50 words for snow: the category of phenomena in science

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Abstract
Scientists and philosophers routinely talk about phenomena, and the ways in which they relate to explanation, theory and practice in science. However, there are very few definitions of the term, which is often used synonymously with "data", "model" and in older literature, "hypothesis". In this paper I will attempt to clarify how phenomena are recognized, categorized and the role they play in scientific epistemology. I conclude that phenomena are not necessarily theory-based commitments, but that they are what explanations are called to account for, which are not presently explained.

KEYWORDS
Phenomenon, category, linguistic determinism, classification, species, constructionism, theory-dependence, realism, observation, explanation, constrastive explanation, starting problem

1. Introduction

In a well-known and generally debunked story, Inuit people have around 50 words for snow. Or so the argument by anthropologist Franz Boas goes[1]. In fact, people who engage with the phenomena of their environments often make distinctions that those who rarely or never engage in the same way with those phenomena don't. Snow is a salient aspect of boreal environments; it matters whether the snow is packed, or loose, falling or not, just as those in less severe environments make distinctions about varieties of rain. Are these special terms or just adjectival nouns? How much categorization of the world is too much, or too little?

The idea that our experienced world is constructed from our linguistic distinctions is an old one, going back to Wilhelm von Humboldt in the early nineteenth century (and possibly much earlier, all the way back to Plato, (Wolff and Holmes 2011)). Sapir (a student of Boas') and Whorf (Sapir's student) proposed a thesis, well-known as the Sapir-Whorf hypothesis, that our categories determine how we experience the world. This is known as *linguistic determinism*, either of our cognition, or our ontology, of the world. The ruling notion here is that ideas (or linguistic structures and terms) specify our experience.

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[1]Boas documented 60 words for ice, snow and related phenomena; and the debunking itself may need debunking. See Krupnik and Müller-Wille (2010)

Science finds itself dealing with this issue constantly. I am most interested in *species* concepts, but similar issues arise with *gene* (see Portin (1993)), *individual* (see Wilson and Barker (2017)), niche (Colwell 1992; Leibold et al. 1997) and even such fundamental terms as ecology, environment, and even development (Keller and Lloyd 1992). And that is just in biology. Chemistry has its categorical issues (Scerri 2007), as does physics. Even mathematics has it (Lakatos 1978).

Why is this? Science is supposed, or so I was taught, to be increasingly specific and exact in its terms. Each term is refined and redefined to apply to classes of phenomena that are real, causal, and important theoretically. We know that the social sciences have the problem: terms like *religion*, *society*, *folkway*, *class*, and so on are routinely held to be constructed kinds, and terms like *gender*, *race*, and other identity terms have a strong political overtone as well. And yet, categorical terms persist. Why?

There are a few explanations on offer. One is that our language evolved for functional, usually economic, reasons. We identify the categories we do due to something akin to Marx's notion of *false consciousness*, in which we speak of the world in ways that are imposed for socioeconomic reasons (which serve the interests of the powerful). Let us call that the Marxian explanation. Another is that we identify the categories that suit our metaphysical, ontological or theoretical worldview. I call that the *Weltanschauung* explanation[2]. Both have their defenders.

The Marxian account is often employed by those called postmodernists, or structuralists. It suggests that concepts are relative to interests. Change the interests and you change the concepts. Such conceptual relativity gives primary causal roles to the conditions in which ideas develop. It is often used to suggest that truth is a matter of functional coherence in a social sense. The truth of the scientific image (Van Fraassen 1980) is not comparable with the truth of, say, the magical thinking of the Asante people in West Africa. They are in their own ways equally "valid".

The *Weltanschauung* account implies that our best theory in science, religion or economics (which is neither) licenses the categorization of the world. In short, as Quine once wrote, to be is to be the value of a variable in some theory. This implies, as Ludwig Fleck (cf. Löwy 1988) and Thomas Kuhn both noted (as part of another old tradition in philosophy) that if your theory changes, so too do your commitments to what is in the world. And moreover, these commitments are not commensurate. Newton's use of the term mass is not the same as Einstein's. This is an essentialistic account of categories: the theory defines the class in terms of intensional theoretical properties. This is also known as scientific realism and holds that things exist only if they are aspects of our best theory.

Both of these views are set against the empiricist view, which was widely held until the nineteenth century, despite Hume and Kant rejecting it. This held that one merely has to observe the world in order to categorize it[3]. Pierre Duhem, the physicist of the late nineteenth century, attacked naive empiricism in favour of his Kantian view that phenomena are determined by one's theoretical concepts (Duhem 1954, chapter II), and this became the default view after the collapse of Logical Positivism in the 1950s. The logical positivists held there were two languages in science—one based upon observational operations, and one based upon theoretical concepts and properties. This theory-observation dichotomy failed in the fact of the highly theoretical nature of observations in, specifically, physics. Phenomena were not "ready-made",

[2]Tied to the *Weltansichten* of a national language by Humboldt.

[3]Discussions of "phenomena" are widespread in the nineteenth century and earlier, for example, but there are few to no specifications of the term. Newton, for instance, simply lists unsolved problems in his *Principia* under the heading "Phenomena"

but constructed.

Constructionism of categories is thus the consensus among philosophers of science, just as it was among anthropologists and linguists for a time. Sometimes it is social construction, sometimes it is theoretical construction. Empiricists are naive. In recent years, though, empiricism has been making a comeback. In particular Michaela Massimi (2008a; 2011) has argued that while constructive empiricism such as van Fraassen's fails, the distinction made by Bogen and Woodward (1988) between data and phenomena is correct:

> Thus, the metaphysical framework is close to that of experimental realism, whereby (i) phenomena such as weak neutral currents exist in the world 'out there'; (ii) they manifest themselves by causally producing data such as bubble chamber photographs, which we then (iii) learn how to recognise from other data due to background noise via reliable procedures. (2011, 103)

Massimi offers what she calls "scientific perspectivism" in which

> Knowledge claims in science are perspective-dependent when their truth-conditions (understood as rules for determining truth-values based on features of the context of use) depend on the scientific perspective in which such claims are made. Yet such knowledge claims must also be assessable from the point of view of other (subsequent or rival) scientific perspectives. (2016, 13)

Hence, while a perspective depends upon the theoretical issues and rules of a discipline or investigation, it must be cross checked by other perspectives. Massimi holds this is a kind of realism.

If claims to know the reality of things depend on our prior knowledge in this way, by cross checking from other fields and theories, can we ever say that we do really know things? Isn't it the case that all we know is what coheres with our experiences and our existing knowledge? Are our categories of the world just social constructions? This debate has raged for decades among linguists, philosophers and social scientists. Those who go full constructivist say that each culture, or even each individual, has their authorities, sources, beliefs and religions. In other words, it's perspectives all the way down.

Those who go full realist, though, want to anchor our categories in hard facts, universally accessible and confirmable. Sure, we have perspectives, but get enough of them together and you converge upon the joints of nature (appealing to Plato's "cut nature at its joints" comment, *Phaedro* 265e). Popper once approvingly quoted Churchill about the mathematical prediction of a solar eclipse:

> You . . . look, and your sense of sight immediately tells you that their calculations are vindicated . . . We have taken what is called in military map-making 'a cross bearing'. We have got independent testimony to the reality of the sun. When my metaphysical friends tell me that the data on which the astronomers made their calculations [of an eclipse] were necessarily obtained originally through the evidence of their senses, I say 'No'. (Popper 1972, 43)

Such cross bearings are held by Popper among others to converge upon a theory-observation pairing of the world that indeed cuts at the joints. Theory comes to present a structural description of the world that breaks things into their real objects, classes and relations (this is known as "structural realism", Psillos (2006)). This way, we develop a number of categories of things like atoms, fields, orbits and other aspects of the world-as-it-is, even if incompletely or partially. Or so the story goes.

Both of these accounts may be true of some categories, but I doubt they hold for

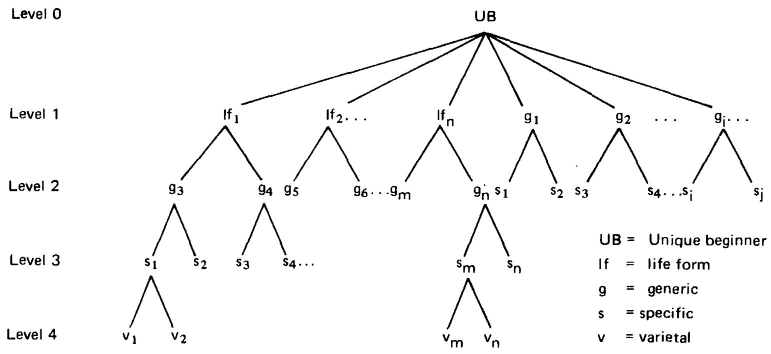

Figure 1. Schematic relationship of the five universal ethnobiological taxonomic categories and their relative hierarchic levels in an idealized folk taxonomy.

all of our scientific categories. In fact, even when categorical structure converges, it retains a constructed aspect (why wouldn't it? The terminology of science is hardly discovered "out there"), and no constructed category that is empirically inadequate, as van Fraassen noted, is scientific.

But there is another way we gather our concepts and categories. It is termed "folk" science. For instance, living kinds are usually distinguished by cultures in a fairly predictable fashion, as Berlin and his collaborators have shown (Berlin 1973, 1976, 1992; Berlin et al. 1973). According to this view, traditional societies begin with what they term a "unique beginner", then "life form", then generic, then specific, then varietal (see Figure 1 from Berlin et al. (1973)). This closely approximates Linnaeus' kingdom–class–genus–species–variety hierarchy.

Similar folk hierarchies have been proposed by Bulmer and Tyler (1968) and Scott Atran (1985; 1990; 1998; 1999). Moreover, folk psychology and folk rules for weather prediction and so forth have similar hierarchies, which itself is a reason why the hierarchies of traditional logic held such sway, as they formalized folk categorization and practices. Now, in the case of ethnobiology (or ethnopsychology, etc.), clearly these shared categories are not based on science and theory, and yet again and again such categories have been shown to have close correlations with scientific categories (and of course some surprising differences). Moreover, Linnaeus and his predecessors named species, in the absence of much scientific theory, that have remained species up to 500 years later. In my book (Wilkins 2018) I document cases of medieval classifications being "natural", in the sense of being categories still accepted in modern biology. If empiricism is false, and theory is absent, how did these categories come to be? Is it just accidental? Or *are* the phenomena ready-made in some non-trivial sense?

By focusing on the theory–observation dichotomy so exclusively, philosophers of science have tended to overlook the phenomenality of categories in both wider culture and science. Initially, when a field has no theory to speak of (other than ancillary theories in related or technical disciplines like optics), the guiding principle for categorization is experienced observation. To illustrate with an anecdote or two:

A few years back I happened to host a world-renowned American bryophytologist in Sydney, so I took him to the location in the Blue Mountains where Darwin had gone while on his *Beagle* voyage. I expected naively that he would find the vistas breathtaking and the sense of history would be his focus. Instead, we hardly got out of the carpark, as he found some liverworts with gametophore stages. He thanked me profusely, much to my consternation, for taking him there. I could barely see them except as background plant-like things on the car park embankment.

A year or so later, a well-known coleopterist, specializing in a group of beetles that have many representatives in Australia, visited from the US, and so his host and I took him out into the bush. As we wandered, I was impressed that he not only knew where to look for the beetles, but that he could even see them in the undergrowth and litter. I couldn't. To him, they leapt out in his field of vision —he had the search image in his head and literally saw them as patterns in the mess. In some ways, I was like James' newborn child, in a blooming, buzzing confusion of experiences[4], while he was an expert observer, filtering out the noise to spot the targets. In a later trip I got to see something of how he did it, but not much. I lacked the professional learning and experience.

In many ways, this is like the ways a traditional hunter hunts. A trained hunter sees the prey even when camouflaged or obscured. They know where to look and what to look for. Successes in the past, and folk lore, reinforce those observational techniques that will find the natural types of things being hunted[5]. A forager is the same: they see the indications of useful plants and animals that can be used for food, medicine and cultural purposes. If your living depends on getting the natural world (mostly) right, experience tends towards the right phenomena.

There is not much in the way of theory here. Neither is the observation naively empiricist, nor is it just evolved predispositions, or else I would have had no problems at least seeing the differences that identified these types. This is culturally-scaffolded and experienced observation that converges, out of necessity, on natural phenomena. There are several aspects here. One is that of course observation of any kind relies initially upon our evolved predispositions to respond to certain types, scales, and duration of phenomena. We do not respond observationally to very slow processes, which we tend to normalize as the "natural" state of affairs (akin to the supposed quote of Einstein's, that "Common sense is nothing more than a deposit of prejudices laid down by the mind before you reach eighteen"). We do not respond to the very rapid either, treating it as noise or static. But at the mesoscale, we do respond observationally to a good many natural phenomena, which are handed to us by our evolved senses, and these can be truth tracking even though they evolved, mostly, as fitness enhancers. When our fitness depends on getting observations right, we evolve truthful senses.

But that is not enough. No single set of observations or the experience of a single person is going to engage with enough of the world to develop what we might call "well-formed categories". Evolution sacrifices false negatives on the altar of false positives (Wilkins and Griffiths 2013; Griffiths and Wilkins 2014). So a different process aggregates and selects out the experiences of many: cultural evolution. We are trained by our peers, and the expertise of others is passed on, sometimes as rules, yes, and definitions, but mostly as setting up the framework in which individual learning, trial and error, will home in on the "right" categories. Thus, three factors determine observing phenomena: biology, culture and individual experience. Each selects out as much error as it is worth eliding, but the cultural and individual experience is faster and much more efficient at removing error than biology, so long as the success is sufficiently cogent. Finding food, for example, is much more significant than finding a decorative feather.

All that is (conceptually and historically) before science even enters the race. However, it explains why folk taxonomies are often quite robust in the light of the conceptual selection processes of technically advanced sciences; as a first cut, folk taxonomies

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[4] James (1890, 488)

[5] Sterelny (2003) refers to this kind of cultural cognition as "scaffolding".

are not too bad at identifying real phenomena. If they were, people would die, or lose fitness overall. But ethnotaxonomy is not foolproof; which comes as no surprise, and it can be overturned by cultural factors if the costs are outweighed by the benefits. To give the classical example, let us consider Bulmer's classical paper: "Why is the Cassowary is not a Bird?" (Bulmer 1967). Bulmer noted that of the New Guinea tribe, the Karam, that

> . . . at the smallest units which Karam discriminate, the 'terminal taxa' . . . Karam show an enormous, detailed and on the whole highly accurate knowledge of natural history, and that though, even with vertebrate animals, their terminal taxa only correspond well in about 60 per cent. of cases with the species recognised by the scientific zoologist, they are nevertheless in general well aware of species differences among larger and more familiar creatures.

However, he said, at the upper level of classification

> . . . objective biological facts no longer dominate the scene. They are still important, but they allow a far greater, almost infinitely varied, set of possibilities to the taxonomist. This is the level at which culture takes over and determines the selection of taxonomically significant characters.

For cultural reasons, the cassowary, which walks on two legs but does not fly, is regarded by the Karam as "human" and thus is forbidden to be killed or eaten. Clearly cultural exigencies overcome the natural in this case. But, and this is not often noted, adopting the arbitrary categorical standards of one's culture, particularly when it costs you some missed opportunity, is a good way to enhance your standing in the community by demonstrating your commitment, and it raises your fitness (*vis á vis* social aid) more than it lowers it (in terms of lost protein); so it is not surprising that this occurs.

Consequently, we must expect that while a culture will categorize the environment in which it exists in ways that to a degree track truth (that is, delineate real phenomena) not all the natural categories of a culture will do so. Inuit may have multiple terms for snow based on the ways they interact with snow, but the having of multiple terms doesn't mean that the categories they name are somehow dividing nature at its joints.

I just used the term *phenomenon*. What does that mean? The etymology gives us a clue: it comes from the Greek word *phainomai* (φαίνομαι), meaning "appearance to the senses". Leaving aside phenomenological philosophy, which is mostly about the subjective elements of cognition, in our context, this means a phenomenon is not self-standing. It is a two place predicate: P appears to O. The world has many, possibly an infinite number of clusterings of things. A phenomenon is one that an observer O observes. Now a phenomenon can be something that exists, such as when I note that doors permit egress when open but not when closed. Or it can be something that bears no truthful relation to the world, such as when I see eyes in leaves while on LSD [which the Greeks called *phantasia*, (φαντασία)].

Scientific phenomena tend to eliminate the latter kind in favor of the former. A phenomenon in science relies on a good observational system as well as real clusters of properties in the world. So what counts as a scientific phenomenon is not defined by the categories of folk science/culture/psychology. Instead it depends upon the use of scientific instruments, techniques and methodologies, as well as the use, when available, of theories. If we get 50 words for snow out of that, we can have a reasonable confidence that we are delineating natural properties, given that these operations of science have been honed over time to be successful at exactly that.

2. The economics of cultural categories

If experienced observers are trained to observe natural phenomena in their environment, *pace* the "interference" of cultural accidents, what is it they observe? As I mentioned before, we are not born into a world of ready-made phenomena. William James referred to the sensory world of a newborn baby as a "blooming, buzzing confusion":

> Experience, from the very first, presents us with concerted objects, vaguely continuous with the rest of the world which envelops them in space and time, and potentially divisible into inward elements and parts. These objects we break asunder and unite. ... The baby, assailed by eyes, ears, nose, skin and entrails at once, feels it all as one great blooming, buzzing confusion; and to the very end of life, our location of all things in one space is due to the fact that the original extents or bignesses of all the sensations which came to our notice at once, coalesced into one and the same space. (James 1890, 487–488)

But if the world is divisible in so many ways, why do we divide our experiences in the ways we do? What makes some aspects of our immediate environment salient? That is, why do some stimuli have more importance than others for us? That there are phenomena in the objective world is not at issue. But there are an indefinitely large number of possible ways to carve the world up in our categories. We must be able to make a start. What James did not know, in 1890, is that there is a prior set of what one might think of as neurological guidelines for making sense of the world. Mach, and Lorenz following him, referred to this as the "evolutionary *a posterioria*" (Lorenz 1996). These are what Kant called the synthetic *a prioria*, that which we "know" to begin with, but which we cannot have derived from logical truths.

Consider vision. We do not learn to see, we learn to *interpret* what we see. Assuming normal development, the visual system functions at birth. However, control of the system, focusing and the like, and the neural pathways necessary to process the inputs, are not yet developed, and they need to kick off by individual adaptation, or neural plasticity. But what to attend to in the beginning, if there is such a plurality of alternatives? Evolution has provided a number of *dispositions* to attend to edges, motion, and tonal variation (irradiance). James' "objects" are the outcome of these discriminatory dispositions we have at birth.

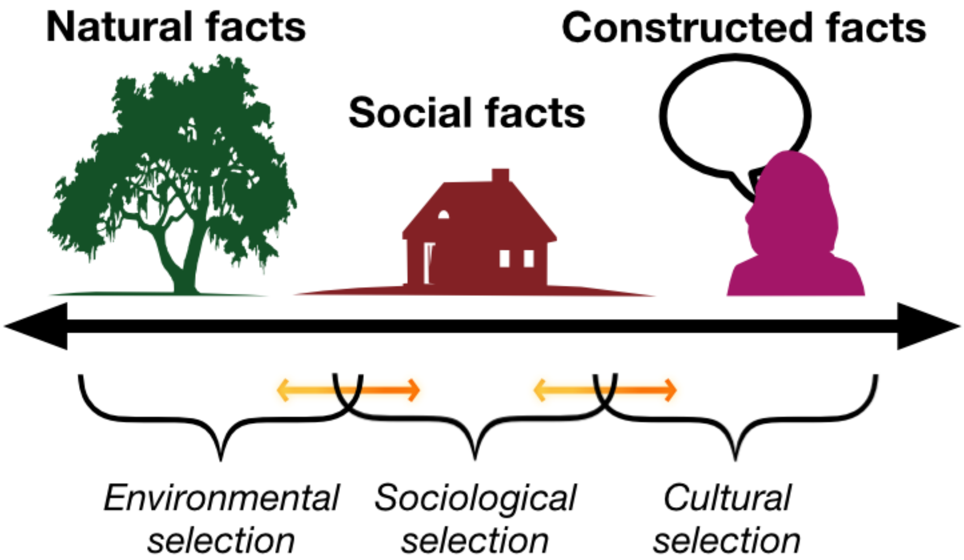
So our dispositions in a way "make" the phenomena we observe. This veers too closely to the "constructed reality" version of our first section, though. It is better to say that a phenomenon P exists as a relation between the observer O, and the environment being observed E. O has a set of prior dispositions that make some aspects of E salient. Some of these are biological dispositions, inherited through reproduction of the organisms. Some of these are sociological dispositions, based upon, yes, language, folk taxonomy, and social institutions like (for example) scientific social practices. And some are simply categories that we are disposed by our cultural biases to slot observations into. We do not construct our natural categories so much as negotiate them with the external world at varying degrees of distance from our individual dispositions. We could do a pseudo equation:

$$P = O[b, s, c] \cdot E[d]$$

where the observer's dispositions of biology (b), societal context (s) and culture (c) modulate with the local environment (based on distance d from O) to produce a phenomenon. The "weight" of each variable determines how "natural" the phenomenon is. Visualized as a continuum, when the variables of s and c are low, and the stimuli are very local, the phenomenon is something more likely to be objective, vis a vis our perceptual apparatus, and hence "natural" or "real". When s and c are high value, the phenomenon is more likely to be a constructed (sociocultural) one. Assuming that the selection process tends to make phenomena more accurate as representations of the environment, we can represent phenomena like this (where "facts" are statements of phenomena; i.e., categories). When selective pressure on the categories is low for s and c, we have a more purely constructed category, as when philosophers come up with a fake category for illustrative purposes ("gavagai"), or when a politician appeals to some ideal that makes no sense (like the unregulated market as a way to achieve the most rational distribution of resources), although once such a category is in play, acceptance of it tracks the signaling aspect of categories, if not the factuality of the content of the ideas they purport to represent.

This is not to say there is a sharp demarcation between sociological and cultural "spaces", nor between the sociocultural and the biological. Instead, human development encompasses all three. Biology proffers dispositions to develop in particular ways, sociology sets the constraints and resources, and culture sets the prior "art", as it were, that developing humans acquire and then set about modifying. Since all three domains, as it were, are in my view about the acquisition and use of resources that affect the fitness of the individuals, there is selection going on at all three levels, and this is what defines them as domains in the first place.

We can illustrate this as if human development occurred upon a single spectrum (of selective pressures):



since trees and other nonsocial facts are not constructed by humans, while houses and other social facts are constructed by social dynamics. Cultural facts are symbolic and passed on through imitation and education. Moreover, what is a sociological fact can affect the nonsocial and cultural and vice versa. Categories about each "domain" are constructed to a degree, but what they are constructed to represent are environmental, and the environment is "natural" (that is, not socially constructed), sociological (constructed to represent social organization and patterns) and cultural (constructed to represent the symbolic milieu). Since humans always have constructed their environment to a degree (the so-called "niche construction" hypothesis applied to humans, Odling-Smee et al. 2003), we are now, and have always been, in our "natural" environment. However, there is another way to conceptualize niche construction: as the individual adaptive environment that buffers development against "strict" natural selection. Organisms adapt individually within the degrees of freedom allowed by their biology. When this has a cost to fitness, natural selection takes over.

So natural selection, social selection and cultural selection represent three layers of selective processes with the consequent differences in the rates of their evolution. However, they are unlinked in many ways: cultural selection can be slower than sociological and even biological—consider biological selection against diseases brought about by trade, a social process, and the cultural changes that entails, for example. This is a complex interplay of evolutionary forces. Still, the variables in my "equation" can still be given values, if we can identify for any given trait, biological through to cultural, what the modal values are. If not—if it is too complex to do so—we can still understand what the forces are to a degree of precision allowed by the particular case.

The Karam category which includes humans and cassowaries, but not other birds, is one example. It is constructed in opposition to natural facts, but adherence to it signals commitment to the sociocultural order of the tribe. Another example is the "uncleanness" of pigs among Jews and Muslims. In fact, the notion of clean and unclean in most cultures serves not as a categorization of the facts about these food sources, although that is the rationale for them, but as an honest signal you are tracking cultural norms. When seen like this (admittedly a gross oversimplification) we can now ask, at last, how it is that science can claim to be approaching, delineating, and explaining, natural phenomena.

3. Species, a case study

> All classificatory terms are impossible of exact definition. Their use always has and always will depend upon the consensus of opinion of those best qualified by wisdom, experience and natural good sense. They will never become stable; we shall never cease to amend, to change, to repudiate old and propose new, because we shall never reach the final summation of science. [S. W. Williston, "What is a species?", *American Naturalist*, (Williston 1908, 184–194)]

We have come to understand that categories about the natural (i.e., the non constructed) world are not based on some naive notion of phenomena or observations, but upon experienced (that is, expert) observations. Nevertheless they can be modulated and biased by cultural and sociological factors. Let me talk now about an example close to my heart: species (and other taxonomic categories).

As is well-known (Wilkins 2018; Zachos 2016), scientists do not at all agree on what species are. Nor, it must be said, do they agree on what counts as sufficient evidence that two organisms are in different species; at least, not all of the time. And yet, the standard view is that species are fundamental units of evolution, ecology, and the other ways that we deal with the biological world. In my book *Species: The evolution of the idea* I argue that the very notion of a "level" of biological taxa such as species is itself the outcome of sociocultural factors—to wit, the need to work out what "kinds" meant in the Noah's Ark story so the logistics could be rationalized. If that is the origin, why does *species* persist among scientists as a category?

As a PhD student, I attended a workshop that included the late John Maynard Smith, and of course I took the opportunity to ask him about species (this was very early on in my studies on the topic, so I had nothing profound to offer him), and to eat half his sandwich. I asked him what he thought species were, and his response, which I dismissed at the time, was that they were merely communicative terms, for the convenience of biologists. I now know this was (in 2000) a common response to the species problem by geneticists, but it shocked me. How could "species" help communication, if they were not referring to anything? If the category was unreal, as he suggested, then what were we talking about?

Other biologists, particularly Brent Mishler, have been saying this for some time: "species" is an unnecessary rank in an otherwise rankless evolutionary process[6]. He, and other species deniers, considered the very category nonscientific. I suggested something similar in 2003, but it was muddled. Against the deniers are the species realists, who argue that species not only exist, but that there is a "level of organization" in biology that answers to "species". I have had papers rejected because reviewers took it as an article of faith that there was such a level of organization not only in this or that group, but across the entirety of biology (except, perhaps, single-celled microbes), and the editors and reviewers would not accept a challenge to this view.

There is a distinction between **category** realism and **entity** realism about species that needs to be attended to. Category species realism is the view that the category is real as well as the individual entities that fall within it, and it is that which I take aim at here (see also Mishler and Wilkins 2018). Entity realism is the view that, individually, the entities (as populations, lineages, etc.) that get called species are real, but the category is not natural. It occurs to me now that Maynard Smith may have been right. We can refer to individual species like *Homo sapiens*, *Mesoneura opaca*, and *Alchorena ilicifolia* (Ereshefsky 1998), even if they are not all of the same category or type. I can have diverse things in my pocket: a coin, a lighter, and a ticket. They do not need to be a natural kind to get referred to as "pocketed items".

It looks very much as though species as a category is not natural in the "natural kind" sense that philosophers have been talking about since Mill. Instead it looks like the outcome of a series of more or less frozen accidents in theology, philosophy, and science, for which retrospective justifications have been given as the needs arise. In short, species is a cassowary—a purely cultural concept. Or is it? Several things mitigate this rabid reductionism. One is that species are often named and retained for centuries, no matter how the science progresses. Many of the species identified in the medieval period remain "good" species, for example, although their nomenclature and arrangement in relation to other species have changed a lot. It seems that good observers can identify things in the unconstructed world even in the absence of theory and method held to be essential to good science. Another is that as I noted in the first part, humans tend to find levels of categorization of the unconstructed world fairly consistently and across cultures and times.

Hence my argument, given in my Wilkins and Ebach (2013), and expanded on in Wilkins (2018), that **species are phenomena**. Given that natural phenomena are a reciprocal arrangement between the unconstructed world and experienced observers, and that we have a tendency to find patterns in the world, this explains why it is that we so often find species in this or that group so obvious, even though there are multiple boundary cases where they are not obvious at all. Species, and by extension to the rest of science, natural categories, are patterns we match in observational data, based on our prior experience of uncontested cases. They need not be, and often rarely are, theory-based; but they are based on the practicalities of doing natural science. In some

[6]E.g., Mishler (2010). See also Mishler and Wilkins (2018).

disciplines, such as physics, one needs to be very theoretical to even observe microscale and even mesoscale phenomena; but that doesn't mean we are forced to be theoretical in the observation of biological, geological or even astronomical phenomena; at least, not all the time.

This goes to the main question of this essay: are the categories of science in any way objective? To answer this, we need to discuss the role explanation plays in science, or at least offer a rough sketch of it.

4. Constructing phenomena

There is a naive empiricist view, held by nobody on close inspection, that phenomena merely present themselves to the observer, and call for explanation. At least since Kant, such a view has been untenable, as Michela Massimi (2008b) has shown. As she notes, it is well understood that phenomena are underdetermined by observational data, and she plumps instead for the following view:

> whenever we have prima facie rival potential causes for the same phenomena, in order to distinguish between them and to determine which entity-with-causal-power has actually produced the observed effect, we must in the end rely on a description of what causal powers/capacities/dispositions an entity is to have so as to produce the observed effects. This description is given by a scientific theory. [Quoted in Massimi (2011), from her 2004]

On the one hand, I agree that we need to have prior understanding of the causal powers that produce our observations of phenomena, but on the other, I do not agree that this is, *necessarily*, described by a scientific theory. To make any sense of that view, and to support my whole pragmatist view of phenomena and explanation, I must needs do a bit of work.

Let us begin with the naive empiricist. He says that in observing the world, certain phenomena are ready made and call for explanation. But the Kantian replies that the naive empiricist must choose what patterns in the data to include in the phenomenon, and what to exclude as irrelevant or noisy. Imagine trying to program an AI to select the "right" patterns to call explanatorily relevant phenomena out of all possible data sources, for instance[7]. Hence, she will say, the naive empiricist has no access to phenomena until he has a theory of causality, relevance, and explanation in that (the phenomena's) domain.

But this leaves us with a **Starting Problem**. Bayesian logic will deal with this by an iterative process of refining the prior probabilities based on new data, (asymptotically?) approaching the correct patterns. But the naive first investigator (not necessarily a naive empiricist) faces a field that *has* no scientific theory on which to draw; no prior probabilities for that domain. How to commence? What should she pay attention to? Bayes suggests an answer, and it has to do with how we extrapolate from the general knowledge we have of the world to specialized domains.

Consider a phenomenon: the precession of Mercury. This is where the orbital perihelion itself moves slowly around the sun. It is as clear a phenomenon as you can find in science, but it would not have been a phenomenon to the Ptolemaic astronomers for the simple reason that they could not observe it without previously having adopted both a heliocentric (or perhaps Keplerian) model of the solar system, and Newtonian physics (as opposed to, say, Descartes' vortex physics). Yet the observation of the precession of Mercury's perihelion could be done without very much in the way of the-

[7] Cf. the "frame problem" in AI. [CITE STANFORD]

oretical knowledge, using measuring instruments that in no way depended upon either theory. It simply was not an anomaly worth noting until the Newtonian/Copernican model had been adopted, and it deviated from the expectations of that model. And even then, it took around 150 years to show up as an anomaly.

With this (admittedly violently oversimplified) statement of the history of this issue, let us draw some tentative inferences.

(1) Astronomers had an account of the causal powers that resulted in the observations – the transmission of light via optical lenses, along with assorted geometrical and mathematical techniques. None of this required theory very much theory. As Hacking 1983 noted, optical techniques were testable without these theories, and the theory of the propagation of light was not finalized until after this.
(2) Measurement is more important here than theoretical descriptions of these causal powers, contra Massimi.
(3) Measurement is something that is independent of the phenomenon, and the domain of explanation, that sets up the anomaly.
(4) The phenomenon calls for an explanation (and possibly, a new theory).

It seems from this example, and many others, that in order to identify a phenomenon, one does need prior assumptions about what is "normal" or expected dynamics of the domain under investigation. How do we acquire these expectations, in order to be able to construct the phenomena? The usual explanation is, as Massimi says, that we rely on prior theory. And this is often, indeed in modern science, usually the case. There's a lot of theory in play in nearly all domains of scientific exploration. Nobody starts an investigation nowadays without a slew of ancillary theory and techniques. So we can concede that to the Kantian.

But the Starting Problem generalizes beyond individual cases of novel investigations. How did science *itself* get going? In the absence of prior theory of any real utility, there has to be a process. I was taught, as an undergraduate, that there was always a prior theory – Aristotle, Ptolemy, Galen, and so forth. But at some point as we move back in time, the meaning of "theory", as a set of models, techniques and predictive results, fades away into religious and metaphysical speculation, superstition, and cultural practices (literally: cultic behaviors like consulting oracles). Did science bootstrap itself into existence? If it did, that is a counterexample to the claim that we use theory to construct phenomena. There are two approaches offered to this issue:

(1) Science evolved from quasi-scientific etiological accounts (origin stories of the gods and the creation of the cosmos); and
(2) We have "theory" from our evolution as cognitively competent organisms.

Neither is all that compelling. Etiologies (like Hesiod's *Theogony*) have a completely different function to natural philosophical investigation. They are moralizing narratives, not empirical tests and studies of the nonsocial world. And to call our cognitive predispositions as evolved "theory" is to beggar the meaning of "theory" so completely that anything is theory, a move I do not like at all. If "theory" has any meaning in science, it must not be watered down to include our disposition to notice mesoscale phenomena that we might eat, navigate, fear or copulate with.

And yet, that set of sensory dispositions is what *does* underpin scientific investigations. Most of our measuring tools are ways to represent at mesoscale what we cannot otherwise see or notice. A telescope and a microscope both present phenomena in ways we can use our evolved sensory apparatus to observe. However, as anyone who has used either of these devices knows, some experience is required to *interpret* what is seen, and the more a measuring device abstracts the microscale or macroscale, the more training it takes to be able to interpret what is measured. Such things as molecular assays, statistical analyses, cloud chambers, and x-rays all require more than naive sensory processing [see note 5 in Bogen and Woodward (1988)].

This is why I have said that a phenomenon is something recognized by observation undertaken by a trained and experienced specialist[8]: it's not just the observing (concurring with Bogen and Woodward) and it's not just based on theoretical description (concurring with Massimi). But *phenomena are something that contrasts with all our prior expectations*, and so they call for explanation. Those prior commitments may include theory, to be sure, but often they do not. That is to say, they do not need to involve theories in the domain under investigation. We all have theories of this or that which set our expectations, or something that might, with sufficient effort, be cast as theories, but most of what we expect comes from the exigencies of interacting with the environment, both social and extrasocial, in order to make a living. That is to say, trial and error, leading to success or failure in some motivated goal.

5. Explanations

> We should consider in each case what *Question* it is that is proposed, and what answer to it would, in the instance before us, be the most *opposite* or *contrasted* to the one to be examined. E.G. "You will find this doctrine in Bacon" may be contrasted, either with "You will find in Bacon a *different* doctrine," or with, "You will find this doctrine in a *different author*." [Archbishop Richard Whately, *Elements of Logic* (Whately 1875, II. iv. i)].

So in order to understand why phenomena are things that call for explanations, we have to understand why explanations are called for, and for that I turn to the Contrastive Theory of Explanation (Van Fraassen 1980; Garfinkel 1981).

Anyone who has ever had a child knows the issue with indefinite "why?" questions. The interrogator asks why for every answer that is given, until the responder gets tired or emotional. And this is not just a problem with preschool age children, but also with scientists. It is for this reason – and indefinitely long chain of questions – that science is divided into investigatory domains, for once one has gotten to the base level of explanations in a domain, to continue to ask why is to hand off the problem to another expert group. This prevents scientists from getting emotional, to a degree.

Science is divided in practice into smaller bite-size questions: why does a peacock have that ridiculous tail? Why does Mercury's precession not match Newtonian predictions? What causes X? Once a question is resolved, experts move onto another tractable issue in the "field". Scientific fields, however, are often merely institutional divisions, and crossing those divisions negatively impacts on the ability of a researcher or research group to gain funding and respect (one reason why interdisciplinary studies are looked on askance). So, the limitations of investigation and explanation are often fairly arbitrary and artificial.

And yet, when a science finds that its division of labor is no longer progressing, such institutional divisions will often evaporate and be redrawn. Molecular biology,

[8] Woodward says "Phenomena are stable, repeatable effects or processes that are potential objects of prediction and systematic explanation by general theories and which can serve as evidence for such theories" (Woodward 2000). I do not deny this definition either.

an amalgam of molecular chemistry and genetics and cytology and developmental biology (increasingly), and so on, is a good instance of this. In this way, sciences adapt to a twofold set of pressures: one being social (or, as I have called it in this series, “constructed”) and the other being natural (“unconstructed”), to produce a set of problems, explanatory resources, techniques and categories that are natural *and* artificial.

In the early 1980s, Bas van Fraassen (1980) and Alan Garfinkel (1981), independently came up with an account of explanation which has come to be known as “contrastive explanation”. Prior to this, explanation was generally regarded as the process of deriving the observed outcome from laws and initial conditions, the so-called nomological-deductive model[9]. Van Fraassen and Garfinkel, however, argued that to explain is to select the best solution from a contrast-class of alternatives for that problem. In short, to explain is to give a relevant answer to a well-defined question Q.

In van Fraasen’s contrastive model (1980, 142ff), there are three factors in an explanation:

- A *Topic,* T; that is, a fact within an investigatory subject.
- A *set of contrasts.* Lipton (1991) calls these “foils” (F).
- A *relevance relation,* R (to exclude answers that are not part of that topic.

A why-question is thus a three-place relation: $Q = T,\ F,\ R$, and an answer to the Question is of the form:

Fact, T, in contrast to all the **alternative foils** F, because of **Cause**.

Or, to put it in ordinary English, that fact is the fact because it isn’t the case that anything else is the fact because something *makes* it the fact. This is pretty obvious, but it points up what an explanation must do—exclude all other possible alternative facts, in a way that expresses an answer to that question.

This is all abstract (and I have avoided van Fraassen’s notation to try to make it less so), so let’s use Garfinkel’s unfortunately apocryphal example of Willy Sutton[10]. Sutton was a notorious bank robber in the 1920s and 1930s. Once, so the anecdote goes, Sutton was asked why he robbed banks (the version Garfinkel uses has a priest asking; other versions have reporters and gaolers). Sutton replied “That’s where the money is”. The interrogator intended, from context, to ask why Sutton robbed rather than making an honest living; Sutton, however, had a different contrast-class: robbing banks versus robbing corner stores, for example. He explains his choice of target, rather than his choice of activity, by noting the greater return in robbing banks. Much strife in the history of science has been caused by a failure of competing researchers to set up the same contrast-class, and often scientists call their opponents (in the social sense) “unscientific” due a lack of shared contrasts. But that is for another discourse. For now, I want to focus on how contrast classes cause phenomena to be noted.

A contrast-class is related to the physics notion of a phase space (or a Hilbert Space, for mathematicians)[11]. Such a space is defined by a number of axes, each of which represents an independent variable of the issues or ideas in play, and which can be of any number of dimensions. Assuming each axis is a set of real numbers, there are

[9] The N-D Model of explanation doesn’t allow for unattached phenomena; all phenomena must be metrics within a theory.

[10] Although according to the Quote Investigator <`https://quoteinvestigator.com/2013/02/10/where-money-is/`>he may have said it, despite his later denials.

[11] Gärdenfors (2000) calls this the “geometrical approach to the structure of concepts”. See especially his section 4.4.3.

indefinitely many possible coordinates within that space, each one of which represents in our case a potential answer to a topic question. To answer a question is thus to assert a coordinate in that space is correct.

Now any scientific field at a given time and place, has an existing set of alternatives within a phase space. And in that field, there are a subset of viable alternative explanations/foils. The smaller the viable subset, the more consensus there is in that field. In the case of a field that has a *single* coordinate explanation, there is 100% consensus. Generally, though, there is not just one alternative in play. Sometimes the viability set is dependent upon measurement error, sometimes on variant theoretical terms, and so on.

Now, suppose a field which has a high consensus (to pluck a figure out of the air, 97%). This means that the experts in the field have a bounded set of expectations for any new observation. Nobody expects in climate science, for instance, that the next set of measurements will show a massive cooling, or even a stable set of temperatures. The explanation for the facts observed over the past 100 years is that CO2 is causing the retention of heat. Such a phenomenon would be anomalous to the current state of explanation (i.e., *outside* the viability space). In fact, it would be the *making* of the phenomenon. It would call for an explanation not currently in play.

This is not the only way that phenomena are recognized, of course. If a set of prior measurements (say, on animal sizes) fell within a distribution curve, prior to a novel observation of sizes outside that range, this may also trigger recognition of a phenomenon that calls for explanation. Likewise, an entirely novel set of observations (of a new species, for instance) may be a phenomenon that calls for numerous explanations to be tested. This is how the Starting Problem is overcome. Observers *never* start *tabula rasa*, and so even a prescientific culture can recognize phenomena when there are no prior explanations to be had, as prior experience sets the limit of expectations, and thus identifies phenomena. Even a prescientific observer (like a hunter) has prior expectations, based on cultural inheritances and personal experience.

To return to the phase space idea, what makes answers viable in that space? There is no principled answer to that, I think, but one point is that science iteratively refines its viability spaces over time based largely on empirical data. If your expectations are that, for instance, orbits will be the shortest geodesics in the gravity well, an orbit that an explanatory model allows that is not the shortest geodesic is outside the viability space. As van Fraasen noted, a theory must be empirically adequate[12]. It must also force that coordinate, within measurement error, to be that sort of outcome, and so on. But theory is not the only thing that sets empirical expectations.

So phenomena are things that stand out. What does this mean for natural categories?

6. Concluding remarks

To summarize: When scientists investigate phenomena, they are implicitly or explicitly approaching the topic at issue with a set of contrasting categories, which are specified by the research question. And that set of categories determines what the phenomenon is. This determinism closely resembles another claim often made in the philosophy of science and language: that we construct all our categories – that no category is really realistic. This very popular view is often associated with the theory-dependence of

[12] There are numerous “theoretical virtues”. See the forthcoming paper by Michael Keas 2017.

observation thesis[13]. Here, though, I am arguing that observation need not be theory dependent, but it is, and must be, expectation dependent.

Category antirealism is widespread. This often relies on ideas that scientific theories are all-encompassing worldviews (such as "paradigms") that determine what we see, because observations underdetermine explanations and hypotheses. The Starting Problem, though, indicates that we never approach observation naively, but that we always have dispositions, either biological or sociocultural or a mixture of both, to observe some things and not others, out of the infinity of things we might observe. Science is a process of refining observations based on a process of refining our expectations (hence the reference to Bayesian logic before). In short, nobody starts knowing nothing. Science can bootstrap from folk-science because folk-science itself is already bootstrapped from evolved biological and cultural dispositions.

Natural categories are thus not naive. At the same time, though, they are seen by experienced observers. The key term here is "experienced": no observer, whether a scientist or an enthusiast, begins their observations without prior experience being passed on by those who went before and taught that observer. Such teaching may be theoretical, but it may also be ostensive ("like *that*"), or pragmatic ("you can do this with these"), and it will be taught through trial and correction by the prior expert.

To return to "species", experienced observers of species are those who have at their disposal a range of tools:

- Practical training by prior experts
- Knowledge of related species (and thus expectations about what will be seen, so that novel features are highlighted)
- Whatever theoretical prior information there may be
- Cognitive ancillary tools, such as what counts as a good explanation in that field.

All these go to set up what taxonomists call "good species" (Amitani 2015). The prior expectations are based on prototypical models of species (in that group of organisms) so that what fits that model is regarded as a properly describable species; boundary cases are problems to be resolved. This implies, as I have said before, that species are not a theoretical category. That is, there is no unique set of biological groups to which the term "species" applies. It is not a natural category in general. But it *can* be a natural category for a group of organisms; say, a group of birds, cats or plants. If, that is, there are shared ways (modalities, as I called it in 2003) of *being* a species in *that group*. This is, of course, an empirical matter. We have to identify species as phenomena that share certain modalities for that group, and be able to identify the exceptions (as in the whiptail lizard asexual species (Reeder et al. 2002)).

This was a long diversion into a simple topic. I wondered if species could be accounted for as phenomena. From that I asked what a phenomenon is. I rejected the *a priori* claim that natural categories of science are necessarily derived from theories, which has long been the default opinion in philosophy of science. And I argued that phenomena are highlighted as things that stand out from our existing expectations of the domain in question, using the contrastive account of explanations (but I did not give any view about what explanation *is*, because that varies by domain and field). Why don't I offer an account of explanation?

In part, as I noted, explanations satisfy *local* criteria for a field or domain. Consider mountains. How we explain a given mountain depends on the evidence available (evidence of stratigraphy, isotope ratios, etc.), and theoretical postulates like Steno's

[13]Which I have discussed in Wilkins and Ebach (2013, chapter 6).

principles of superposition, original horizontality, continuity, and crosscutting. Moreover, we appeal to causal processes like vulcanism, erosion, uplift, subduction, and so on, some of which are more theoretical than observed. There are, as in most philosophical debates, especially in the philosophy of science, monists and pluralists on explanation. Given that in my view (not argued here) there are no unitary, unique and universal modes of explanation in any field of science, for now, it is enough to say that explanation sets up the things that need to be explained, but so too do observation, experience and economic motivations.

Taxa (that is, categories of the natural world) are not therefore *de jure* objects of theory, and hence, they are not fully vulnerable to the antirealist attack. But neither are they ready-made, either in the mind or the "external" world. They are constructed categories that are held hostage to empirical data, to experience of the world, and to (as Hacking 1983 it) intervention in that world. And that is enough, I think.

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