

**Towards an Adequate Definition of Species within the Framework of Evolutionary Biology,  
Genetics, and Phylogenetic Systematics, which is Empirically Testable, Generally  
Applicable and Mindful of Existing Concepts, yet which Avoids their Weaknesses**

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## 1. Introduction

In this thesis I will be arguing that species are to be best understood in terms of the cohesion concept of species, as expounded by Wiley, Brooks and Collier.<sup>1</sup> Species are held together by forces, or mechanisms, that maintain their distinctness from other species. Species under this view are individuals (as opposed to classes or collections), and are dynamical systems which are best delineated according to the principles which individuate all dynamical systems (i.e. cohesion). The reason why such a theory is necessary is because of the lack of a single, sufficient account of species in modern biology. My initial claim is that biology is plagued today by numerous, similar, yet often conflicting concepts of species, resulting in problems for biologists trying to work towards a singular phylogenetic tree for the living world, which the theory of evolution by natural selection predicts. Some examples of this are the trap door spider of Southern California, or the *Aptostichus atomarius* species complex. In a paper by Bond & Stockman (2008) entitled *An Integrative Method for Delimiting Cohesion Species: Finding the Population-Species Interface in a Group of Californian Trapdoor Spiders with Extreme Genetic Divergence and Geographic Structuring*, this specific species complex is shown to have a highly variable number of actual species, depending on which species complex is employed, as they state:

Highly structured, genetically divergent, yet morphologically homogenous species (e.g., nonvagile cryptic species), although often ignored or overlooked, provide one

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<sup>1</sup> Templeton (1992) also has a cohesion concept of species, but it is much more nominal than the view I defend and lacks the unifying character of the dynamical approach used in this thesis because it posits only a collection of mechanisms without a general account of how they are related or interact with each other.

of the greatest challenges to delimiting species (e.g., Bond et al. 2001; Hedin and Wood 2002; Sinclair et al. 2004; Boyer et al. 2007). Populations, or very small groups of populations, constitute divergent genetic lineages but present somewhat of a contradiction because they lack the “requisite” characteristics often used when delimiting species. Morphological approaches to species delimitation in many of these groups grossly oversimplify and underestimate diversity (Bond et al. 2001; Bickford et al. 2006); in short, these traditional applications fail if our interests extend beyond what can simply be diagnosed with a visual and/or anthropomorphic-based assessment. When genetically divergent, morphologically equivalent lineages exhibit microallopatric population structuring, lineage-based approaches to delineating species are further confounded; virtually all population groups are independent lineages and, thus, qualify as species (Agapow et al. 2004; Hickerson et al. 2006), likely yielding specious results.

These sorts of problems are common in biology, and result in the sort of confusion displayed above. The debate over which species concept is the most suitable is one which rages on. Various theorists have weighed in on the debate, all the way from Aristotle to more modern thinkers like Ernst Mayr and Alan Templeton. Aristotle posited an essentialist and typological species concept, in which species were defined according to specific properties which gave them their being. More modern thinkers dismiss this typological thinking, acknowledging that species are better treated as individuals instead of classes, and posit certain specific forces by which we are to define them; for instance, Ernst Mayr’s Biological species concept, which treats species as actual or potentially interbreeding populations of organisms. Other species concept favour

ecological factors, mate recognition, competition, and others as being the primary defining aspects of species.

It is my contention in this thesis that these species concepts all fail for more or less the same reason, and that a better way of defining species exists. My argument is that most modern species concepts are reductionist (or operationalist) in nature, and that they fail because they ignore the various developments made in the actual science of evolutionary biology. This phenomenon, I claim, is largely unknown to most theorists, and occurs due to residual and tacit assumptions creeping in from outdated species concepts, and leftover empiricist tendencies. I base my critique on a doctrine of naturalized metaphysics which asserts that most modern metaphysics has been led astray by a tendency to ignore actual science, as I state in a 2011 paper:

It is Ladyman et al's claim that modern metaphysics has become woefully isolated from actual science. Metaphysicians are left arguing over issues long resolved by actual science. For instance, Ladyman et al draw attention to the debate amongst over the nature of matter, specifically the nature of matter as either 'gunk' (in the sense of an infinitely divisible substance) or atoms (atoms being partless particles). This debate, according to Ladyman et al, 'is essentially being conducted in the same terms as it was by the pre-Socratic philosophers among whom the atomists were represented by Democritus and the gunkists by Anaxagoras...It is preposterous that in spite of the developments in the scientific understanding of matter that have occurred since then, contemporary metaphysicians blithely continue to suppose that

the dichotomy between atoms and gunk remains relevant, and that it can be addressed a priori.' (2007 20). (Grant 2011).

While many species concepts today posit some mechanism(s) relevant to evolutionary theory, the tendency is to restrict the definition to simply one or a few, ignoring a host of others, and in so doing limiting the organisms and populations which may be considered species. I argue that this is because of residual, tacit assumptions left over from empiricism and other habits of science which try to find hard and fast rules to define concepts.

My contention is that the metaphysics concerning species needs to be treated in a naturalistic fashion, namely by acknowledging and using our best current, actual, science on the topic. Modern evolutionary biology posits a number of mechanisms responsible for evolution and change in species, from natural selection and genetic drift, to ecological and developmental factors. All of these (and perhaps others yet unknown) need to be taken into account when dealing with the individuation of species.

I claim that essentialism about species is not an entirely inaccurate way of approaching the problem, and that our best concept should be an essentialist one, albeit a very specific kind of essentialist. I argue for the use of Locke's real essence as a good way of understanding species. According to Locke, a real essence of a thing is the underlying causal structure which gives a thing its being. My argument is that a good species concept needs to be true to the causal elements which give species their being, and for me these things are evolutionary mechanisms

taken as a whole. Other modern species concepts tend to take only one or a few mechanisms and treat them as wholly defining of species. This has the unintended consequence of reducing species individuation to these mechanisms in an operationalist fashion. At the heart of this is the issue of pattern and process in science and trying to find a bridge between the two. Theory is a necessary part of bridging this gap if we want to avoid certain pitfalls such as extreme micro reduction, as I will show.

I begin this thesis by describing a history of species concepts in order to illustrate the problem and shed light on how it is that we got to where we are now. From there I go on to expound the doctrine of naturalized metaphysics and show how it is relevant to the species problem, and argue for why it is the best way of approaching it. I will then go into some detail about the specifics of the metaphysics, such as the problem of operationalism, and how Locke's real essences are an appropriate way of approaching the problem. I argue that species are best understood as dynamical systems, which are systems governed by forces and flows, and therefore the best way to individuate species is the same way in which all dynamical systems are individuated, namely through cohesion.

I also go on to show how, using Kuhnian theories of the structure of scientific theories and the nature of paradigms, it is possible that tacit assumptions in science can be passed on from one paradigm to the next. The term 'species' itself carries with it many of the assumptions present in earlier paradigms such as Aristotle's, and so I claim that it is because of this that a large amount of confusion regarding species concepts has come to be.

## 2. History

Most of the current literature on the species problem divides its history into roughly three major epochs. The first epoch is the epoch of Aristotle and essentialism. The second is that of Darwin, and the impact the theory of evolution by natural selection had on our understanding of species. The third epoch is the one we are in now, which is a kind of pluralist epoch that I will say began around the middle of the 20<sup>th</sup> century with Michael Ghiselin's (1974) and David Hull's (1978) independent 'discoveries' that species aren't, in fact, classes, but rather individuals, in an ontological sense; that is, species are spatially and temporally located particulars with a history. Whilst there is a large degree of consensus surrounding the notion of species as individuals, there is a quite spectacular multitude of concepts about the *nature* of those individuals. For instance, are they individuals composed of organisms that can interbreed (Mayr & Provine 1981)? Or are they individuals composed of organisms that recognize each other and can thus mate (Paterson 1980)? Or, are they something entirely different? This is the central question I will be attempting to answer in this thesis. In order to do this, however, it will be useful to examine how it was that we got to where we are now in terms of our understanding of the term 'species'. What we are dealing with, loosely, is a series of Kuhnian scientific revolutions within the realm of biology through which the term 'species' has survived, but which has changed its meaning somewhat. I feel it is largely the unjustified retention of previous understandings of 'species' that has contributed to much of the confusion and conflict regarding species concepts that we see today. This question I will deal with as this thesis unfolds. For now I would like to take a look at the history of the concept of species beginning with Aristotle and making my way through to contemporary times.

Most historians of the species problem begin their account with Aristotle. He is believed to be the quintessential essentialist, and it is his concept of species as organisms bound to classes based on certain essential properties that is taken to be the first. Species, according to Aristotle, were ‘eternal, immutable and discrete’ (Hull 1988; 82), much like the basic components that made up the natural world. All species had a set of necessary and sufficient morphological properties which facilitated the classification of individual organisms into them (Hull 1988). This view has, in recent years, come under scrutiny. Some scholars are taking a revisionary approach to Aristotle’s work and claiming that, in fact, Aristotle was not committed to essentialism about species as we understand it today (Richards 2010).

I am no scholar of Aristotle, and thus do not take up the challenge of making truth claims about whether or not he was, in fact, an essentialist. My aim in this chapter is somewhat more modest, that is, to give as complete an account as possible of the various strands of thought that led thinkers in various ages to conjure up this or the other species concept. Thus, this chapter will be somewhat more like a *dramatis personae* of the various conceptual characters in the history of the problem. A more ambitious task, as mentioned in the introduction to this chapter, will be to look at and discuss the various contexts in which the term ‘species’ exists/existed and to see how the retention of residual meanings has resulted in confusion. This will give us a better understanding not only of how our current species concepts are mistaken, and in which ways they can be remedied, but also in which ways they are correct but incomplete.

As stated above, the essentialism of Aristotle has, in recent time, come under the scrutiny of historians of biology. It is their claim that Aristotle didn't hold the essentialist view of species that we suppose he did, a claim that is termed the 'canonical view' of the history of essentialism by Wilson et al (2009), according to which species concepts were essentialist post-Aristotle and non-essentialist post-Darwin. Darwin's theory of evolution by natural selection is supposed to have shifted our understanding of species to such an extent that any notion of them as immutable could no longer stand (Wilson 2009). Again, whether or not this is true falls not within the scope of this chapter. My aim is to give an account of the various theoretical frameworks within which the term 'species' has functioned throughout history, starting with essentialism. I mentioned briefly above what essentialism about species entailed. Here I will quote Ernst Mayr for a more detailed description:

... [t]here are a limited number of fixed, unchangeable "ideas" underlying the observed variability [in nature], with the eidos (idea) being the only thing that is fixed and real, while the observed variability has no more reality than the shadows of an object on a cave wall . . . For the typologist the type (eidos) is real and the variation an illusion. (Mayr 1959).

And here, Ereshefsky:

Kind essentialism has a number of tenets. One tenet is that all and only the

members of a kind have a common essence. A second tenet is that the essence of a kind is responsible for the traits typically associated with the members of that kind. For example, gold's atomic structure is responsible for gold's disposition to melt at certain temperatures. Third, knowing a kind's essence helps us explain and predict those properties typically associated with a kind. (Ereshefsky 2007a in Wilson et al 2009)

So, essentialism, in general, posits the notion that individuals of all kinds (living and non-living) have a specific property, or set of properties, which defines them in some way. This property, or set of properties, constitutes an individual's 'being'. These properties, or essences, define kinds, which are classes into which individuals are placed based on their possession of this or the other property. Sober (1980) suggests a number of conditions that essentialism requires of its defining properties: They must be explanatory in some way, i.e. the essential property must explain why a particular organism is the way it is. Sober claims that, otherwise, the conditions for essentialism can be met trivially (1980). For instance, the condition that all and only the members of a species must contain a certain property can be met by simply listing a set of spatiotemporal locations of the organisms involved. This is guaranteed by the fact that there is, more than likely, a finite number of members of a species (1980). Of course, what essentialists are really looking for is a diagnostic property; a property that determines whether any possible organism is a member of a certain species (1980). It cannot be the case that there exists a member, say, of Homo Sapiens that does not have the defining characteristic which makes it so. Thus it is a necessary truth that members of a species have this defining characteristic. This, Sober claims, is equally satisfiable by giving the logical truism that all members of Homo Sapiens are members of Homo Sapiens.

Of course the truth of logical claims does not vindicate the truth of essentialism (1980). It is thus why Sober claims that essentialists require an explanatory aspect to defining characteristics. Sober also stresses that a species needs to be defined in terms of the *organisms* which compose it. This he calls a 'constituent definition' where wholes are defined in terms of their parts. More generally, it states that entities at some level of organisation must be defined in terms of some lower level of organisation (1980)

We thus have a suitable account of what essentialism is supposed to be: As well as being a general doctrine applying to both living and non-living entities according to which their 'being' is determined by the possession of a specific property or set of properties, essentialism also requires that defining properties be explanatory. It is also *necessary* that any entity with a defining characteristic be part of the group which is defined by that characteristic.

This brand of essentialism in biology is said to have reigned from antiquity to roughly the period that Darwin first posited that species change, or evolve, via a process of natural selection (Sober 1980; Hull 1988; Richards 2010.). It was this discovery of change which supposedly challenged the doctrine and forced its usurpation from primary usage. Sober (1980) challenges this objection to essentialism by drawing our attention to a number of facts about it. Firstly, essentialists are not necessarily committed to species stasis. It was a quite commonly held belief that organisms could change their species, in much the same way that elements in the periodic table transmute from one into the other. This was an attribute of the organisms composing a kind, not of the kind itself. Kinds are fixed, whereas organisms have the ability to change their kinds, as Hull (1988

82) notes: 'To be sure, an organism might change its species, just as a sample of lead might be transmuted into a sample of gold, but the species themselves remain unchanged in the process'.

Another common objection to the essentialist concept of species concerns their perceived continuity. It is claimed that if species are to be the kinds they must be discrete. Even Aristotle, however, did not fully maintain this notion, as Sober (1980) quotes:

... nature proceeds little by little from inanimate things to living creatures, in such a way that we are unable, in the continuous sequence to determine the boundary line between them or to say which side an intermediate kind falls. Next, after inanimate things come the plants: and among the plants there are differences between one kind and another in the extent to which they seem to share in life, and the whole genus of plants appears to be alive when compared with other objects, but seems lifeless when compared with animals. The transition from them to the animals is a continuous one, as remarked before. For with some kinds of things found in the sea one would be at a loss to tell whether they are animals or plants. (From *History of Animals* in Sober 1980).

Hull (1988) notes the same thing:

[Aristotle] did not think the boundaries in conceptual space between species are perfectly discrete; they are usually a matter of 'the more or the less'.

So, essentialism does not require that organisms remain static, or that they should be strictly delineable. Sober (1980) points out that even in cases where essentialism is vindicated, such as chemistry, strict delimitation is merely an ideal; vagueness appears everywhere. So, then, in what ways does essentialism fail to be a suitable ontology for species? Sober attributes this to a failure to give an adequate account of variability within species. It was long supposed that any variability was a consequence of some sort of deviation from a natural norm, brought about by certain irregularities (Sober 1980). Morphological differences between organisms in a species could be discounted so long as it was the case that there existed some underlying structure, or essence (Sober 1980). The epistemic difficulty of unearthing this essence is no strong reason to discount its reality; Aristotle, for instance, postulated a Natural State Model which stated that all individuals tend towards a Natural State, which is disrupted by outside forces (Sober 1980). Thus for Aristotle, most of nature was characterized by ‘monsters’ who didn’t match their Natural state (for Aristotle this meant offspring being identical in every way to their paternal parent. Females, he thought, were deviation from the Natural State, albeit necessary deviations) (Sober 1980).

As time wore on, it became apparent that variations within species were less a deviation from the norm than the norm itself. Sober (1980) affords us a useful history of this process with reference to the development of error theory. As this field developed, and as our understanding of how variation occurred, it became increasingly difficult to sustain the notion that variations amongst organisms were the result of deviation from a norm. I will not go into detail on this development, but instead simply give a brief account of some of the main points.

Early error theory was a theory *about* errors, and was first developed in the late 18<sup>th</sup> and early 19<sup>th</sup> centuries (Sober 1980). What it sought to do was find a way of interpreting ‘discordant observations’ (Sober 1980). The underlying assumption was that in nature there is one true value and, in the same way that Aristotle understood it, interfering forces result in divergent results: ‘The problem for the theory of errors was to penetrate the veil of variability and to discover behind it the single value which was the constant cause of the multiplicity of different readings.’ (Sober 1980). Notice that the theory in this formulation is an epistemological account, not an ontological one; it was a theory about why our *observations* about nature differ, not about why nature itself differs (Sober 1980). The theory was given an ontological slant in the 1830’s by the Belgian statistician Adolphe Quetelet (Sober 1980). He drew on a distinction made by Laplace regarding physical forces, between ‘constant causes’ and ‘disturbing causes’ (Sober 1980). Quetelet applied this distinction to man and developed the notion of the *average man*. Average man was, to him, the proper subject of the social sciences (Sober 1980). What this man amounted to was the peak of a bell curve. This concept of man is still one in which there is a primary, or constant, being, which is acted upon by accidental or disturbing forces, which results in their being variations and idiosyncrasies. As Sober expresses it, ‘For Quetelet, variability within a population *is caused by* deviation from type’ (Sober 1980). Such variability is something which, for Quetelet, needs to be explained away. It is not something pertinent to the entity itself, but rather a barrier to an understanding of the entity. The final move away from the notion of variability as deviation from type was instigated by Frances Galton, a cousin of Darwin’s, who developed a new way of accounting for variability: heredity (Sober 1980). Although his ideas about heredity are, in retrospect, rather primitive, they are nonetheless pivotal in the move away from a typological view of species. For Galton ‘variability is not to be

explained away as the result of interference with a single prototype. Rather, variability within one generation is explained by appeal to variability in the previous generation and to facts about the transmission of variability' (Sober 1980). Instead of seeing variability as some unfortunate by-product of interfering forces, which function solely to cloud a clear view of an essence or type, it became something which is an important part of nature itself; something real, and causally efficacious (Sober 1980).

This move away from typological thinking resulted in the move towards 'population thinking', as Sober terms it. This mode is characterized by treating the population 'as a unit of organization. The population is an entity, subject to its own forces, and obeying its own laws' (Sober 1980; 370). This move negated the need for constituent definition, something which essentialism relies on. As Sober writes,

Essentialism requires that species concepts be legitimized by constituent definition, but evolutionary theory, in its articulation of population models, makes such demands unnecessary. Explanations can proceed without this reductionistic requirement being met (Sober 1980; 372).

For essentialism, a species or population must be defined in terms of organization at a lower level, i.e. the organism. For population thinking, species and populations can be defined in terms of their own distinct organisation. This is one way in which evolutionary theory began to

undermine essentialist thinking (Sober 1980). The typologist, or essentialist, sees the individual as real (with 'real' being equated with 'causally efficacious'), whereas the populationist ignores individuals and views as real the organisation of the population as a whole. Each is attempting to explain diversity and invariance within populations. Typologists explain away diversity by seeking invariant properties amongst individuals. Populationists find invariance in a specific rate of divergence (amongst other things) which is a *property of populations* (Sober 1980).

Perhaps the most significant ontological development regarding species came with their recent designation as individuals. Although populationist notions of species had been around for some time, the move towards treating species as individuals was only made explicit, by both Ghiselin (1974) and Hull (1978) independently, in the middle to late 20<sup>th</sup> century. Treating species as individuals makes sense from both a biological, as well as a logical perspective (Ghiselin 1974; 536). The basic point that Ghiselin wants to make is that 'multiplicity does not suffice to render an object a mere class' (1974; 536). So, for instance, a human being, or a nation state, is made up of various parts, but still maintain their status as a single, cohesive entity. David Hull's argument for treating species as individuals centres more around investigating the current ways in which biologists talk about species, and concluding that treating them as individuals makes the most sense. The basic tenets of each view are that species are historical entities, who, despite their multiplicity, in both spatiotemporality and morphology, remain single, individual units of organisation. Thus understood, individuals are spatiotemporally restricted entities, which are contrasted with classes as spatiotemporally unrestricted entities.

Treating species as individuals places constraints on the types of species concepts which can be accepted. So, any concept which makes it possible, for instance, for the same species to arise in separate spatiotemporal localities, must be dismissed.

The move away from typological, or essentialist, thinking, and the adoption of populationist or individualist notions of species, has left us not with any single, cohesive species concept. Instead we see, more than ever, a smorgasbord of competing and contradictory ones. So we have Mayr's Biological Species concept (Mayr 1992), probably the most widely known and utilized species concept, which privileges gene flow and interbreeding over other properties of populations. Here species are defined as 'groups of actually or potentially interbreeding natural populations which are reproductively isolated from other such groups' (Mayr 1963; Mayr 1992). We also have Ghiselin's Reproductive Competition concept (1974) - a twist on the Biological concept which treats competition as the main factor; as well as the Recognition species concepts (Paterson & McEvey 1993), and Van Valen's Ecological species concept (1976), to name but a few. The question has now largely moved away from species as either classes or individuals and towards the question 'individual whats' (Ghiselin, 1974 537). This vast array of species concepts has convinced some (Mishler & Brandon 1987; Ereshefsky 1998) of pluralism, and in some cases the non-reality of species (Ereshefsky 1998). I will deal here with some common pluralistic accounts of species, and certain cases where this pluralism has led to species anti-realism.

Pluralistic accounts of species, generally, take the view that there is more than one appropriate definition of species (Ereshefsky 1998; Mishler & Brandon 1987). Contrary to this view is monism, which posits the existence of a single appropriate species concept; for instance, either

Mayr's Biological concept, *or* Van Valen's Ecological concept. According to Ereshefsky (1998), there exist at least five different brands of pluralism. These concepts have arisen, he claims, not solely because biologists have offered up so many different ones, but instead because of the suggestion that more than one of them could be true (Ereshefsky 1998). He uses the example of the Phylogenetic species concept and the Biological species concept. Each of these concepts divides the natural world up differently. For instance, there are many populations which qualify as Phylogenetic species (namely, they contain the descendants of an ancestral species) but which do not qualify as Biological, or interbreeding species (an example of such a case is asexual species, which cannot, obviously, qualify as Biological species, but which can operate as Phylogenetic species). Thus, as Ereshefsky points out, evolution segments the natural world into a number of different *kinds* of species, depending on the evolutionary force you choose to acknowledge (Ereshefsky 1998; Mishler & Brandon 1987). Pluralists disagree, however, about a number of the details of this claim. For instance, Mishler and Brandon (1987) believe that these different forces act on different branches of the tree of life. Thus there is no overlap of organisms belonging to two different species (Ereshefsky 1998; 106). On this view there is but one correct classification of the natural world. Others, such as Ereshefsky (1998) himself, believe that organisms can belong to numerous species at the same time, for instance, to a Phylogenetic species, or a Biological one. Other pluralists, such as Kitcher (1984) and Dupre (1993), espouse the more radical view that species can be made up of 'qualitatively defined sets' (Ereshefsky 1998; 107). This view, according to Ereshefsky, is too radical, and has the dire consequence of placing species outside of the domain of evolutionary biology (1998; 107); once the genealogical connections within species are severed 'one casts away the primary mode of explanation in evolutionary biology' (Ereshefsky 1998; 107).

Associated with pluralism is a debate concerning the reality of species. Not all species pluralists are necessarily anti-realists. Kitcher (1984) and Dupre (1993), for instance, take a realist stance, arguing for the equally real classifications of the world afforded by the various species concepts. Even the Cohesion concept itself, which I argue for below, is a specific kind of realist pluralism. Others such as Stanford (1995) insist that species pluralism should cause us to be anti-realists. This is for largely epistemological reasons: we cannot have knowledge about which species concept is the correct one; therefore we must remain agnostic, choosing concepts largely on instrumental grounds (Ereshefsky 1998; 104; Stanford 1995). Ereshefsky posits another form of anti-realism, which is not concerned with the reality of individual species, understood in whichever manner, but rather with the existence of the species category (1998). He argues for this by claiming that there is nothing in common between different species classified according to different concepts. This, he claims, suggests that there are different *types* of species, or base taxa, negating the notion of a unified species category (Ereshefsky 1998).

Of course, there have been numerous objections to species pluralism, most obviously coming from those who posit specific species concepts (Ereshefsky 1998; Mishler & Brandon 1987; Paterson 1980; Ghiselin 1974; Wiley 1981; Mayr 1992). One primary difficulty with pluralism is the problem of selecting appropriate concepts for appropriate reasons. Most monists feel that pluralism leads to an arbitrary designation of species concepts (Paterson 1980; Ghiselin 1974; Mayr 1992). One pluralist Stanford (1995) has argued that the acceptance of concepts depends on their explanatory tractability: 'Species concepts that allow the construction of explanatorily useful classifications should be accepted, provided they are not 'redundant, boring or wrongheaded' (Stanford 1995; 80). Of course, the acceptance of monism makes demands which

cannot be accepted by pluralists: for instance, the exclusion of asexually reproducing organisms as species in the Biological species concept (Mayr 1992).

There is yet a third way of dealing with the species problem, which tries to incorporate and unite all, or most, of the seemingly disparate species concepts. This is termed the Cohesion concept of species. There is, however, more than one Cohesion concept. The term was first coined by Brooks and Wiley (1986), and is implicit in Wiley (1981) with its discussion of vertical and horizontal cohesion within species, but was later adopted by Alan Templeton (1989) and his followers. Templeton's version is today widely recognized as *the* Cohesion concept. Cohesion thinkers are simultaneously monistic and pluralistic. They are monists in the sense that there is a single, broadly defined property which defines species, namely cohesion. They are pluralists in that this property is determined of a number of other properties, notably those that make up the various other species concepts. Templeton defines the concept as follows: 'The cohesion concept species is the most inclusive population of individuals having the potential for phenotypic cohesion through intrinsic cohesion mechanisms' (1989). The mechanisms here include those forces which, singularly, make up the species concepts previously mentioned. They function together in the Cohesion concept to maintain a species' cohesion (Templeton, 1989). So, on this account, there is a balancing of the various evolutionary forces, such as gene flow, natural selection, genetic drift, as well as ecological and environmental factors, whose net effect is a cohesive, organized species. To understand the Cohesion concept better, we should mention something about cohesion in general, as it is applied throughout the sciences.

### 3. Cohesion

Cohesion is used as the individuating factor, or identity relation, in dynamical systems, i.e. those systems governed by forces and flows (Collier 1986, Collier 2010; Wiley 1981). Collier offers us this useful definition:

Cohesion refers to the cause of the dynamical stabilities that are necessary for the continued existence of a system or system component as a distinct entity... These stabilities arise from the constraints which dynamical interactions within a system impose on the dynamics of its components. Since stability in even relatively simple case resists penetration by traditional methods (see any text on non-linear systems for examples), we should not assume that an account of cohesion requires mechanism, decomposability, or reductionistic diagnosability. The basic form of cohesion is a dynamical property of a system that is insensitive to local variations in the system components (e.g. thermal fluctuations, vibrations or collisions), including those (non-linear) interactions that formed it, and to external influences (Collier 2003).

Cohesion also has a number of important properties which, again, Collier affords us:

B1: The first basic property of cohesion is that it comes in degrees. This is a direct consequence of its being grounded in forces and flows, which come in varying kinds, dimensions and strengths. Cohesion, then, must also accommodate kinds, dimensions and strengths. Secondly, and following on from the first property

together with the individuating role of cohesion, B2: Cohesion must involve a balance of the intensities of centrifugal and centripetal forces and flows that favors the inward, or centripetal. Last, this balance cannot be absolute, but must be likely over the boundaries of the cohesive entity. Just as there are intensities of forces and flows that must be balanced, there are, due to fluctuations, propensities of forces and flows that show some statistical distribution in space and time (or other relevant dynamical dimensions). B3: Cohesion must involve a balance of propensities of centrifugal and centripetal forces and flows that favors the inward, or centripetal. Note that the asymmetry of the balances in B1 and B2 implies a distinction between inner and outer, consistent with the role of cohesion in individuating something from its surroundings. The derived aspects of cohesion now follow from the basic properties as they apply to specific systems with many properties. From B1, only some properties are relevant to cohesion. Thus, A1: In general, a dynamical system will display a mix of cohesive and non-cohesive properties. Next, from B2 and B3, A2 Cohesion then is not just the presence of interaction. Whence, A3 a property is cohesive only where there is appropriate and sufficient restorative interaction to stabilize it. From A1, A4: cohesiveness is perturbation-context dependent with system properties varying in their cohesiveness as perturbation kinds and strengths are varied. Given the characterization of cohesion as a condition of a certain form of balance, A5: The interactive cohesive support of nominally system properties may extend across within-system, system-environment and within-environment interactions. Following from this, cohesion is not to be confined to stability of first order

properties like rock shape, kite; rather, A6: cohesion characterizes all properties, including higher order process properties, that are interaction-stabilized against relevant perturbations (Collier 2003)

Determining cohesion is an empirical matter based on an evaluation of the forces within a system. So, for example, in the case of the quartz crystal, it is clear that there is a balance of forces such that it remains in a stable condition and retains its discernible characteristics and properties; namely, a particular arrangement of silicon atoms, and the balance of these forces is quantifiable. The opposite is true of the gas in a jar, which experiences little cohesion due to the nature of the forces acting between its molecules. Cohesion here is constantly breaking, and thus giving the gas no discernible, stable pattern. In terms of biology, especially evolutionary biology, cohesion mechanisms are those mechanisms which promote genetic relatedness, and therefore, phenotypic cohesion (Grant 2011)

It is a specific kind of cohesion concept, namely one which is based on treating species as products of information flow, proposed by Brooks and Wiley (1986), which I argue for in the end.

#### **4. Paradigm shifts and the term ‘species’**

It is clear from what has been said already that the concept of species has undergone a number of radical changes over the years in conjunction (more or less) with shifts in the theoretical frameworks of biology. From the Aristotelian framework through to the Darwinian, and the

contemporary (I acknowledge that a great deal occurred between the time of Aristotle and Darwin, although I use Darwin as a marker as it is because of him, most people believe, that our understanding of species changed the most dramatically and significantly.) We could go so far as to say that these changes in theoretical frameworks coincide quite well with what Kuhn would have called paradigm shifts.

Kuhn first introduces us to his notion of paradigms in his book *The Structure of Scientific Revolutions* (Kuhn 1970). He further clarifies this notion in a chapter of the book *The Structure of Scientific Theories* (Kuhn 1977), due to a large amount of confusion generated out of his initial book. At its most general level, a paradigm is a “disciplinary matrix” which includes a scientific community’s metaphysical commitments, methodological practices, symbolic generalisations (of the form  $f=ma$ , for instance), and exemplars. Exemplars are, for Kuhn, a more particular sort of paradigm, and a far more significant one in his view (Kuhn 1977). This sense of the term “paradigm” is derived in large part from Michael Polanyi’s work on tacit knowing, and focal and subsidiary knowledge (Polanyi 1962). According to Polanyi, understanding something involves tacit knowing, which is the *awareness* we have of particulars, or constituents, whilst attending to a comprehensive whole, without having specific knowledge of the particulars (Polanyi 1962). Once we shift our focus from the whole to the particulars (i.e. make the particulars the objects of our focal knowledge, rather than our subsidiary knowledge) we become incapable of performing the task (Polanyi 1962). For example, riding a bike involves a great deal of muscular coordination, but although we are aware of this coordination, we cannot say exactly how it is that each muscle works in order to explain how to ride a bike (Polanyi, 1962). Kuhn uses a modified version of this theory in his explication on paradigms, particularly paradigms as

exemplars. Scientific communities hold as part of their disciplinary matrix a great deal of exemplary problems, which are more or less consistent throughout the community or field (Kuhn 1977). These are the problems which students of science are required to be able to solve as a matter of routine, and are the prime examples of the workings of a specific theory (Kuhn 1977). Kuhn suggests that exemplars such as these function to provide a kind of tacit knowledge that allows practitioners within a field to recognise similarities in new or novel problems and to be able to apply the appropriate formalism without directly relevant empirical information (Kuhn 1977). It is this learned ability to recognise similarities between problems that makes up a scientific community's subsidiary knowledge, or, in this case, their paradigm. The scientific equivalent, for Kuhn, of Polanyi's shift of focus from the whole to particulars, is the quest to derive correspondence rules for terms or symbols within a scientific field (Kuhn 1977), correspondence rules being sets of criteria which determine when it is appropriate to apply a certain term or symbol to nature, or some particular phenomenon. Kuhn thinks that although it would be possible, perhaps, to come up with any number of correspondence rules for a given term or symbol, doing so would in fact impede the ability of practitioners to fulfil their tasks (Kuhn 1977). The application of this or that correspondence rule actually shifts the locus of a given problem, and in so doing shifts the nature of the follow up research and experiments, according to Kuhn (1977). Scientists are far better at performing their tasks when symbols are attached to nature in an intuitive, implicit fashion, rather than an explicit one (Kuhn 1977).

Having said this, one could characterise the species problem as a quest to uncover explicit correspondence rules for the application of the term species to this or that group of organisms. It is my belief, based on Kuhn's reasoning, that this is what has led to a great deal of confusion

regarding the definition of species throughout the ages, and especially today. Most, if not all, species concepts are explicit, hard and fast rules for applying the label of species to a given group of organisms. For Mayr (1992) the criteria are isolation and interbreeding. For Ghiselin (1987), it is competition. These definitions are operational and hence reductive in nature. They are operational in that they attempt to define species in terms of empirical measurement. The idea behind operationalism is that a theoretical term is meaningful only if we have some way of directly measuring its instances empirically (Chang 2009). So, in Mayr's case, species are strictly those groups of organisms which exhibit interbreeding and gene flow, whether actual or potential. It is assumed that such concepts as gene flow and interbreeding are empirically testable.

## **5. Why Mayr and Ghiselin are Operationalist**

Ernst Mayr (1992) and Michael Ghiselin (1987) offer definitions of species based on one (or a few) aspects of species evolution. For Mayr, species are defined by actual (or potential) degrees of interbreeding and gene flow (1992). For Ghiselin, the definition rests on patterns of competition and mate recognition (1987). It is my contention that these definitions of species are fundamentally operationalist (or at least, appeal to a certain species of operationalism), and therefore fall foul of common objections against operationalism. I will show here why I think that this is the case.

### 5.1. Operationalism

Operationalism is the belief that scientific concepts must be defined in terms of the operations used to measure them (Chang 2009; Gillies 1972). So length is defined by the way in which we go about measuring it. There are a number of different ways of measuring length, depending on scale (Chang 2009; Gillies 1972). For instance, small objects (on a human scale) can be measured using a meter rule (Chang 2009; Gillies 1972). Larger objects, on the scale of celestial bodies, require length or distance measurements to be made by bouncing light and measuring the time taken for its return (Chang 2009; Gillies 1972). On a very small scale, length is almost meaningless in terms of how we usually understand it (Chang 2009; Gillies 1972). For the operationalist, each of these different means of measurement represents a different concept (Chang 2009). Bridgman did himself recognize this as a problem and conceded that in cases where measurements overlap, a common name can be given for them, but only for practical reasons (Chang 2009; Gillies 1972).

The problem which interests me here is the problem of the relationship between operationalism and reductionism. I take it as uncontroversial that operationalism is a form of reductionism. Operationalists, in most cases, seek to reduce various concepts to strict empirical processes. For example, length is reduced simply to the method we use to determine it. There should ideally, for the operationalist, be a different concept for each method we use to describe various instances of length. This kind of reduction I believe to be ontological in nature. Concepts are reduced, in kind, to the various means we use to determine them. By contrast, an epistemological reduction of this kind would not necessarily eliminate the abstract concept of length. Rather the method, or operation used would function as an explanation, linking real world, practical data, with an

abstract concept. A useful way to illustrate this point is to imagine a class of first year undergrad philosophy students learning about Turing and his machine for the first time. Turing, quite explicitly, wanted to replace the abstract concept of thought with the operationalist definition of his machine. He meant nothing more by thought than the successful playing of the imitation game by a machine (Turing 1950). If this machine was able to make itself indistinguishable from a human to another human observer, the machine was thinking (Turing 1950). However, what is commonly the case with undergraduates, the tendency is always there to see the successful playing of the imitation game by a machine as pointing to, and constituting, a broader concept of thought. For them it seems that the imitation game provides evidence for the existence of a broader notion of thought, but does not exhaust the meaning. Students persist in asking whether or not the machine has consciousness, or whether it is capable of creativity. They are not satisfied with the reduction alone. If we take seriously the purely operationalist definition, then there is no further question. Thought is the successful playing of the imitation game. This is a common objection to operationalism in general, which was put forward by Donald Gillies (1972). His contention was that operationalism doesn't exhaust the meaning of a concept. If we take the extreme form of operationalism as being that concepts completely determined by the operations we use to measure them, then we cannot ask the question of whether or not they are valid. They are valid as a matter of tautology (Chang 2009).

It is my contention that species concepts such as Mayr's and Ghiselin's reduce species to operationalist (or quasi-operationalist) definitions which conflate epistemic and ontological models of reduction. In an attempt to square their concepts with the theory of evolution, they take as wholly constitutive of species certain processes, such as gene flow and interbreeding,

which has the almost ironic consequence of eliminating broader elements of the theory from their considerations. By homing in on one aspect of evolutionary theory in the search for an explicit definition of species, they exclude other relevant factors and are forced to retrofit their concepts by excluding various portions of the natural world, such as asexually reproducing organisms, and thereby end up treading the same path as the tautological, strict operationalist definition.

Let's look at Ghiselin's (1974) modification of the biological concept of species to illustrate my point. Ghiselin (1974) emphasizes competition, as an extension of the biological concept of species, to be a fundamental defining aspect of species. He specifies two kinds of competition, interspecific competition, and intraspecific competition. Interspecific competition is simply the competition between species for natural resources, or for the means of survival (Ghiselin 1974). Intraspecific competition is the competition between members of the same species for *genetic* resources (Ghiselin 1974). This presupposes interbreeding and as only those organisms which can interbreed would be able to compete for genetic resources. This kind of competition he also terms 'reproductive competition'. Thus species can be defined as follows: "They are the most extensive units in the natural economy such that reproductive competition occurs among their parts" [Ghiselin 1974; 538]. This definition is reductive in nature. It reduces species to degrees of reproductive competition, even, famously, to the exclusion of asexual organisms. Ghiselin (1974) expands on his concept with an analogy with the normal economy. Species, he says, are like firms, who compete with each other, i.e. between firms, and who have internal competition between members. Like in the normal economy, Ghiselin says, there are firms, constituted by lots of members, and there are also individual tradesmen, who are not themselves firms, but part of the economy nonetheless (1974). Individual tradesmen, so the analogy goes, are asexual

organisms who participate in the economy, but do not interact with each other in such a way as to constitute a whole.

Mayr's (1992) species concept (the Biological species concept, as it is commonly known) is perhaps the most widely known and used species concept of all. It is basically the same as Ghiselin's. In fact, Ghiselin's is primarily an extension of Mayr's concept, extended in order to deal with issues such as potential interbreeding<sup>2</sup>. For Mayr, species are populations which interbreed successfully, or have the potential to interbreed successfully, and which are reproductively isolated from one another (1992). The problems with both these species concepts are numerous, yet they persist as the concept of choice for a number of reasons. Firstly, they do work for most common cases. Secondly, they are based on theory, to a certain degree, and therefore conform with a modern, naturalistic understanding of species. The problems arise when one begins to consider the theory of evolution as a whole, and not just in terms of gene flow or isolation or competition.

Other species concepts exist which are equally operationalist (and therefore reductive) as the two already mentioned. Paterson (1985) postulated what he called the mate recognition concept of species.

I will begin my analysis of these concepts in terms of operationalism by considering the first reason why such concepts are so widely accepted and employed: they work (most of the time).

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<sup>2</sup> Potential interbreeding isn't a problem if we treat species as competing organisms, as it doesn't matter whether or not certain organisms are interbreeding, so long as they are competing with one another, which Ghiselin maintains is happening constantly. What matters is whether or not organisms are competing reproductively, or merely for survival

How are we to judge whether such a concept might work? What are the standards to which we must hold all concepts of species? Surely the answer is theory. The theory of evolution (whether Darwinian or otherwise) was developed to explain (among other things) various patterns observed in nature; patterns of order and diversity within the natural world. I will assume, for the time being, that species are real, and that they are individuals. Mayr himself believes this too, so even if I am wrong about it, at least I am attacking Mayr on his own terms<sup>3</sup>. There are many good reasons for believing in the reality of species, which modern understandings of the theory of evolution reveal quite convincingly. Species appear to be groups of organisms which display degrees of complexity and organization which makes the reduction of them impossible in a lot of cases (Collier 2010; Gabbay 2011). This is a result of a growing body of knowledge of different evolutionary mechanisms, the interactions between which make the possibility of reduction more and more untenable. Nevertheless, if we deny the reality of the species category, then we must find some other units upon which the forces of evolution operate, whether these be individual organisms, or genes, or something else. If this is the case, then the perceived structure and order in the natural world is merely epiphenomenal; a perceived pattern which has no reality of its own, and is wholly reducible to forces acting below it. As I have contended, Mayr appears to be a reductionist about species, though he does assert their reality. As I see it, this is problematic for Mayr, and is where the relevance of theory is best displayed. The Biological species concept must be taken to be, if we maintain that species are real, as fitting with our intuitions about what species must look like, that is, cohesive entities making up the patterns of similarity and diversity within the natural world. Famously, the Biological species concept rejects the status of asexual

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<sup>3</sup> This does not mean that I take their concepts to be highly unhelpful. Being initially based in the theory itself, the biological species concept shows itself to be highly useful in many cases of species delineation. I am simply drawing the comparison in order to illustrate certain tendencies which resemble operationalism, and in so doing open up the concept to particular weaknesses

organisms as species, despite the fact that they display a large degree of apparent cohesion; they display patterns which are not unlike those observed within sexually reproducing organisms. This fact, along with a growing knowledge base of mechanisms which can maintain the integrity of genetic and phenotypic similarity without the need for gene flow or interbreeding, make the Biological species concept increasingly redundant, and force it into a realm of a self-validating tautology.

The move towards operationalism is one solution to the pattern versus process problem (Collier et al. 1996). The problem is one of finding a bridge between perceived pattern in the natural world, and the dynamics, or processes, which cause the patterns. Processes are inherently unobservable, raising questions of their existence (Collier et al. 1996). This is exactly the concern that led Bridgman to adopt his operationalist view concerning theory in physics (Chang 2009). The Harvard physicist became aware of the problem after reading Einstein on the Theory of General Relativity, and saw that there was one operation required for judging the simultaneity of two events separated by space, and another for judging simultaneity when the events were in the same place. One could not fix one by fixing the other (Chang 2009). This led Bridgman to the conclusion that physicists had been too liberal in their extension of concepts to different experimental values (such as mass, velocity etc), which required different operations (Chang 2009; Gillies 1972). His most famous example is that of the seemingly innocuous concept of length (see earlier discussion). Bridgman contented that there is no reason to assume that these two instances of length (length as measured by meter rules, and length as measured by bouncing signals) represent the same concept. We have no way of accurately correlating the two (given the practical impossibility of constructing a row of meter rules between two celestial bodies). Things

become even more troublesome when dealing in units of light years, where the operation used becomes ever more sophisticated and complex. The same applies for very small objects, such as subatomic particles, where the concept of length can hardly be seen to have meaning in the same way that it does on larger scales. For these reasons Bridgman believed that we have different concepts for different instances of length depending on the operation used to determine it. He conceded that for practical reasons scientists could speak of length across operational divides as being the same concept, provided that we end up with mutually consistent numerical results in the case of an overlap (Chang 2009; Gillies 1972)

This problem of fixing concepts is nowhere more evident than in biology, and especially in the species problem. One could draw an analogy between units of mass and units of species, one being a fundamental unit in mechanics, the other a fundamental unit of evolution. Leaving aside issues of commensurability, the concept of mass has different meanings in different theories of mechanics (Newtonian, Einsteinian etc). In the same way, the concept of species must be determined by the theory in which it is employed. This is the very reason why operationalism has failed in physics. One of the key objections raised against it is its supposed circularity. All operationalist definitions of concepts presuppose theory (Chang 2009; Gillies 1972). It is unclear, as Bridgman suggests, how we move from operationalist definitions of concepts to the theories in which they consist. One cannot make sense of length without presupposing theory; theory about, say, how we measure the length of a meter. If we measure length by using a meter rule, we are presupposing that a theory in which it makes sense to use a meter rule to measure length, or, in more extreme cases, why the result of a measurement made by a meter rule is length itself. The actual lengths and units used in measurement are here unimportant as they can

be fixed by mere stipulation. But even here, the stipulation of a length requires a theory of length which is prior to the operation, or else it is viciously circular.

Of course, as Gillies has pointed out, unless the operation itself exhausts the entire meaning of the concept, or theory, and is so true by convention or tautology, then we need not require any theory (Gillies 1972). If it does not, then it must cohere in some way with a broader understanding of the concept which is fixed in the theory. This tension is inescapable unless we adopt the untenable position of operations exhausting meaning. Bridgman himself was reluctant to postulate this view (that operations exhaust meaning).

The same troubles arise when dealing with species, and it is my contention that fundamentally operationalist concepts of species have led to a superficial understanding of them, and given rise to the confusions embodied in the species problem.

Pattern is highly observable (compared to process), but insufficient for the practice of quantitative science and its role in explanation (Collier et al. 1996). Historically, responses to the problem have gone two ways: either extreme micro-reduction, a la operationalism, or the division of biology into explanatory fields such as evolutionary biology, and descriptive, or comparative fields such as taxonomy (Collier et al. 1996). Both these moves have proven inadequate for understanding the exact nature of species. Operationalism results in selectively ignoring certain forces at play within evolution, and hence eliminating various parts of the entire living world. This is easy to see in the case of Mayr (1992). By having species be only those things which do, or can possibly, interbreed, one eliminates, asexual populations from being

included as species. This is problematic given that asexual populations often display similar cohesive characteristics to sexually reproducing ones. To eliminate them seems like a fairly arbitrary move, and is inadequate for a more comprehensive understanding of how the living world evolves.

It is my contention (as well as others, such as Collier, Wiley, Brooks, etc) that any attempt at reducing species to explicit correspondence rules, such as operationalism, is doomed to failure if we hold that species are dynamical, complex systems exhibiting varying degrees of cohesion (the property which individuates dynamical systems). Such systems contain a mixture of forces and flows which interact in a non-linear fashion, making reduction to lower levels in the hierarchy often impossible, even in principle.

When I speak of reduction in this biological context, I am talking strictly about ontological reduction, that is, the reduction of a certain biological entity, to entities at a lower level. The common example is the reduction of an organism to its basic molecules. It is assumed in reductionism that all higher level properties, e.g. properties at an organismic level or fully explainable and, hence, describable, by properties at a lower level, e.g. molecular interactions. Higher level properties are said to supervene on lower level ones, which means that there can be no change in the higher level property without there being some change in the lower level (Kincaid, 1988). All of these issues of reduction and supervenience are controversial, and I will avoid going into them other than to explain the view that certain biological entities are not reducible, remembering that the implication for this irreducibility is the impossibility of strict

and explicit definition, whether it be operational or otherwise. The link between irreducibility and explicit definition is best expressed by Collier (2003 105)

Explicit definitions place the defined term on only one side of the definition, so that all explicitly defined concepts are in principle eliminable. For example, if bachelors are unmarried adult males, by definition, we need not suppose that there are these things, bachelors, in addition to unmarried adult males. Requiring explicit definitions of irreducible phenomena implies that the concepts of these phenomena, at least, can be reduced to the concepts in their definitions. If the concepts refer to dynamically irreducible phenomena, and the definitions are in dynamical terms, then the definitions presuppose dynamical reducibility. A requirement of explicit definitions for all dynamical phenomena in terms of simpler phenomena would rule out, a priori, nonreducible complex phenomena.

Of course, not all dynamical systems are irreducible, and it is useful here to distinguish between weak and strong emergence, and weak and strong cohesion. Weakly emergent properties are properties in higher-level domains which arise from lower-level domains in a way which is unexpected, but which are nevertheless reducible, in principle, to the lower-domain (Chalmers 2006). Properties which are strongly emergent are also unexpected, but are in principle not reducible to properties in the lower domain (Chalmers 2006). So, certain dynamical systems exhibit higher levels which are still deducible from lower levels, such as ion crystals, whose properties are wholly explainable in terms of the ionic bonds. With species, the degree of

reducibility depends on the amount and kind of cohesion present, and hence the degree of non-linearity in the system.

#### **6. Is it appropriate to treat species like dynamical systems?**

A dynamical system is simply a system which is governed by forces and flows (Collier 2010; Wiley & Brooks 1988). It is a causal system. Not all causal systems are complex, or non-linear. Some are reducible, and others are not. A kite on a string is dynamical, but is not complex enough that it cannot be reduced to its component parts. A change say, in the integrity of the string, or in the interaction between the movement of wind with the fabric of the kite, will result in significant changes to the kite-on-string's cohesion. Complexly organised dynamical systems, however, are largely insensitive to fluctuations at lower levels (Collier 2010; Wiley & Brooks 1988). For instance, a human being, at the organismal level, is not greatly affected by perturbations at a molecular, or cellular level. We undergo changes in our chemistry, without fundamentally damaging the integrity of our functional cohesion. The thesis I have put forward, as I have said before, is that species are like human beings. They are individuals, and they are dynamical. We know they are dynamical because they are entities fundamentally exhibiting forces and flows; forces of gene flow, genetic drift, ecology, development (and whatever else), and it is these forces which contribute to their distinct identities. What keeps a species the way it is (its apparent difference from species around it, whether physical, or behavioural) is determined by the relative strengths of those forces. Members of a species tend to have the same characteristics because they share largely the same genes, which they maintain by interbreeding (Mayr 1992; Mayr & Provine 1981). In cases where species *are* in fact capable of interbreeding,

but do not, such forces as mate recognition, or ecology come into play. Examples of this are lions and tigers, which are capable of producing viable offspring, but which don't due to ecological and geographical separateness.

## **7. Information**

Information, generally, is a contested concept in the field of physics, and thus also in biology. There is an abundance of mathematical definitions of information which function well in certain contexts and not so well in others. It is fairly uncontroversial to state that information exists in biology, and especially within species. Genes are, fundamentally, carriers of information which determine protein synthesis in organisms (Collier 2011; Adriaans & Benthem 2008; Smith 2000), and genes are one of the most important aspects of species. Whether or not genes are the only mode of information transfer in species is debatable, but what is not debatable is that they are the most important (Collier 2008). How does biological information work? Drawing on Collier (Collier 2008), there are two kinds of information which we must distinguish in order to understand it clearly: instrumental and substantive information. Instrumental information is the information which is measured as a useful tool in various scientific pursuits (Collier cites bandwidth etc.). A common example of instrumental uses of information is in tree rings. The size and frequency of tree rings give human observers the ability to deduce certain things about the trees past. This information, however, is not intrinsic or vital to the tree itself. It is only relevant to human observers (Godfrey-Smith & Sterelny 2008; Collier 2008). Substantive information, on the other hand, is information which makes a difference within a system without it necessarily being measured (Godfrey-Smith & Sterelny 2008; Collier 2008). An example

might be genes themselves, whose information is vital to the functioning of the organism which it inhabits. In other words, even if we didn't measure the information contained within a certain gene, it is still causally relevant to the organism itself (Godfrey-Smith & Sterelny 2008; Collier 2008). Such substantive information exists, according to Collier, in a nested hierarchy, working down from the most inclusive (It from bit) to intentionality. This hierarchy is nested as it each level contains the properties of the level before, but with modifications which exclude other elements in the previous level (Collier 2008).

At the most inclusive level, as stated before, is the It from bit notion of information (Collier 2008). This view simply states that any causally grounded distinction makes a difference, and is therefore information (Collier 2008). This view arose out of quantum mechanics, and the term was coined by John Wheeler (1990). It basically means that every causal distinction arises out of information, or itself contains information. Its (things in the world) are derived from bits of information which determines their existence and the states in which they appear (Collier 2008). A refinement of this view is negentropy, which is the view that only things (or Its) which can do work (channelling energy, or what have you) can be considered information. Everything else is noise (Collier 2008). This view is bound up in the notion that if we subtract all of the constraints from a system (having  $H_{(Act)}$ ) we end up with a system having  $H_{(Max)}$ , that is, the removal of internal constraints on a system maximises its entropy, or takes it from its actual entropy, to maximum entropy (Godfrey-Smith & Sterelny 2008; Wiley & Brooks 1988; Collier 2008). Thus, negentropy is the organising factor within a system, which constrains its phase states. Beneath, or within, negentropy is hierarchical information (Collier 2008). Here information travels via channels to different levels in a hierarchy. This hierarchy is real, or natural, by virtue of its *doing*

*something*. Purely abstract, or nominal hierarchies might be the physical, the chemical, the biological, the social, the psychological and the social (Collier 2011). None of these elements in the hierarchy is physical or causal elements. They do not do anything. By contrast, an atom, which is part of a cell, which is part of an organism, is a real hierarchy in that behaviour at the atomic level has a causal impact on what happens at the cellular level and so on (Collier 2011). According to Collier (2011), each level of the hierarchy must exhibit its own cohesion, that is, it must be its own functional, dynamically individuated self (see my earlier account of what cohesion is). Each of these cohesive levels transmits information to higher levels in the hierarchy. Negentropy is obviously involved here because it is this which gives a level its organisation, or cohesion<sup>4</sup>. It is as obviously true that not all forms of negentropic information are hierarchical.

From here we venture further down the informational hierarchy into functional information, and then into intentionality (Collier 2008). It will not be necessary, for my purposes, to go deeper. All that is important is that we recognize that biological systems such as species are fundamentally information systems, which exist in an information hierarchy (Collier 2008). This hierarchy extends from the atomic level, up to the cellular, to the organismal, and to the species (beyond this we have ecological systems).

It may be necessary here to say something about how species can be considered a real level. As I have said, in order for something to be real, or natural, it has to (be able to) *do* something. So what is it, exactly, that species do? My argument, as well as that of others before and

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<sup>4</sup> This is a controversial statement. Brooks and Wiley (1988) assert that organization and cohesion within a level is the result of entropic, not negentropic processes.

contemporaneous to me, is that species play a fundamental role in the theory of evolution (Paterson, 1980; Hull 1978; Ghiselin 1974; Wiley & Brooks 1988; Sober 1980; Mayr 1992; Ghiselin 1987; Templeton 1989; Richards 2010; Ereshefsky 1992). So long as we take evolution to be true, we must take species to be real. In other words, the function of the species is to speciate and give us greater diversity. The forces of evolution thus far posited do not act on individual organisms, or on specific parent-offspring lineages, but on a collection of organisms which is organized in such a way as to be susceptible to changes in the various forces at work. The theory of evolution was proposed to explain diversity, and it is species (as the fundamental units of evolution), which are its carriers.

Another way to approach this problem is by referring to species as largely emergent entities<sup>5</sup>. As emergent and non-reducible, species cannot be dealt with by focus on lower levels of interactions, i.e. specific organismal interactions. I will deal with specific cases shortly, but for now it will be enough to claim that whatever is emergent is real, as emergent properties themselves are dynamical, not just in the sense that they are held together by dynamical forces, but that they themselves exhibit causal effects on things around them, as well as on their lower levels (Collier 2008). Species are real levels in the information hierarchy because their properties cannot be dealt with by reference to lower levels. If a certain population of organisms in a certain location exhibits properties (such as all having a particular set of characters, or behaving in a

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<sup>5</sup> It is necessary here to distinguish between weakly and strongly emergent properties of systems. Strongly emergent properties are in principle irreducible to lower level activity, as well as being unexpected given lower levels. Weakly emergent properties are unexpected, although reducible. Such emergent properties we could say are epiphenomenal. Not all entities which look like species will, therefore, be species. In some cases cohesion may be reducible to one or two (linear) interactions between forces, which can be reduced. The advantage of the cohesion concept, however, is that it gives us the framework for dealing with such cases.

particular way, or interbreeding only with each other (interestingly, this point has been used as a criticism of Mayr's biological species concept in that gene flow and interbreeding are properties of cohesive species, and therefore not the cause of their cohesiveness)) which is not explainable by any one specific evolutionary force, then it must be treated macroscopically. This macroscopic entity interacts with its environment (as a whole) as well as with its lower levels to either maintain or breakdown its own cohesion to form new species with new properties.

### **7.1.Information Channels**

A real level in any information hierarchy has to have a number of different pathways through which it receives information from lower levels. Up to now we have been talking about evolutionary forces acting on a locus to produce a cohesive entity called a species. I think a more accurate portrayal of this picture is to imagine these various forces as acting on the flow of information from lower levels to higher ones. If we have a look at the known forces involved in evolution today (gene flow, genetic drift, natural selection, development, ecology, etc.), each of these has some role to play in the transmission of information, either directly or indirectly, thereby impacting constraints on information flow. The most obvious of these channels is involved during gene flow, and it is argued (by many, including Collier) that this is probably the most important factor when it comes to maintaining species cohesion. The reason for this is so obvious as to almost resist explanation (Smith 2000). Genes are carriers of information in that they are codes which carry instructions for the building of an organism during ontogenesis. This information is passed from parents to the next generation via sexual reproduction. A high degree of gene flow within a population (that is, a high degree of interbreeding between all members of

the population) maintains cohesion by ensuring that a majority of the genes from that population is passed down into the next, therefore maintaining identity and a shared evolutionary fate (Barker 2007). Natural selection impacts this information flow over generations by eliminating those pieces of code (or genes) which do not survive environmental stresses (Barker 2007). Not all of the genes in a given population have an equal likelihood of reaching the next generation. If they did, the entropy of the system would be maximal. Natural selection, however, ensures that only those genes which increase an organism's likelihood of reproducing will be passed on. Natural selection places a restriction on the phase space of a species system, lowering its entropy, and thereby acting as a controlling/organising factor.

This is not where the buck stops with regard to information channels, however. There is also an interesting interaction between genes and their phenotypes, which places further restrictions on the transfer of information to the macroscopic, or species, level, as Collier (2008; 773) states:

The route from information stored and transmitted from DNA to the phenotype of an organism is much more complex than the replication of genetic information in reproduction. Replication is fairly well understood, but gene expression, especially in multicellular eukaryotes, is very complex and not very direct

There is not a one to one relationship between genes and phenotypes. The process, as Collier states, is extremely complex, and clarity is difficult to come across on the topic. Suffice to say there are certain well known examples of mechanisms which demonstrate this complexity, and

which show how constraints are placed on the information transfer from gene to realised trait. One such example is the Baldwin Effect.

The Baldwin Effect is an evolutionary mechanism which was discovered in the late 19<sup>th</sup> century. Three separate researchers stumbled upon it independently, James Baldwin amongst them (Simpson 1952). The effect, or mechanism, is one in which characters acquired during an organism's life span (called accommodations) that are, by definition, non-hereditary, become reinforced or replaced by genetic or hereditary characters. This is, essentially, a version of the Lamarckian view of acquired traits becoming hereditary traits. It is a mechanism that has garnered a large amount of controversy, not because it is not true, but because it is difficult to find definitive cases of it. As Simpson (1952) states, all of the necessary factors required for it to work do in fact exist and occur. The difficulty is in finding cases where one can say for certain that the Baldwin Effect took place, and not just standard natural selection. For instance, it is obvious that accommodations exist, by the banal fact that muscles increase in size through frequent use (Simpson 1952). We also know that there are strong correspondences between some of these accommodations, and heritable, genetic effects. The example that Simpson cites is Goldschmidt's phenocopies, which are copies of genetic traits by other organisms without a change in heredity. The existence of phenocopies, according to Simpson, implies the existence of genocopies too, which are copies of non-genetic traits by a change in heredity. The way the mechanism is supposed to work is that accommodations, or acquired traits, increase the fitness of members of the population who have the ability to acquire this trait. Over time, this trait begins to appear in the population as a genetic or innate trait. The problem is with determining whether or not the trait was *actually* wholly acquired from the environment, or whether it was a part of

the genotype in the first place, which perhaps never reached expression due to environmental factors.

What makes the second possibility plausible is the work done by Waddington (Waddington 1942) on canalization. The theory of canalization is used to explain evolutionary robustness, that is, the tendency for lineages to maintain their identities despite changes in environment. Waddington used this theory to explain experiments done on *Drosophila* pupae, which he subjected to heat shock in early stages of development. This heat shock caused a crossveinless phenotype to emerge in some specimens (one can equate the heat shock and the emerging phenotype to the appearance of an accommodation or acquired trait in Baldwin's sense). Waddington subsequently selected for specimens which displayed this phenotype. Over a number of generations, going through the same process, the *Drosophila* pupae began to be born with the crossveinless phenotype, without being subjected to the heat shock. Waddington proposed the idea that the crossveinless phenotype always existed in the *Drosophila* pupae within its genetic makeup as a potential phenotype, but because of environmental factors (canalization) it never reached expression. Only an extreme environmental pressure, and selection, led to its emergence as a perceived innate trait. He likened canalization to a stone rolling down a hill, in a deep canal with high walls. This is the journey of the genotype to expression in the phenotype. There are a number of potential phenotypes that can be realized, but due to canalization, development always travels down a reasonably similar path. This then ensures that the identity (or cohesion) of the species can remain intact despite environmental shift. Only extreme environmental pressures (such as a heat shock in early development) can cause a change in this developmental trajectory. Waddington himself defined the phenomenon as

follows "the capacity to produce a particular definite end-result in spite of a certain variability both in the initial situation from which development starts and in the conditions met with during its course" (Waddington 1975; 99). In terms of the Baldwin effect, canalization may give us a better understanding of innateness. Ariew (1996), who takes his cue from Lorenz, states that canalization gives us a more accurate picture of the dichotomy between innate and acquired traits. As he states:

This makes Waddington's idea a good candidate for an account of innateness: the degree to which a biological trait is innate for a genotype is the degree to which a developmental pathway for individuals possessing an instance of that genotype is canalized. The degree to which a developmental pathway is canalized is the degree to which it is bound to produce the end-state regardless of environmental variation in either (a) its initial state, or (b) during the course of development (1996).

For the Baldwin effect this could mean that instead of viewing the mechanism as a move from acquired trait to genetic trait, we could say that all traits are inherently genetic, but that due to canalization, only a few reach expression, but that this can change in the face of environmental pressures. There will be, of course, non-genetic, acquired traits arising in individuals (say, for arguments sake, the chopping off of a mouse's tail). Such traits don't become heritable because they are not genetic. It is still the case the heredity occurs through genes. If the genotype doesn't exist, it cannot be transmitted.

Some extreme views within the field of developmental biology wish to remove the dichotomy between environment and genes altogether. Some (namely, Griffiths and Gray 1994) make the claim that evolution is best understood as "... [the] differential replication of total development processes or life cycles" (1994). The distinction between replicators and interactors can no longer be maintained, they argue, and must be replaced by looking at development as a whole (1994). Whatever the case may be, whether we can safely divide genetic and environmental factors, I think the important thing to realise is the complexity involved in genotype to phenotype translation, which acts, from an information perspective, as an information channel, and how this information channel has the potential to act as maintainer of cohesion. The importance of development in maintaining species cohesion may not simply be secondary in importance to gene flow or interbreeding. Some (Gould among them) would go so far as to say that gene flow is neither necessary, nor sufficient, for maintaining a species. If the evidence for this proves conclusive, we have a very strong case for rejecting Mayr's Biological species concept, and for accepting the Cohesion concept as an alternative. Matthew Barker (2006) in his paper *The Empirical Inadequacy of Species Cohesion by Gene Flow* wishes to make just such a case. For him, gene flow is certainly not the primary maintainer of a species' identity over time, although it certainly *could* be. Barker asserts, rightly, that the current hegemonic view of species is one which privileges gene flow and interbreeding over other factors in maintaining species cohesion. The cohesiveness of species is largely taken for granted in recent times (we can equate this with the consensus view that species are, after all, individuals and not classes). The problem becomes, what is the nature of species cohesion? For those such as Mayr and Ghiselin, interbreeding and gene flow is paramount. Gene flow works by ensuring that the greatest number of genes from a given population make it into the next generation. The more genes from one population which

make it into the subsequent population, the more similar that subsequent population will be to the prior population as a whole. Therefore, a distinct identity (cohesion) is maintained. On this thinking, if an isolation event occurs, such as a change in geography (let's imagine a river forms, dividing the population in two), the genes from one side of the river cannot flow to the other side (I'm assuming that this is not an aerial species, and cannot cross water. Or perhaps the river is wide enough and turbulent enough that even species capable of swimming could not). If something such as genetic drift (or a new selection pressure) were to occur in a population on one side of the river, the lack of gene flow with the other side would mean that there would be nothing to maintain their similarity. Those new novel genes don't make it across to the other side to keep subsequent generation looking the same. What Barker presents in his paper, however, is evidence to suggest that even without gene flow (in populations which are not restricted, physically, from interbreeding) species identity is maintained. There are also cases in which large amounts of gene flow are not enough to maintain species identity, and cohesion is broken via other factors. I will cite Barker for some examples below.

Colonies of the butterfly *Euphydryas editha*, for example, were found scattered through California with distances of up to 200 km separating them, and yet gene flow between colonies was nearly zero when gaps between them measured as little as 100 m (Ehrlich 1965, 327). (Barker 2006)

Also:

Moving to sexually reproducing *plants*, there is even greater evidence of gene flow being unnecessary for species cohesion. Typically, either wind or insect pollination facilitates sexual reproduction in plants. One might assume that wind facilitates reproduction over vast distances, “but this assumption is not borne out by the available data” (1229). For example, outcrossing in *Zea mays* (corn) is only 1% at distances greater than 18 m, and in *Beta vulgaris* (beets) is just 0.3% at distances beyond 200 m; likewise among Coulter pines, successful dispersal does not reach far beyond 10m (Ehrlich and Raven 1969, 1229). Consequently, Futuyma (1998, 317) has summarized that “a large number of crop species have shown that fields separated by a kilometer or more are effectively isolated in most cases.” (Barker 2006)

What this clearly demonstrates is that certain species, even when they are within breeding range, sometimes don't interbreed, yet still maintain a distinct, cohesive identity. More data that Barker reveals shows that there are even cases where species do interbreed rather extensively, yet this is not sufficient to hold the species together as a cohesive entity.

For instance:

Templeton's (1989) work shows even high rates of gene flow between populations can fail to bring populations into a cohesive whole. This is clearest in the case of *syngameons*, which are *collections* of what most biologists would consider to be populations of distinct species. Despite being of different species, however, populations in a syngameon collective frequently overcome *pre-zygotic*

reproductive barriers. Thus, there is gene flow between distinct species in the syngameon, and yet this is insufficient, even over significant periods of ‘evolutionary time’, to unify the corresponding species, populations, and organisms into one unit that displays a single instance of species cohesion. (Barker 2006).

So, where does this leave the Biological species concept? In a rather precarious position. It is clear from the above that gene flow is neither necessary, nor sufficient for species cohesion. Of course, this in no way discredits gene flow as a vital aspect of species cohesion<sup>6</sup>. It simply makes the case that to posit gene flow and isolation as primary factors in the definition of species is misguided, and fails to take into account, even by simple inspection of the literature, the true complexity of the evolutionary picture.

## 8. Naturalized Metaphysics

Throughout this thesis, in my grappling with the species problem, I have been primarily concerned with one area of philosophy, and that is metaphysics. The species problem is first and foremost a metaphysical problem. It involves all those elements and topics which have been at the heart of metaphysical discourse for many hundreds, and even thousands, of years; things such as identity over time, whole-parts relations, individuals versus classes, realism vs. nominalism. While all of these problems are in some way related, they are distinct problems, and all of them are present when dealing with the species problem. I have shown throughout this thesis how both

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<sup>6</sup> An important point to remember is that the Cohesion concept by no means wants to remove gene flow from the species problem dialogue, but rather, it wants to place it within a context in which other forces function to maintain species identity

antiquated and contemporary species concepts fail in some way because of problematic metaphysical assumptions, whether it were Aristotle's essentialism and classes-based ontology, or Mayr's reductionist operationalism. All of our species concepts up till now have suffered to varying degrees because of residual unchallenged metaphysical assumptions from previous eras. In this chapter my aim is to outline the core metaphysical concepts which have guided me to my conclusions about species. I will discuss a new approach to metaphysics which rejects the a priori nit-picking of most analytical metaphysics, and proposes that philosophy does more to align itself with current, testable science. Much of my thinking about this has been drawn from Ladyman et al.'s book *Every Thing Must Go* (2007), and so I will be referring to it quite extensively. As a helpful starting point to their ideas I provide the following quote:

It is Ladyman et al's claim that modern metaphysics has become woefully isolated from actual science. Metaphysicians are left arguing over issues long resolved by actual science. For instance, Ladyman et al draw attention to the debate amongst over the nature of matter, specifically the nature of matter as either 'gunk' (in the sense of an infinitely divisible substance) or atoms (atoms being partless particles). This debate, according to Ladyman et al, 'is essentially being conducted in the same terms as it was by the pre-Socratic philosophers among whom the atomists were represented by Democritus and the gunkists by Anaxagoras...It is preposterous that in spite of the developments in the scientific understanding of matter that have occurred since then, contemporary metaphysicians blithely continue to suppose that the dichotomy between atoms and gunk remains relevant, and that it can be addressed a priori.' (2007, 20). (Grant 2011).

Here we have a good summation of the problem. Much analytic metaphysics has failed to make progress because it fails to engage with what is really going on in the scientific world. This is, essentially, the core of my critique of modern species concepts, but I will deal with this in more detail later. For now let us unpack the idea of Naturalized Metaphysics.

The idea of a naturalized metaphysics is nothing new. Naturalism is found in the thinking of many philosophers exploring concepts like identity, time, space, etc. throughout the history of western philosophy. Two notable examples of this are John Locke, and Wilfred Sellars. Both of these philosophers advocated in some way the practice of metaphysics by way of empirical, or scientific, knowledge. Probably Sellars' most well known idea is that of the distinction between his two images of the world, i.e. the Manifest image, and the Scientific image. Loosely speaking the Manifest image of the world is the one which man employs by way of common, or instinctual sense [1]. It is, according to Sellars "the framework in terms of which man came to be aware of himself as man-in-the-world" (Sellars 1963). At the centre of the Manifest image are people and things (deVries 2011). It is in employing this framework that we navigate our world on an everyday basis. Contrasting with this image of the world is the Scientific image. The nomenclature of these images is misleading as the Manifest image needn't be unscientific, although it is more promiscuous in its methodology than the Scientific image. What makes the Scientific image scientific is not necessarily its methodology, but rather the fact that it is based on the content of actual, practiced science; that is, the real theories and empirical data that scientists devise and collect (deVries 2011)

Sellars is not a strong advocate for the Manifest image, as he says, “[The Manifest] image, in so far as it pertains to man, is a 'false' image; this falsity threatens man himself, inasmuch as he is, in an important sense, the being which has this image of himself” (1963). Instead, he defends the Scientific image. He acknowledges the possibility of both being in some ways correct, but dismisses it on grounds which I shall not go into here. Essentially, Sellars believes our pursuits as philosophers should not be aimed at ascertaining truths through the Manifest image, but rather by “knowing our way around” the sciences and the Scientific image.

A useful analogue to Sellars’ images is given to us by Ladyman et. al. (2007). They speak about the “domestication” of the hard sciences as being the dominant practice of metaphysicians up till now. What this practice consists of is translating, or making sense of, our theoretical progress in the sciences in terms of common sense, or folk, understandings of the world (2007). This practice, they feel, is misguided, the result of which has been a lack of progress and understanding in the field of metaphysics. Our intuitions very rarely match up with the science. For instance, a common sense understanding of matter is that it consists of smaller and smaller bits of other matter. So elements are comprised of atoms, which we see as nuclei orbited by electrons (in much the same way as the moon orbits the earth), and these atoms are comprised of other smaller bits, which are in turn comprised of other smaller bits. The science of subatomic particles, however, shows that atoms and their constituents are not anything like matter in the way we conventionally understand it. Subatomic particles do not break down into smaller parts in the same way that a building breaks down into bricks and steel. The subatomic world is something which eludes a common sense understanding. It is so strange that attempts to visualize it are often impossible to most people. A possible explanation for this is that human beings did

not evolve to have an understanding of the atomic or subatomic realm. We evolved to deal with objects and things at a human scale (2007). Our intuitions about the behavior of things at this scale work fairly well most of the time, but when it comes to very small or very large objects, our common sense fails us dismally.

Collier (2010b) has argued that one could make a case for reconciling these two images. While this may initially sound problematic for my position, I will show here that it in fact strengthens it, mainly because it demonstrates a methodological approach which naturalism must adopt when comparing two competing paradigms.

Collier (2010b) argues that the Sellars' two images are instances of distinct paradigms in the Kuhnian sense. Reconciling these images requires that we make explicit the tacit assumptions in each so that we can compare them (Collier 2010b). He also argues that neither image can be justified by its own standards, and that criticisms of each generally come from the other, according to the standards and framework of the other (2010b). I am agnostic regarding whether or not the images are, in fact, reconcilable. What I take as important in Collier's argument is the claim that tacit assumptions must be made explicit in order for us to compare them. This methodology is distinct from the misguided methodology of previous metaphysicians who simply try to translate the Scientific Image into the Manifest Image. This process of explication is an essential part of a naturalistic framework, as it reflects the empirical impulse which is the cornerstone of naturalism. It is my belief that just as a naturalistic metaphysician must try to make the necessary connections between various scientific fields, he must also always be aware of the possibility of reconciling common sense and scientific concepts, so long as the

methodology is a suitable one. A truly naturalistic metaphysics must remain agnostic regarding the reconcilability of the two images. This possibility of reconciliation is a hallmark of naturalistic metaphysics, and not a weakness. It is in this mode that I have proceeded in trying to make sense of how our previous species concepts have failed us; namely by trying to make explicit the tacit assumptions in each, and how each has fallen prey to certain positivistic tendencies from previous eras. In the case of species, common sense notions need to be expelled, as species function primarily within a scientific framework, so reconciliation is not necessary, although it is necessary to point out how “Manifest” notions of species have stilted progress in our understanding of them. As I have argued elsewhere in this thesis, despite the recognition by species concept theorists in modern times that species are, in the end, entities tied to a specific scientific theory of evolution, the tendency to treat them in an a priori, intuitive manner is still not lost. Mayr’s Biological Species Concept may insist on its being strictly naturalistic, or biological, yet it ignores a whole host of factors which contribute to evolution and speciation, turning itself into a tautology in order to evade criticism. I argue that these tendencies are a hangover from previous tacit understandings of species which we have yet to recognize and thwart.

The other naturalist I mention at the beginning of this chapter is John Locke. His thinking on the nature of Real and Nominal essences should shed more light on our understanding of how a naturalistic metaphysics might differ from an analytic one. Locke, in his *Essay Concerning Human Understanding* (1690) made the distinction between Real and Nominal essences. The Real essence “is what makes something what it is, and in the case of physical substances, it is the underlying physical cause of the object's observable qualities” (Jones 2013). The Nominal

essence, on the other hand is “an abstract idea that we make when we identify similar qualities shared by objects; the nominal essence is the idea of those shared similarities.” (Jones 2013). One could, however crudely, make an analogue between Locke’s Real essence and Sellars’ Scientific image, as well as between Locke’s Nominal essence and Sellars’ Manifest image. Even though Sellars is dealing with overarching world views, and Locke with individual objects, there is still something to say for how they relate in similar ways to our discussion of naturalized metaphysics. One could make the comparison between how something’s Nominal essence falls within a folk, or common sense framework, or understanding of the world. So, the idea of a lion, for instance, invokes a number of ideas about what a lion is in the mind of the common person using the word, i.e. a mammal of such and such a size, with a mane, a golden colouring, sharp teeth, etc. This is the Nominal essence of a lion, but it is also an understanding of a lion as part of the Manifest image of the world; our common sense or intuitive understanding of a lion. The Real essence of a lion, however, refers to the *cause* of how lions came to be lions, that is, a distinct species. This is a scientific issue, and so fits within the Scientific image of the world. A lion came to be a lion through a complex process of evolution involving a number of mechanisms acting over a very large timescale. The word “lion” as understood in a Nominal or Manifest way is not the same as it is understood in a Real or Scientific way, although in this particular case, the referent does appear to match up.

I will argue here that the Cohesion concept of species aims at the Real essence of species, and not the Nominal. I also contend that previous recent species concepts, although earnest in their aims, end up hitting upon Nominal essences of species. As I have argued before, this is because of the existence of residual “attitudes” towards species which we have inherited from the past.

One of these “attitudes” is a kind essentialism, in which we seek to find a set of criteria that we can use to bestow species-hood upon a certain population or groups of populations. The Aristotelian version of this was the seeking of certain characteristics of organisms which are essential to its being that thing. In more modern times, despite explicit efforts to reject it, kind essentialism crops up in the form of operationalist definitions of species. So, the essential characteristic of a species under Biological species view is that its members interbreed (or can potentially interbreed). This is thought to be essential to what gives that species its unique species-ness. The temptation to look upon this view as reflective of a Real essence exists because it is a causal definition. But, as I have argued previously, the rejection of other possible main causes of evolution means that the definition becomes tautological and operational, and so begins to resemble essentialism in the Aristotelian sense.

The Cohesion concept itself, to be clear, is a type of essentialism, though not in the Aristotelian sense. Again, Locke’s distinction makes this clearer. Strict definitions of things (Essences) are, for Locke, always Nominal. That is, they follow the usage of the word, and not the underlying causes which make something what it is. We cannot sum up the Real essence of a thing in terms of an explicit definition. As soon as we do this we are committed to a nominal description. So, for Locke, species definitions like the Biological one, or the Mate Recognition one, are always nominal as they get at definitions of words, and not the things themselves (the underlying causes). Such definitions are based on things which we can observe (I would extend this to measure, too). On the other hand, the Cohesion concept aims at the Real essence of species. It does away with the existence of a set of criteria for defining a species, and looks to the causal mechanisms which are responsible for giving that species its perceived Cohesion. Each species

‘essence’ must be found empirically (moving from the standpoint of the dominant theory, which is at this time evolutionary theory). Thus each ‘essence’ for each species will be unique. Of course, limitations do exist, as with Nominal essences, we are still constrained by what we can observe. We can mitigate these difficulties, however, by positing unobservable theory, which is precisely what the Cohesion concept does.

## **9. Essentialism, Individuals, and the Real Essence**

The Cohesion concept of species is, as I have mentioned before, an essentialist doctrine. It is not, however, essentialist in the same sense that Aristotle argued for it. The essentialism I argue is inherent in the Cohesion concept is a causal essentialism, one which we can be understood as similar to Locke’s conception of the Real essence. In this chapter I will be making the case for why the Cohesion concept is essentialist. Next, I will make a distinction between essentialism as commonly understood in the literature, and the essentialism I think best describes that found in the Cohesion concept. I will then show how this essentialism is not a weakness of the concept, but rather a strength; how essentialism is a mark of good scientific practice and not bad. The kind of essentialism which I propose in the following chapter has a great deal to do with causation, and so I will need to say something about that, too.

The Cohesion concept is essentialist because it is making claims about the underlying nature of species. It is as simple as that, although not quite. It is not enough that it makes claims about the nature of species, as claims about the nature of species can be that they have no underlying essence. The exact claim that the Cohesion concept is making, however, is that there are various

underlying mechanisms involved in giving species their properties, and therefore their distinctness from other species. It is the result of these mechanisms interacting with each other that ensure that a species remains distinct. Without such forces, and the interaction of these forces, the species would fail to exist in its distinct form. This is, clearly, an essentialist way of thinking about species; there are certain properties, or facts about the world, which are necessary (essential) to the existence of such and such an entity.

The way in which this type of essentialism differs from more common forms of essentialism, I believe, can best be explained using Locke's distinction between Real and Nominal essences. Locke was an anti-essentialist in the Aristotelian sense. Traditionally, Locke has been understood to be opposed to the idea that entities in the world have any essence:

That *essence, in the ordinary use of the word* (my emphasis), relates to sorts, and that it is considered in particular things no farther than as they are ranked into sorts, appears from hence; that take but away the abstract ideas by which we sort individuals, and rank them under common names, and then the thought of anything essential to any of them instantly vanishes: we have no notion of the one without the other; which plainly shows their relation. It is necessary for me to be as I am: God and nature has made me so: but there is nothing I have is essential to me (Locke 1690).

He also goes on to say:

An accident or disease may very much change my colour or shape; a fever or fall may take away my reason or memory, or both (Locke 1690)

Locke is here talking about essence in the ordinary use of the word, that is, as I understand it, the Aristotelian sense of the word. There are no properties which an individual has which are essential to it in this way; that is, properties which are perceived through direct observation.

None of these [properties] are essential to the one or the other, or to any individual whatsoever, till the mind refers it to some sort or species of things (Locke 1690)

For Locke, properties of things are only essential as far as we divide them into classes. These classes are nominalistic and man-made, and therefore not essential to their existence or to their nature.

All of this may seem to contradict my assertion that the Cohesion concept is essentialistic as well as real. Surely an essentialist concept of species cannot claim to be real if no property of things is real? Not so if we see the Cohesion concept as referring to underlying, causal properties of species, and not simply the properties expressed in individual organisms. Locke speaks of this as the Real essence of a thing, and places it in opposition to the Nominal essence. Still, Locke did

say that Real essences belonged to types of things and never individuals. So, while the Nominal essence of gold might be a substance of such and such a colour and such and such a weight, the Real essence (that thing which causes, or gives rise to these properties) still belongs to the whole class of gold, and individual instances of gold have no essence. This is a contentious issue, namely for the status of individuals in relation to essences. It is an important consideration for myself and this thesis as my claim is that species are individuals *and* they have essences. Some have interpreted Locke as being open to this idea, as Jones says:

On the other hand, if real essences are only for sorted individuals, then there are no real essences for individuals until we have classified them by a nominal essence; until an individual is sorted, it only has an internal constitution, not a real essence. If one adopts the relativized real essence interpretation of the *Essay*, then there are no real essences for unsorted individuals. If, however, one adopts the unrelativized real essence interpretation, then it appears possible that Locke could think that unsorted individuals have a real essence and not just an internal constitution. (Jones; 2013).

I tend to agree with Jones's unrelativized real essence interpretation, but I would go further than that and argue that Locke probably would have endorsed a view of unsorted individuals having real essences had he been privy to a lot of modern philosophising on individuals. Denying that an individual can have a real essence places them in murky ontological water. Surely, if a real essence is something which gives a thing its thing-ness, and is independent of our knowledge of it (indeed, real essences are, according to Locke, mostly unknowable), it wouldn't make sense

for it only to exist in conjunction with a nominal essence. Surely it must exist independently. Even conceding that not all individuals are essence-less, only unsorted ones, the sorted individuals only receive an essence in as far as they belong to a type, and not in terms of their numerical uniqueness. How is it that Locke deals with individuation if not through essences?

### **9.1. Individuals**

An individual thing can only be pointed to. This is what distinguishes them from classes. Classes are groups of things which share certain common properties or characteristics. Classes can be grouped according to properties which are not, by necessity, contained within the group; that is, they can be defined in a non-circular way. In a grouping of vehicles all possessing the colour white and named “Bright Cars”, we have an independent definition of the colour white, which we can then look for in other cars to see whether they fit into the group termed “Bright Cars”. With individuals, there is no no-circular way of defining them. There is no way one could comprehensively outline every property of an individual, and then understand that collection of properties without explicit reference to the individual itself. An exhaustive list of properties for an individual thing cannot be the essence of that individual, because in outlining these properties we have simply re-described the thing itself. This is similar in many ways to Locke’s distinction between simple and complex ideas. Simple ideas come to us through experience only. We cannot define such ideas as, according to Locke, a definition is “the showing the meaning of one word by several other not synonymous terms.” (Locke 2004).

We haven’t got the cause of why that thing is distinct from other things. For instance, one could

try to define me by saying “Russell is a male of 26 years old, born in Durban on such a date, with blood type x, height y, weight z, to such and such parents, with the following moral code, having experienced such and such an experience on such and such a date...” until you have exhausted every property of me (assuming such a thing is even conceptually possible). One would still not have arrived at my essence. This collection of properties is not what causes me to be me, it *is* me.

Locke was anti-essentialist in the sense that he believed that we cannot know what the real essence of an object or type is. That is, we cannot know the deep internal constitution of an object which gives it its properties. Locke’s skepticism is epistemic, not ontological, and he admits himself that all things must have some kind of essence; something which gives them their being:

[An essence] may be taken for the being of anything whereby it is what it is. And thus the real internal, but generally. . . unknown, constitution of thing whereon their discoverable qualities depend, may be called their essence. (Locke 1928)

The problem is, according to Locke, that we cannot know what these things are (or rather, we cannot observe them, so we cannot use them in our classifications). I have spoken at length already about the importance of unobservables in the form of theory, and it is my feeling that unobservables such as these provide a sufficient bridge to making Locke’s real essences a viable way of approaching classifications. A successful scientific theory provides a causal explanation

for the way things are, thus giving us a way into its real essence. The epistemic problems that Locke faced at the time are greatly diminished today.

Essentialism, as I have argued before, stems from good scientific practice, not bad. Aristotle's essentialism, whilst outdated, made use of his contemporary understanding and theories about the world. He did the best he could with what he had. One reason for the abandonment of theory and causation and the embracing of phenetics and pattern cladism in the biological sciences, was the need to reject a certain kind of essentialism from cropping up in our scientific practice. It was thought that positing any kind of theory in our classification of the natural world was done a priori, and therefore, in many ways, at random. The need for a classification system based purely on empirical methods was what was apparently needed. Essentialism, however, is not the enemy here, if, like I have said, we seek out the real essences as posited by Locke. Such essences are only attainable, however, through the introduction of theory, which gives us a causal explanation for the way things are. Essentialism of this kind is necessary if we want to avoid the pitfalls of other approaches such as extreme micro reduction (Collier et al. 1996)

The gist of my argument defending naturalized metaphysics is that we should not throw out the impulse to introduce theory; rather, we should amend the theories. The alternative, as history has proven, leads us down paths which are not useful

So, what are the relevant causes involved in giving a species its distinct-ness from other species? It is important to remember here that different causes apply to different types of things when it comes to individuation. For instance, for most physical objects in the world, their particular

spatio-temporal properties give us the ability to distinguish between them. For other things in the world (say, consciousness, or personality), spatio-temporality gives us very little to go on. Likewise, in order to individuate species, we must be clear about the relevant causes of their distinctness. In this case, as I have stated previously, they are the various mechanisms relevant to the theory of evolution. It is this cause, which, when (or if) discovered gives us the Real essence of that thing. We discover the essence of the individual once we have embarked on empirical research. Of course, it may be the case that the thing in question has no Real essence, and is therefore not a distinct thing at all, or perhaps, it has a certain degree of distinctness or individuality. We must start from the position of hypothesis about observed entities and then seek to find the causes which give it identity. Sometimes we may find that there are no real, causal forces behind its identity, and so its characterization is purely nominal.

## **Conclusion**

As can be seen from the preceding arguments, species concepts are ubiquitous and varying and because of this a great deal of confusion abounds. My solution is to adopt a naturalistic approach to metaphysics, and adopt a species concept (the Cohesion concept) which is mindful of actual scientific theory. There are a number of different forces involved in the process of evolution; a process which drives the increasing diversity we see in the living world. A suitable species concept must bear all of these processes in mind, lest we fall into the trap of defining species in an unhelpful way.

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