertion at Xq27.1, resulting in extreme hair growth. The very possibility of exception precludes laws.

The possibility of exceptions is grounded in, first, physical possibility and, second, historical essentialism. First, cases where particulars that lack certain properties, like hairlessness, are physically possible; they are permitted by whatever physico-chemical laws govern genetic mutation/insertion. Second, historical essences are collections of events, and any possible event is liable to be part of that history. Putting these together, any biological change that is within the realm of physical possibility for a biological particular is therefore capable of entering into the historical essence of that particular. This fact precludes laws and renders exceptionless generalizations unlikely for any biological property for which change is possible. Insofar as philosophers of biology appear to agree that *all* properties of biological objects are liable to change (see anti-essentialist consensus, Chapter 2), this reasoning precludes all biological laws and renders unlikely all generalizations *to the extent that* those laws/generalizations invoke shared properties of biological objects. This is how Kind Historicism relates to the problem of laws in biology.

Notice that not all generalisations range over natural kinds. The example of the Krebs' cycle, above, is one such case. It ranges over the class of all aerobic organisms. In physics and chemistry we can think of many generalisations that range over higher-level groups, such as the claim that *all metals are conductive*. This generalisation ranges over the higher-level kind, 'metal'. These may not be 'laws', but it is nevertheless often observed that physics and chemistry offer stronger (more broadly invariant) such generalisations than biology. Can Kind Historicism shed any light on these cases? Generalisations such as these, in spite of ranging over induction-supporting kinds, still bear some relations to the features of the type of kind category of their particulars (microstructural or historical) even though those particulars will belong to an assortment of natural kinds. This is easier to see with the non-biological generalisations. The class 'metal' is made up of a selection of natural kinds *a, b, c, … n*. Each kind, *a…n*, contains particulars co-instantiating a microstructure. It is therefore possible that microstructures of kinds *a…n* share some property (or properties) *P*. In the case of metals, these microstructures share certain properties (to do with numbers of free electrons) that support the causal disposition to conduct electricity.[162] The possibility of strong generalisations ranging over non-kinds is therefore grounded in the invariant multiply-instantiated essences of non-biological kinds. Since biological kinds lack invariant multiply-instantiated essences, they are less hospitable to such generalisations. The class 'aerobic organism' spans many biological natural kinds. Within any one of those natural kinds are diverse essences yielding diverse organisms. Compounding within-kind variation across a class made up of multiple kinds renders shared essential properties unlikely. Moreover, for reasons outlined above, any similarities that do arise will be contingent, not necessary.

2. Final Thoughts

Contemporary philosophy of biology has not been hospitable to metaphysics and ontology. A general anti-metaphysical attitude in our field is evidenced by a recent survey of philosophical opinion (Bourget & Chalmers 2014), which reveals that philosophers of biology are far less metaphysically-inclined than their peers.[163] I hope to have shown that some of the anti-metaphysical attitude, particularly that directed toward essentialism and natural kinds, is based on misunderstandings of those positions. I have also shown that, contrary to popular opinion, a categorically monistic account of biological kinds *is* possible, and that such an account need not conflict with Darwinian biological theory. To the contrary, my Kind Historicism flows straightforwardly from a Darwinian view of the biological world. It is a Darwinian metaphysic.

My account also clarifies the role of natural kind (and individuality) enquiries relative to scientific investigation. When it comes to doing science, these are the wrong tools for the job. The right tools are induction-supporting kinds, which I have discussed sparingly. If

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[162] I am choosing to ignore the complication that this generalization toggles between claims about atoms and claims about pure substances formed of those atoms. This is not a serious complication; but would make the explanation unnecessarily complicated.

[163] On *a priori* knowledge, and on the existence of abstract objects, Philosophers of Biology were far more bullish than their peers in Philosophy of the Physical Sciences, General Philosophers of Science, and All Philosophers. For example, only 9.1% of Philosophers of Biology support Platonism about abstract objects, whereas general philosophical opinion puts the number at 31.5%.

philosophers of biology want to aid scientific classification and individuation, they ought to forget about natural kinds and instead determine the virtuous and vicious features of induction-supporting kinds. So too with individuation. Searching for '*The*' individuals is fraught. It is better to seek an account of individuation that ties scientific investigation to whatever features of the world are relevant to those investigations. In both classification and individuation, seeking to award badges of ontological merit will only lead philosophers astray. This is not because accounts of natural kinds and individuation do not apply to the biological world, but because such metaphysical investigations are divorced from the practical reasoning of science.

Some further questions in this line of enquiry remain. First, some philosophers take certain moral facts to flow from metaphysical facts (about kinds or individuals). In debates about abortion, for instance, philosophers have appealed to Aristotelian individuation criteria to determine when a foetus is part of or distinct from the mother (e.g. Smith & Brogaard 2003). Such uses of biological ontology are not covered by my argument against natural kinds in scientific reasoning. Future work might examine an extension of my pragmatic attitude toward classification to cover these metaphysically-informed scientific ethics. Second, my discussion of laws and generalisations in biology is but a sketch. A full treatment of the issue should be more firmly grounded in the actual generalisations of the biological sciences, examining the classes used.

Additionally, accounts such as this one must reckon with the 'structuralist tendency' (French 2011) emerging in the philosophy of science. In debates about realism, taking their lead from the metaphysical implications of quantum physics, structuralists eschew the existence of objects in favour of structures. The precise understanding of 'structure' notwithstanding, it is difficult to square the potential non-existence of objects with the views presented here. It has been suggested, for instance, that structural realisms offer a solution to the problem of biological individuality (French 2011, 2013), taking focus away from questions about the precise composition of biological individuals and toward questions about the relevant causal and relational structures that maintain/generate (what appear to us to be) those individuals. The emphasis on the processes in which individuals participate is commensurate with my focus on individuals as evolutionarily composed. However the general rejection of objects challenges any claims whatsoever to biological natural kinds—insofar as the particulars that form kinds are objects. Though Steve French has made several appeals for an application of structural realism to biology (French 2011, 2013), exactly what this ontological eliminativism means for both biology and its metaphysics remains to be seen.[164]

A final metaphilosophical point remains, concerning the worth of explanations in scientific metaphysics. Kind Historicism is a part of a recent broad trend, called 'scientific metaphysics', which attempts to bring metaphysical/ontological theory in alignment with scientific knowledge. As an evolutionary metaphysics, Kind Historicism fits the bill. It preserves the historical way of thinking about biology developed since Darwin, and thus does not change fundamentally how we think of the biological world. Instead, it provides metaphysical accounts of certain features of the biological world. As scientific metaphysics grows, perhaps eventually developing some shared methods or tools, many such accounts will emerge. When and as this happens, we must be prepared to ask after their utility. I have established that the metaphysics of natural kinds has no practical scientific utility, but I have not asked about the *explanatory* utility of my account. I believe this account is philosophically important, and timely, for reasons offered throughout, particularly Chapters 1 and 2; and I can say the same for much of the work that goes on in scientific metaphysics. But philosophical importance is not everything. The explanatory worth of these accounts should not be overshadowed by their philosophical timeliness. We should ask whether these scientific metaphysical accounts explain anything. We should ask for instance whether Kind Historicism explains anything that Darwinian theory does not. For now, at least, my account is on the table—a table increasingly crowded with other such metaphysical accounts. Soon, when we have established by preponderance of cases that science and metaphysics can be brought into alignment, we must sit at that table and begin to digest.

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[164] I am confident that structural realism helps make sense of the ever-changing notion of the gene in biology. French's semantic articulation of ontic structural realism is compatible with the evolution of the gene concept I presented in Bartol (2011, Ch 2).

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