

The Reality of Species: Real Phenomena Not Theoretical Objects

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INTRODUCTION

Many scientists and philosophers have said something like “species are the units of evolution” or “the units of biodiversity”; it is even the title of some well-known books on the subject (Ereshefsky 1992; Claridge et al. 1997). What does this even mean? In this chapter I argue that species are real, phenomenal objects rather than objects of any biological theory, let alone of evolutionary theory. *Species* is a term which everybody thinks they understand, but which nobody agrees upon, to denote the “basic units” of groups of biological organisms. Philosophically, there have been three stances taken in the past fifty years: the *species essentialist* view that all organisms of a species must share some (usually genetic) properties (Devitt 2008); the *individualist* view that species are historical objects without essences (Hull 1976, 1978); and the *homeostatic* view that species are coherently maintained objects due to some causal processes (Boyd 2010).

In addressing the impact of evolution upon the concept of species, we must first ask what *are* the units of evolution? The ontology depends a lot on what theory is being employed. When talking about population genetics, the basic units, of course, are the *allele* and the *locus* (Griffiths & Stotz 2007). When talking about development, then the unit is the *organism*, as it also is when you are talking about ecological interactions, although “species” is used here as the term for a class of ecologically exchangeable organisms, that is, organisms that play the same role in the local ecosystem (Wilkins 2007a, 2007b). Although organisms are pretty well all different (which is the point of population genetics), for the purpose of trophic webs (the food webs of ecology), conspecifics are treated as being interchangeable elemental units. Then there are the larger units of evolution: *populations* (Godfrey-Smith 2009), and the particular revision of that concept, the “deme” (Winsor 2000). A deme is basically the population that can interbreed—the term in the equations of population genetics is “ N_e ,” the number of effective, or reproductive, individuals. However, non-breeders also play a role in many

species in contributing to the fitness of their kin, by helping raise them, or finding food, so the ontology here depends solely upon what the research issues are. While “population” is itself somewhat fuzzy (Gannett 2003)—the more migration there is between two (sexual) populations, the more they start to look like a single population—it is a theoretical object (Millstein 2006).

But species? No biological theory *requires* them. True, ecologists and conservation biologists use the word *species*, but what they are doing is using field guides as a surrogate for the ecological roles played in an ecosystem by individuals of the species who are more or less normal—the “wild type.” Likewise, medical and biological researchers do the same thing with their model organisms. *Mus musculus*, or the common mouse, is used as a model because it is assumed that each individual member of that species shares the same properties (developmental cycles, phenotypes). But in practice they use “strains” that are specially bred to see the effects of gene knockouts, for example. The “objects” here are the genetic strains and the organisms (Ankeny 2000). Systematists use the word *species*, but the explanations given of species *being* species are manifold. The notion of a “gene pool” or “metapopulation” is the foundation of one such explanation (de Queiroz 2005, 2007). But the theories used, the explanations, are not theories of species; they are theories of gene exchange, reproduction, fitness, adaptation, and so on. Species are being *explained*; they do no work in explaining.¹

So, there are two ways we might go if species are not theoretical objects. One is that we may deny that species exist, and a lot of people do this, and have done since the *Origin* and even before, Lamarck being the most prominent. I call them *species deniers*, although the usual term is *species conventionalists*, or *nominalists* (both philosophically and historically misleading terms). A version of species denial is to replace the term *species* with some “neutral term.” *Deme* was one of these, but it got subverted by population geneticists for the meaning given above. Other examples include *operational taxonomic units* (Sneath & Sokal 1973; Sokal & Sneath 1963), *least inclusive taxonomic units* (Pleijel & Rouse 2000), *evolutionarily significant groups* (Hey 2001), and so on. In each case, the term *species* came or is coming back into use. Why is *species* so durable?

The second alternative is *not* taking the term and concept as a theoretical term. *Species* is a useful term because species are real phenomena.² That is, they are things observed that call for explanation, they are *explicanda*. Theories of biology explain why there are species, although the same theories do not apply in the same way for all particular species (Wilkins 2003). Biology is not that neat. Some species are explained in textbook fashion through the acquisition of reproductive isolating mechanisms formed in geographical isolation; some are not. There are species formed by hybridization,³ by sexual selection, and of course asexual or mostly asexual species that are maintained, as I argue elsewhere (2007a, 2007b), by adaptation to niches.

If there is no general theoretical account of species, why do we have this category? It might be because we tend to name things that look similar to us. This is what species deniers think: it is all about us and our cognitive dispositions, not the things themselves. However there *are* some general features of species that license us calling them all species: *species are salient phenomenal objects*. They are salient not because of our perceptual tendencies alone but because they do exist. They are a bit like mountains. Each particular mountain is caused by definite processes, but every mountain is not caused by the same processes. We *identify* mountains because they are there. We *explain* them with theories

of tectonics, vulcanism, or (if they are dunes) wind. Analogously, species are clusters of genomes, phenotypes, and organismic lineages. We explain them because they need explaining. A species is (roughly) where the lineages of genes, genomes, parent–child relationships, haplotypes, and ecological roles all tend to coincide. Not all of these need to coincide in every case, but so long as most of them do, they are species, and we must give an account of them.

PHENOMENAL OBJECTS

Perhaps the most crucial practical aspect of the species concept debate lies in its relevance to conservation, but it is not the most theoretically interesting. Biology, like most sciences, has a need for units of measurement which are grounded in the real world. So *species*—the “rank” of biology that is agreed on most sides as the most or only natural one in the Linnaean hierarchy—determines many measures of biology in fields from genetics to ecology. If, as a significant number of specialists think, the rank is a mere convention (Mishler 1999), then those measures become arbitrary and meaningless. Therefore we need to consider what sort of “unit” a species might be. I can think of three alternatives.

The first is that species are, in fact, simply *a matter of convention*, which is to say, something that makes things convenient for us in communication, just as John Locke said in the *Essay* (Bk III, ch. V, §9; although that was about *logical* species, not biological species).⁴ Instead, claim researchers like polychaete specialist Frederick Pleijel (1999) and geneticist Jody Hey (2001), we need to replace the notion *species* with something like a “least inclusive taxonomic unit” (LITU; Pleijel) or “evolutionary group” (Hey).⁵ There are other replacement concepts in the offing. And the so-called “phylogenetic species concept” is not really a concept of species, at least in one of the versions under that name, so much as something very like a LITU that gets *called* a species (Wheeler & Platnick 2000).

The second alternative is that *species* plays a theoretical role in biology, and this seems intuitively right: we talk about species as the units of evolution, so they are supposed to be required by evolutionary biology; and likewise in ecology, species are the unit that is crucial in defining the biodiversity of a region or ecosystem. But if species are theoretical objects, we ought to find them as a *consequence* of theory, not as a “unit” that we feed into theoretical or operational processes, and this is not the case. Population genetics and evolutionary theory have populations, haplotypes, alleles, trophic nodes, niches and so on, but what they do not have are species. In every case where species are *used* in theory, they are primitives, or stand as surrogate terms for the other things mentioned. Theory does not define species. This might be challenged by adherents of Ernst Mayr’s biological species concept, or one of the derivative or related conceptions—a species is a protected gene pool, as Mayr said (1970: 13). However, the vast bulk of life would not be in species if that were the case, and, anyway, species were well described and identified long before genetics was developed, up to two centuries before. So they must at least be things that can be observed in the absence of theory.

The third alternative is that species are not theoretical objects at all; they are *objects that have phenomenal salience*. That is, we do not *define* species, we *see* them. Consider

mountains again. Mountains are hard to define, and they have a multitude of geological causes, ranging from uplift, subduction, vulcanism, differential erosion, and so forth. “Mountain” is not a theoretical object of geology—subduction zones, tectonic plates, and volcanoes are. A mountain is just something you *see*, although there are no necessary sets of properties (or heights) that mountains have to have, and it is often vague when differentiating between them. A mountain calls for an explanation, and the explanation relies on theory, but equally so do mesas, land bridges, and caves.

So the proposed answer to the question “What is a species?” is that a species is something one sees when one realizes that two organisms are in some relevant manner the same. They are natural objects, not mere conveniences, but they are not derived from explanations, they call for them.

THEORY-DEPENDENCE AND DERIVATION

Traditionally, a theoretical object was something that theory required or employed but which was not empirically ascertainable—“electron” *c.*1920, “gene” prior to 1952, and perhaps still “Higgs boson.” But this is a positivist sense of *theory*—a formal system in which objects are either observational or theoretical. Whether or not one is now a logical empiricist instead of a logical positivist, objects are much more nuanced than that. There is a school of thought that treats scientific ontology, the set of objects that one thinks exists in a domain, as basically the bound variables of the best theory of that domain, that is, Quine’s “to be is to be the value of a variable” (Quine 1948: 32). In this case, species would be theoretical objects if they were such variables of a theory. But if they are not, we need to establish what sort of ontological status they, and other phenomenal non-theoretic objects, may have.

Consider planetary orbits. Observed and debated for a very long time before Newton proposed a general physics that accounted for them (and made predictions about them), orbits were demoted by Newton from theoretically important objects (heavenly spheres) to special cases of larger and more universal physics. “Planetary orbit” is thus a special instantiation of astrophysical dynamics, which aperiodic comets, star systems, and even entire galaxies obey. Even if no orbits actually existed (and we can perhaps envisage this in some world) under this physics, the movements of objects would be still covered by Newtonian dynamics. Likewise species. They obey the biology of populations, interbreeding, selection, drift, and so on, but they are not theoretical objects, any more than planetary orbits are in physics.

This chapter’s general characterization of species is that they are the nexus of the coalescence of genes, haplotypes, parent–child lineages and so on, at or about the same level. In abstract terms, species are these coalescences that are distinct from other such coalescences, something I have called the *synapomorphic* conception of species (Wilkins 2003), and each and every one has a general set of properties and modes of speciation, and a unique set of these that only they have (the synapomorphies, or shared characters, which are causally active in maintaining separation). Because each species is a unique historical object, this makes its *modality* (as I have called it) something that they tend to share only with those taxa they are closely related to. As a result, the modality of a species is as much an evolved trait as having a vertebral column or a nuclear membrane. The

causal process whereby a species evolved and is maintained depends upon shared ancestral traits such as developmental machinery, genetic sequences, and ecological resources. While these may be very similar between related species, they cannot be expected to be the same in each case—not all Rhagoletids will speciate by host race transfer, for instance—and so each species will have a special set of causes. No theory will capture all and only these causes (not every sexual species will be caused by allopatric isolation, and not every asexual species will be caused by a single niche adaptation) except at a level of generality that is so vague as to preclude explanation in terms of mechanisms. As a take-home exercise to the reader, try to imagine under which conditions organisms like ours would not form species at all.

There have been several proposals for what makes an object “theoretical.”⁶ The first is that of Quine: something exists just to the extent that our best theory of a given domain requires them (i.e., binds them with a quantifier). On this view, species are simply not theoretical, and indeed do not exist, because if I am right that no theory of biology *requires* species, then they are never the value of a bound variable in any model of biology.

Another similar but not so restrictive view is “Ramseyfication,” according to which what a theory requires is based on a formalization of the theory—a “Ramsey sentence” (Psillos 2006; also called a “Carnap-Ramsey sentence” or a “Ramsey-Lewis sentence,” see Koslow 2006). Objects exist so long as they are represented either by primitive terms (values of variables, or constants) of the theory or combinations or derivations of those. A primitive here might be empirical, so that species might be primitives of biology, but are not themselves explained by it. This is not the case with species, though, because in every such case of which I am aware, one can replace “the species $X y$ ” with something like “a local population of $X y$ ” or “organisms that behave in a way that is typical of $X y$ ” for functional accounts such as ecological ones. In other words, the species $X y$ is replaceable with objects that the theory actually employs. The Ramsey approach, recently also called the “Canberra Plan” (Braddon-Mitchell & Nola 2009; Jackson 1998), treats these objects as non-objects. Sometimes this is played out under “Structural Realism” in which a theory structure is true (Psillos 2006), but the objects it poses which are “unobservable” may or may not be real, so long as the theory is empirically adequate in other ways. This is irrelevant here.

What makes an object theoretical, and are there other roles objects and their representation play in science? For our view to work, it must be that there are objects that are described by the theory, which in the domain of that theory have a certain coherence or unity as objects. Mechanisms like tectonic drift are obviously theoretical in that sense. But mountains are more difficult. Mountains are real things, but the category as a whole lacks theoretical coherence. That is, a mountain has no theoretical place qua “mountain,” but as a particular mountain, say, Mount St Helens or the Matterhorn, it calls for explanation. If you have to map them, travel around them, or climb over them, they are as real as anything can be, but the choice of demarcation between peaks can be conventional or even just something that perception hands to us on a plate. Nothing in theory demands that this particular mountain exists, or even that there are mountains. On a planet with an atmosphere and no tectonics, after a reasonable period, there may be none. Species are like that. They are real facts about the world, which we perceive rather than define. Of course, this makes identifying them relative in a way to the rules

and capacities of perception. If we had poor vision, we might not “perceive” mountains until we had telescopic surveyor’s sights. Once we have that technology, though (which, note, does not rely upon the theories of geology), we *do* see mountains. Similarly, we may need to use all kinds of assay techniques to see species, but when we have them they are seen.

An instructive example is the discovery by Murray Littlejohn and his advisor of the different species that had previously been called *Rana pipiens*, the “leopard frog” of the southern United States (Littlejohn & Oldham 1968). The leopard frog is widespread, and Littlejohn was using a new piece of equipment designed for speech therapy, the sonograph, to graph the mating calls of these frogs. He discovered that there were up to six distinct mating calls (although new species have since been identified: Platz 1993). Since mating calls in amphibians are highly species-particular, Littlejohn proposed that *R. pipiens* was a species complex in which morphology and ecology were indistinguishable, but that mating was restricted within the mating call groups, the species. Subsequent work proved this to be the case. The differentiation was always there, but you needed the right assay technique. This is not species being “constructed” or any other “postmodern” nonsense. While the *concept* we have of those species is being constructed (and reconstructed as new evidence comes in), the concept denotes realities, either of classes of things that are theoretical, such as populations, haplotypes, genes, developmental sequences or cycles, and so on, or of things that are not required by the theory. When we construct a concept, we are *learning* about the things we describe. It is like finding that Everest has a hitherto-hidden peak that is even higher. Our concept of Everest changes, but the thing itself was already as it is.

WHAT ARE SPECIES?

First of all we must take issue with the entire way the debate has been framed over the past 150 years or so and assert: *There is only one species concept* (Wilkins 2011). That is to say, there is only one concept that we are all trying to define in many ways, according to both our preferred theories of how species come into being and maintain themselves over evolutionary time and what happens to be the general case for the particular group of organisms we have in our minds when we attempt our definitions. The former case is what we might call *theoretical* conceptions of species, where a “conception” is a definition of the word and concept of species. The latter are the *prototypical* conceptions of species. If you work in, say, ichthyology, then your conception of species has to deal with the usual facts about fishes (Rosen 1978). If you are a fern botanist, then those organisms set up your prototype (Wagner 1983). The debate over what species are has been driven by differing prototypes as much as by different theories of speciation. Elsewhere (Wilkins 2009), I list some twenty-six conceptions⁷ of species in the modern (post-Synthesis) literature. I am going to focus now on the few basic ideas that underlie nearly all of these.

The first conception is based on *reproductive isolation*. Since the Synthesis of genetics and Darwinian evolution was formed, the ruling notion of species generation (speciation) was based on the criterion of sexual populations that are isolated from each other, so that they evolve in divergent ways, leading to populations that when they meet, if

they do, in the same range, they no longer tend to interbreed, and their gene pools are now distinct over evolutionary time scales. The conception of species that the Synthesis adopted as a result of this genetic-evolutionary view is sometimes misleadingly called the *biological species concept* (or BSC). It is called this because it was contrasted to the practices of museum taxonomists, who identified species based on differences in the morphology of captured or collected specimens. This was held to be a sterile methodology where the data were more in the heads of the taxonomists than in the real world. Hence, the BSC was biological, while the museum approach was conventional (due to the conveniences of the taxonomists). But the leading idea of the BSC is not that things live in messy populations, although that is part of it, but rather that these populations are *reproductively isolated* from each other. So I prefer to call this conception the *Reproductive Isolation Species Conception* (RISC), or “isolationist” conception for short.⁸ There are several versions of it, but the basic idea—that something inhibits interbreeding when they meet—is common to them all.

Criticisms of the RISC began early (Ehrlich 1961). For a start, it was observed that there was a disconnection between the theoretical justification for the RISC and the ways in which taxonomists who adopted it did their taxonomy. To be confident that you *have* a RISC taxon, you really need to do breeding experiments. Many quite diverse morphs in, say, butterflies, that were identified as distinct species in the 19th century, turned out to be different genders of the same species. “Aha!” said the isolationists, “this is a failure of morphology.” But when similar cases occurred and were found to be different genders *before* the Synthesis, these so-called “morphologists” had no problem seeing them as the same species on that ground. It was understood that form was only a guide to the underlying biological reality, not an end in itself. Worse, isolationists themselves use morphology to identify their species. Breeding experiments, even when technically possible, take enormous time and resources. So while *theoretically* isolationists are basing their work on reproductive isolation, *practically* they are doing just what their supposed mistaken predecessors did. This might lead us to think that the older workers were not so silly after all.

The second of our broad conceptions of species is *ecological isolation*, and is often called the *Ecological Species Conception*, which goes back in one form or another to Linnaeus. However, it acquired currency in modern times when another Swedish botanist named Göte Turesson did some studies during the 1920s of plant morphologies in different ecological conditions (Turesson 1922, 1925, 1929). Turesson coined the term *ecotype* to describe these differing morphologies. He distinguished between ecotypes and *ecospecies*, which were populations prevented by adaptation to a particular ecological niche from interbreeding. In the 1970s, Leigh Van Valen offered a new version, based on the fact that American oaks will freely interbreed, but that the ecological types remain constant (Van Valen 1976). In these cases, the “species” is effectively maintained by the ecological niche. Similar cases are common in plants and single-celled organisms, though less so among multicellular animals. Bacteria and other single-celled organisms which do not often exchange genes may be entirely maintained by this. Lacking sex, they cannot be RISC species, so Turesson coined another term for them, *agamospecies* (“sexless species”). However, in animals asexual reproduction has evolved from sexual species many times (*parthenogens* “virgin origins”), while in plants it is much more common (*apomicts* “apart from mixing”).

The third kind of species conception is known variously as *Morphological*, *Typological*, or *Essentialist*, but these labels are misleading. Sometimes it is called *the Linnaean Conception*, because it is supposed to be the default view before genetics and evolution were discovered, and hence the view of Linnaean taxonomy. This is a bit unfair—Linnaeus never clearly defined a species concept,⁹ and the standard view at the time was that of John Ray, in which a species was twofold: a *form*, which is *reproduced*. This conception was never isolated from normal reproduction by parents. (Moreover, Linnaean and Rayesque species were not defined by essences either, but that is for another time.) The important thing was that it was the overall organization of the organisms that defined them as a species, so long as it was reproduced. Ray's own "definition" was "the distinguishing features that perpetuate themselves in propagation from seed" (quoted in Mayr 1982: 256). Ray's definition was designed to cover plants, but he explicitly extended it to animals, and it was the first time any naturalist had ever given a purely *biological* definition of "species." It was not based on Aristotle or any logical system, but on observation. This earlier definition remained the standard view at the time Darwin began his work, via the authority of Baron Cuvier.

A fourth general conception is based on the convenience of biological work, including mutual communication between specialists: *species conventionalism*, the view that, as Locke had said, species are made for communication, and nothing else. For Darwin, species were real but temporary things, and he believed there was no special rank or level in biology that was unique to species. Contrary to common opinion since the turn of the 20th century (and earlier, vide Agassiz), Darwin was not a conventionalist, but evolutionary thinking made it harder to be exact about species.

This leads us to our final conception. Based on *evolutionary history*, it has two main versions: the *phylogenetic* species conceptions based on cladistics, and the so-called *evolutionary* species concepts, which are often a mixture of the RISC, the ecological species conception, and phylogenetic accounts of reconstructed history. The former are often more like the RISC, because they rely on there being separation of lineages over a large time as defined by their sharing or not evolved traits, and this implies genetic isolation. The latter do not rely on RISC, but only that after the fact the lineages remain distinct for whatever reason. These conceptions are process-based, and are equally as non-operational as the RISC, but cladistics at least has a large number of mathematical and formal techniques for drawing up their cladograms. The problem is that, without some way of saying what the level of separation is for species, cladistics can divide lineages up to a very small level (such as haplotype groups), leading to "taxonomic inflation" (Isaac et al. 2004; Padial & de la Riva 2006). Phylogenetic species can run to as much as nine or ten times in number compared to the ordinary ("Linnaean") kind. The debate rages through the modern systematics community.

After all that, what is a species? Any *universal* concept of species has to range over the entire evolutionary tree, but the modes of being a species will depend on what ways they have evolved to remain distinct from each other. Hence, none of the particular conceptions is sufficient or necessary to cover being a species in all organisms. *However, each conception only tells us what species sometimes are. They do not tell us why the things so defined should even be called "species."* For example, RISC proponents will often say that asexual organisms (*agamosppecies*) are not really species at all, because they lack the defining properties of species which is, of course, reproductive isolation

(Fisher 1930: 135; Dobzhansky 1951: 275; Mayr 1942: 122). So we should call them something else—agamospecies, quasispecies, pseudospecies, paraspecies, etc. This has the unwanted consequence that the bulk of life does not exist in species, but only those few clades that happened to evolve sex. I think we should say that *all* organisms come in “kinds,” some of which are sexual kinds. Others come in genetic bundles or are clustered for ecological reasons, and many are a mixture.

So here is a “definition” of the word *species*:

A species is any lineage of organisms that is distinct from other lineages because of differences in some shared biological property set.

It has to be a *lineage* in order to distinguish biological species (but not just RISC species) from species of chemical compounds, minerals, and symptomatic diseases. However, while all species are lineages, not all lineages are species, not even the monophyletic ones. It has to be a *causal* definition, because formal approaches do no explanatory work (in short, the formalist definition merely restates that there are differences). And it has to be based on *biological properties*, because non-biological properties like range or geography are not enough to include or exclude populations and organisms. All the various conceptions try to give the differences in shared biological properties some detail—and when we look at them that way, it becomes clear why none of them are sufficient or necessary for *all* species: the mechanisms that keep lineages distinct evolved uniquely in every case, and so generalizations cover only some, not all, of life.

PATTERN RECOGNITION AND ABDUCTION

We tend to classify epistemic activities into two kinds: induction (regarding which controversies over its warrant are well known) and deduction (with many arguments about its applicability). But there is something else that we do to learn about what exists in the world: *classification* (Wilkins & Ebach 2013). Classification is typically regarded as either of the other two kinds of inference, but it is, instead, a third kind, similar to abduction, or inference to the best explanation.

When we classify in a theory-lacking domain we are not yet inductively constructing theory, and we are not able to deduce from theory (since there is not any yet) the classes of objects in the domain we are investigating. What is happening here is *pattern recognition* (Bishop 1995). It is one of the distinguishing features of neural network (NN) systems, such as those between our ears, that they will classify patterns. They do so in an interesting fashion. Rather than being cued by theory or explanatory goals, NNs are cued by stereotypical “training sets.” In effect, in order to see patterns, you need to have prior patterns to train your NN.

Where do these training sets come from? There are several sources. One is evolution itself: we are observer/classifier systems of a certain kind. This gives us a host of cue types to which we respond by training our stereotype classifier system. For example, we respond to movement of large objects, to differences in color and shade, and so on, in our optical system. The problem is that so long as our survival and reproductive success is ensured, evolution cannot guarantee us access to the way things “really” are. At best it

gives us a good balance between false positives and false negatives. It is good enough, as it were, for government work (Godfrey-Smith 1991); but is it good enough for science and metaphysics?

Science proceeds by refining its categories of what exists in the world based on two main sources: evidence and explanatory force. In the case of a domain of investigation for which there is as yet no explanation, all we have is evidence; but apart from our evolved dispositions to respond to certain stimuli (called our *Umwelten* by Jakob von Uexkull (1926)), how can we identify the salient aspects of evidence? There is an almost infinite amount of possible information we might use, and so we must glean the *right* sources of information.

One source is economic necessity. Over time, farmers and hunters will tend to respond to the features of the things they are engaged in acquiring and using that are more or less important for success, because those features which are not salient will impose a cost of time and effort that tends to reduce success. This is a process very like natural selection, and has been the basis for what came to be known as *evolutionary epistemology*, in which a parallel process to biological evolution occurs in the domain of knowledge. Cognitive traditions become better at acquiring reliable knowledge because ideas and approaches that do not aid this goal are costly and are abandoned.

However, we have a superfluity of cognitive and conceptual resources. We can retain ideas and practices that are not really natural for social reasons, such as rituals and “explanations” that have no counterpart with the reality being dealt with. The fact that a particular culture is successful at farming by relying upon a ritual calendar (as in pre-Indonesian Bali) does not warrant belief in Hindu gods. The functional aspects of the rituals act to transmit the information even if nobody in the culture fully understands why those rituals make farming successful (Lansing 2007). When a classifier recognizes patterns in economic circumstances, what counts is not the conceptual superstructure, the theories, and the ideologies, but the categories of what matters—in this case of water, soil, and landscapes. How might this explain the success of science? Taxonomists are classifiers in a particular economic context: that of professional science. When a taxonomist encounters organisms in the wild, they are in the same situation as a hunter who hunts in that ecology. To succeed at taxonomy, as to succeed at hunting, the agent must know the *right* things about the target objects. A hunter who does not know what different species of bird look like or how they behave or where they live is in exactly the same economic conditions as a taxonomist who also lacks knowledge. Neither will end up with dinner on the plate (qua hunter or taxonomist). In the case of the taxonomist, the gap between failure and hunger is somewhat more distal than for the hunter (but hunters typically get most of their food from foraging rather than hunting anyway, courtesy of the non-hunters, mostly women, in their village), but ultimately economic success depends directly upon correct pattern recognition. Mayr was fond of telling the story of how when he visited Papua in the 1930s, he and the local hunters identified the same species of bird, with an exception where Western ornithologists also disagreed, and he used this as justification for the reality of those (and all) species. He inferred that science was able to discover kinds of things that were real in the world. However, when E.O. Wilson tried the same experiment about ants, a subject he knows intimately, instead of the locals counting the same species he did (several dozen), he found they did not discriminate them (Wilson 1992: 39). Why did Mayr’s informants know their birds while Wilson’s

did not know their ants? The answer is that birds were of economic importance to locals and ornithologists, while ants were of economic importance only to Wilson and other myrmecologists.

By “economic” I do not mean fiscal, but the acquisition of resources, success at which gives the person investigating a living. What distinguishes scientific success is a unique socioeconomic system of professionalism, credit in society, and access to funds and resources like labs, students, and equipment. The motivations of the individuals concerned are several, often (but not always) based on personal curiosity, but curiosity is not enough if you do not get the resources to do the work. So we are very good at turning our perceptual pattern recognition systems to scientific work. What evolution provides, science refines. It happens that pattern recognition and the subsequent classificatory activities can deliver reliable knowledge of the world when it matters. But given that it is parasitic upon those evolved capacities, and given that scientists are social organisms, this is not without its failures. Social influences, particularly the inherited traditions of ritual and conception that history bequeaths, can skew and bias our categories about the world. This is where theory and experiment come in.

Science, by way of its historical accidents, also seeks to explain things in ways that can be tested. Here the ordinary philosophical issues come into play—we inductively generalize based on the patterns we have recognized, and form hypotheses, and from those hypotheses we derive deductive consequences, which we can test in ways that are not circular, which do not rely upon our original observations. And so we can eliminate hypotheses that do not fit the facts, more or less. This is what Karl Popper and the evolutionary epistemologists built their views upon. What evolutionary epistemology never explained, nor Popper, was how we came up with those hypotheses in the first place. Pattern recognition does.

CONSEQUENCES

This approach makes sense of several facts about biological science.

It explains why we recognized species well in advance of there being anything remotely like a theoretical explanation of them, from the 16th century onwards. Ray formally defined biological species for the first time in 1686, but his view was implicit in the work of natural historians going back to Aristotle and Theophrastus. Genetic and developmental accounts of species did not arise until around 1900.

It explains why when replacement terms are proposed for *species*, they tend to settle on the same sorts of phenomena, and eventually *species* makes a comeback. It also explains why it is that when autochthonous peoples employ organisms economically, say by hunting or raising them, they recognize the same sorts we do for scientific reasons (Atran 1987). Things are phenomenally salient if you have to interact with them.

But most of all it explains something about science. I would like to briefly sketch what I think are the implications of phenomenal objects in the ontology of a science. In the traditional view of science, observation is theory-dependent and objects are theoretical. I am proposing that some objects are *not* theory-dependent in the domain under investigation, and in doing so I can explain why it is that so much of biology is what Rutherford sneeringly called “stamp collecting.” Before you can begin to formulate theories, you have

to gather together the objects under explanation and organize this information into a taxonomy, otherwise it is not even clear what the domain of the theory *is*. The traditional view of science of the 20th century ignored classificatory activities as uninteresting; I am suggesting it is one of the crucial and essential aspects of a science.

One might object that of *course* these objects are theoretical: to observe them is to identify a difference by measurement, and that implies an assay or methodological protocol. This is usually true, although species and mountains do not need much if any theoretical ancillary assumptions. But the point is that they do not need the *theory under investigation* in order to be phenomenal objects. That is, if they are theory-dependent, they are dependent on theories outside the domain in question. Moreover, they are often tokens of a class of phenomenal objects that call for explanation in *those* theories as well (consider optical theories, or genomic clusters in genetic theories).

Since the dependence here is a general kind (such as for optics), the theory-dependence is benign. With respect to our theory *T*, there is no special dependence on which the observations are being made, so the phenomena are *T*-independent. This does not mean there is such a thing as *completely* naive observation—nobody ever starts from total naivety or from a *tabula rasa*. Even observers in the mountains of Papua New Guinea are informed by prior ideas and experience. But we can say they observe species, and do not thereby need to define them.

To summarize:

1. A species is something that forms phenomenal, salient, lineages of populations of organisms and genes.
2. A species can have a particular mode based on evolved biological properties.
3. The species *conception* applied in each case depends on whether that species meets the conditions for that conception.
4. Each species is a phenomenon that calls for a conception and an explanation.

So we do not need to have a monistic or singular definition of *species*, because species are things to be explained, they are *explicanda*, not an a priori category or rank into which every biological organism must be fitted.

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NOTES

1. One possible exception is the work species do in “species selection” theories (Jablonski 2008; Rice 1995; Gould & Lloyd 1999), but it is arguable whether these are actually theories as such, and equally arguable whether the properties that are “theoretical,” which play a role in causal explanations, are those of the

- species, populations within the species, or the individuals or kin groups. If species selection is taken to mean that species whose members have a particular property (like eurytopy) tend to speciate more often, then “species” in this sense is merely a mass noun (Grandy 2008).
2. This is not to deny that *species* is also maintained by conventional and social practices. If entire volumes are dedicated to describing species, anyone who wishes to be taken seriously in that field has to refer to those described objects.
 3. For example: Knobloch 1959, Mallet 2007, 2008; Arnold & Meyer 2006; Wagner 1983.
 4. This was the view of John Maynard Smith as well (1958).
 5. Brent Mishler (pers. comm.) has suggested that a cladistic taxonomy requires only the *smallest named and recognized clades* (SNaRCs). This is not a replacement term for *species* however, as there is no rank beyond what has been recognized as monophyletic.
 6. See Brittan 1986; Ladyman 1998; French & Ladyman 2003.
 7. A conception is a variant definition of a concept. This in turn is distinct from the various formulations of conceptions of the concept. For example, other researchers have given several hundred formulations (Lherminier & Solignac 2000), but mostly these are comprised of a much lower number of conceptions.
 8. The historical account of this terminology, which derives from Ernst Mayr, is given in my 2009 book.
 9. Although he did use an interbreeding criterion (Müller-Wille & Orel 2007), and so may be charitably interpreted as holding a RISC.

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