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Singularist Semirealism

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ABSTRACT

This paper proposes to carve out a new position in the scientific realism/antirealism debate and argue that it captures some of the most important realist and some of the most important antirealist considerations. The view, briefly stated, is that there is always a fact of the matter about whether the singular statements science gives us are literally true, but there is no fact of the matter about whether the non-singular statements science gives us are literally true. I call this view singularist semirealism. Singularist semirealism sides with scientific realism with regards to singular statements but it is an antirealist view with regards to non-singular statements. In this sense, singularist semirealism could be considered to be 'the best of both worlds'.

1 Introduction

The scientific realism/antirealism debate has been, and remains, quite confusing. Different philosophers interpret these labels differently and it is not even clear that there is one single debate between scientific realism and antirealism (see Newton-Smith [1978]; Hacking [1983] for taxonomies). According to a widespread formulation of scientific realism, 'scientific theories are either true or false independent of what we know: science at least aims at the truth and the truth is how the world is' (Hacking [1983], p. 27). In other words, science aims to give us literally true claims about the world and there is a fact of the matter, independent of us, about how the world is.

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Note that this definition has two conjuncts: one about what science aims to do and the other about the relation between scientific theories and the world. Scientific antirealists can deny either of these two conjuncts. They can deny that science aims to give us literally true claims about the world. This would give rise to epistemic antirealism. Or they can deny that there is a fact of the matter about whether these claims are true. This would land us in ontological antirealism. As scientific antirealism is, on the face of it, a negative claim—the denial of scientific realism—there is also a wide variety of antirealist proposals as to what the realist concept of truth should be replaced with.

My main focus in this article is the debate between scientific realism and ontological antirealism. As a result, I put the 'epistemic' or 'aim of science' conjunct in the above formulation aside. And there are independent reasons for doing so. Hacking's definition—like the one offered by Van Fraassen's, who characterizes scientific realism as the view that 'science aims to give us [. . .] a literally true story of what the world is like' (Van Fraassen [1980], p. 8)—relies heavily on the concept of the 'aim of science'. But, there are some familiar worries about what constitutes the aim of such a complex social enterprise (see, for example, Rowbottom [2010], Section 2). As I want to focus on the debate between scientific realists and ontological antirealists, I can ignore the appeal to the 'aim of science'.

Thus, the half of Hacking's statement of scientific realism that I will focus on is that according to which 'scientific theories are either true or false independent of what we know' (Hacking [1983], p. 27). In other words, there is always a fact of the matter about whether statements science gives us are literally true. Ontological antirealists deny this claim.

This article proposes to carve out a new position in the scientific realism/ antirealism debate and argue that it captures the most important realist and the most important antirealist considerations. The view, briefly stated, is that there is always a fact of the matter about whether the *singular statements* science gives us are literally true, but there is no fact of the matter about whether *non-singular statements* are literally true. I call this view singularist semirealism. I call it semirealism because it is a realist view with regards to singular claims but an antirealist view with regards to non-singular claims. And I call it singularist in order to differentiate it from other versions of semirealism (especially Chakravartty [1998], [2007]). Many realists will find my view too antirealist and maybe some antirealists will find it too realist. It is pointless to argue about labels; my aim is to preserve something from the advantages of both realism and antirealism.

There have been a number of attempts at reconciling scientific realism and scientific antirealism. The most famous of these is probably structural realism (see, for example, Worrall [1989]; Ladyman [1998]): we should be realist about structure but not about anything else. Like structural realism, my account also aims to find the 'best of both worlds' in this debate. However, it attempts to do that in a new way: my claim is that we should be realist about singular claims but not about non-singular ones.

2 Truth and Correctness

We can make a distinction between scientific realism/antirealism about theoretical entities and scientific realism/antirealism about theories (see Hacking [1983], pp. 27–8). Realism about entities is the view that theoretical (unobservable) entities that scientific theories postulate really do exist. Realism about scientific theories, as we have seen, is the view that 'scientific theories are either true or false independent of what we know' (Hacking [1983], pp. 27). I am interested in the second debate here and leave those bits of the debate that are about unobservable theoretical entities on the side.

Scientific realism about theories is normally defined in terms of the truth of scientific theories. But it has been pointed out that there may be too much emphasis on linguistic representations in this formulation; scientific theories do not always come in linguistic form (see the rich recent literature on scientific models and Godfrey-Smith [2003], pp. 186–9 for a summary of its relevance to the scientific realism debate). Thus, if we want to formulate scientific realism in such a way that it is general enough to cover all these cases, we need to find a way of including non-linguistic representations too.

In order to avoid these problems, we should replace the concept of truth with the concept of correctness. Scientific realism is the view according to which there is always a fact of the matter about whether the representations science gives us are correct.

Representations attribute properties to entities. Say, a representation attributes property P to an entity e. If e in fact has property P, then this representation is correct. Scientific realists claim that there is always a fact of the matter about whether representations that science gives us are correct. That is, if science gives us a representation that attributes property P to an entity e, there is always a fact of the matter about whether e is in fact P. Scientific antirealists, on the other hand, deny that there is a fact of the matter about whether this representations is correct, about whether e is in fact P.

What is important for the purposes of this paper is that depending on what property P and entity e are, we may get different answers if we ask whether there is a fact of the matter about whether e is P. I will suggest that if P is a property-token, then there is a fact of the matter about whether e is P. But if P is a property-type, then there is no such fact of the matter. But let us proceed more slowly.

3 Property-Tokens and Property-Types

What is the relation between property-tokens and property-types? The color of my laptop is a property-token. Property-tokens are particulars. Gray is a property-type. What is the relation between the two? There seems to be two possible views:

(a*) There is a fact of the matter about whether a property-token belongs to, or is an instantiation of, a property-type

(b*) There is no fact of the matter about whether a property-token belongs to, or is an instantiation of, a property-type.

Three comments: First, when I say that 'there is no fact of the matter about whether a property-token belongs to, or is an instantiation of, a property-type', this means that there no *mind-independent* fact of the matter about this.

Second, the difference between (a*) and (b*) is not to be confused with the classic realism/nominalism distinction about property-types. In fact, both (a*) and (b*) are consistent with both realism and nominalism. More specifically, (a*) does not exclude nominalism. One could think that property-types are resemblance-classes of property-tokens (or of tropes, see Williams [1953], p. 81; Campbell [1981], p. 134; Bacon [1995], p. 18) and, as a result, there is a fact of the matter about whether a property-token belongs to a property-type, but property-types are merely resemblance-classes of property-tokens.

Third, there is a potential ambiguity in this way of drawing the distinction. It may be the case, after all, that there are some property-types that are such that there is a fact of the matter about whether a property-token is an instantiation of it, but there are some other property-types, where there isn't such fact of the matter.

An old and respectable way of characterizing the relation between property-types is the determinable-determinate relation (Johnston [1921]; Funkhouser [2006]). To use a classic example, being red is a determinate of being colored, but a determinable of being scarlet. Without giving a full analysis of the determinable-determinate relation, some of the most important features of this relation need to be pointed out. First, it is an irreflexive, asymmetric, and transitive relation between property-types. There are many ways of being red and being scarlet is one of these: for something to be scarlet is for it to be red, in a specific way. If something is red, it also has to be of a certain specific shade of red; there is no such thing as being red *simpliciter*.

The determinable-determinate relation is a relative one: the same property, for example, of being red, can be the determinate of the determinable being colored, but the determinable of the determinate being scarlet. Thus, the determinable-determinate relation gives us hierarchical ordering of property-types in a given property-space. Property-types with no further determinates, if there are any, are known as super-determinates.

In the light of this, we seem to have three options[1]:

(a) For any property-type (determinable or super-determinate), there is a fact of the matter about whether a property-token belongs to, or is an instantiation of, this property-type.

(b) For super-determinate, but not for determinable, property-types, there is a fact of the matter about whether a property-token belongs to, or is an instantiation of, this property-type.

(c) For no property-type is there a fact of the matter about whether a property-token belongs to, or is an instantiation of, this property-type.

The question is, then, whether we should endorse (a), (b), or (c). If we accept (a), then there is a fact of the matter about whether a property-token belongs to, or is an instantiation of, a determinable property-type. In other words, (a) commits us to the existence of mind-independent determinable property-types.

There are two versions of this claim, depending on whether one has nominalist or realist leanings. If one is nominalist about property-types, then (a) amounts to endorsing the existence of loose resemblance classes of property-tokens, that is, resemblance classes defined in terms of non-exact resemblance (Williams [1953], p. 81; Campbell [1981], p. 134). If one is realist, then (a) amounts to positing the existence of determinable universals (Armstrong [1978]). There are problems with the former way of thinking about determinables as loose resemblance classes. David Manley argues that any such attempt would face familiar worries that allegedly made *object* resemblance nominalism lose its appeal: the companionship problem and the imperfect community problem (Manley [2002], especially pp. 86–7; these problems are first raised in Goodman [1951]). Further, as Bacon points out, this way of defining resemblance classes would require a primitive similarity measure for each property-type, something not exactly in line with the general nominalist sentiment towards parsimony (Bacon [1995], p. 18).

But there is a more general worry about assuming the mind-independent existence of determinable property-types (see Gillett-Rives [2005] for a detailed overview). David Lewis made a famous distinction between sparse and abundant (or, natural and unnatural) properties, that is, property-types (Lewis [1983], [1986]). Abundant properties, as Lewis puts it, 'carve reality at the joints – and everywhere else as well. If it's distinctions we want, too much

[1] The options (a), (b), and (c) do not exhaust the logical space for possible views about the relation between property-types and property-tokens. The forth possible view is that for determinable, but not for super-determinate, property-types, there is a fact of the matter about whether a property-token belongs to, or is an instantiation of, this property-type. I ignore this possibility because it seems to combine the weaknesses of (b) and (c) without inheriting their strengths.

structure is no better than none' (Lewis [1983], p. 346). He concludes that we need some other, more restricted, concept of properties, which he calls sparse properties. Sparse properties (or perfectly natural properties) are 'an élite minority of special properties' (Lewis [1983], p. 346); they are the properties with 'relevant causal powers' (Lewis [1983], p. 347). But then the question is whether determinable property-types have any causal powers. If they don't, then they cannot be sparse properties.

To use Shoemaker's rhetoric, if properties are individuated by their causal powers (Shoemaker [1979]) and determinables do not have causal powers, then determinables are not properties. But do determinable properties have causal powers? Although some have argued that they do (see especially Yablo [1992]), the (weak) consensus is that they do not; only super-determinates have causal powers (see Armstrong [1961], [1978]; Lewis [1983] and Crane [2009] for a very thorough overview). Causal relevance is only one of seven desiderata Lewis sets for natural properties and, taking all of them into consideration, it seems that the least problematic account of natural properties is one that takes these to be super-determinate (see Denby [2001]; Weatherson [2005]; Hawthorne [2006] for discussion). But if only super-determinate property-types have causal powers (and satisfy the other six desiderata for sparse properties), then we have no reason to postulate the mind-independent existence of determinable property-types (in any non-trivial, that is, non-abundant sense). And this leads to (b).

There is a nominalist and a realist version of (b) as well. The nominalist would say that super-determinate property-types are exact resemblance classes of property-tokens (Williams [1953], p. 117; Bacon [1995], p. 18). And the realist would say that all universals are super-determinate (Lewis [1983] is flirting with this option, see especially p. 357 and see also his posthumous Lewis [2009], where he says that perfectly natural properties are 'not at all [...] determinable' (p. 204). This way of thinking about property-types would leave open the question about how we can then talk about determinable property-types, such as being red. I will talk about this in the discussion of (c), but first I need to raise a problem that is specific to (b).

If we accept (b), we need to hold that there are such things as super-determinate property-types (whether they are exact resemblance classes or universals): property-types with no further determinates. This is an assumption that many would be happy to make. W. E. Johnston, for example, who introduced the determinable-determinate relation, takes the existence of super-determinates as a 'universally adopted postulate' (Johnson [1921], p. 185), and many agreed with him (see, for example, Armstrong [1961]). But should we postulate the existence of such property-types? Why should we assume that there are property-types that have no further determinates? Wouldn't it be possible that every determinate has further determinates? It has been argued that what Johnson called a 'universally adopted postulate' is in fact false (see, for example, Sanford [2006]; Sorensen [2011]).

In short, if we deny that there are super-determinate property-types and also deny (a), then (c) seems to be the only viable option remaining. But even if we hold that there are super-determinate property-types and that there is a fact of the matter about whether a property-token belongs to, or is an instantiation of, a super-determinate property-type, this may not be a particularly interesting fact of the matter from the point of view of the scientific realism/antirealism debate, as the super-determinate property-types, if they really are super-determinate, are unlikely to appear in the claims scientists make: no two rabbits have exactly the same super-determinate properties (see Mayr [1959] and Nanay [2010a]), and maybe not even two atoms do. Scientists often (arguably, always) consider two property-tokens to be 'the same' for their purposes even if they do not belong to, or are not instantiations of, the same super-determinate property-type. So if we want to give an account of the relation between property-tokens and property-types that is of any use to scientists, we need to turn to (c).

Is (c) a crazy claim? I don't think so. It is the simple view that there is no fact of the matter about whether a property-token belongs to, or is an instantiation of, a property-type, as property-types are our own groupings of property-tokens. Property-tokens are out there, independent of us. Property-types aren't.

Think of property-tokens as points in a property-space (see Quine [1969]; Clark [2000] on the concept of property-space—Lewis also uses this metaphor for highlighting the difference between natural and unnatural properties in Lewis [2001], pp. 385–286). Some pairs of property-tokens resemble each other more than others; they are closer together in this property-space. Property-types are regions in the property-space.

One picture about this property-space is that there is a fact of the matter about where the 'true' boundaries of the regions of this property-space lie: there is a fact of the matter about where one natural property-type (one universal or one resemblance class of property-tokens) ends and where the other begins. And there is one way of partitioning the property-space that cuts it at its joints, that would divide it along the lines of natural properties. This is the picture (a) and (b) take for granted.

Another, alternative picture is the following: Some pairs of property-tokens are closer together in the property-space; they resemble each other more than others. But property-types are our arbitrary ways of delineating regions of this property-space. The property-space does not have joints: it consists of lots of property-tokens, some close together, some further away from each other. Property-types are our ways of grouping these points in the property-space and the way we group property-tokens together depends on our interests. In other words, there is a fact of the matter about how similar any two property-tokens are. But there is no fact of the matter about whether a property-token belongs to a property-type (see also Nanay [2009a], [2010a], [2011a], [2011b]).

To use an old analogy, property-tokens could be thought of as stars and property-types as constellations (Cournot [1851]; Darwin [1859], p. 397; Goodman [1978]; see Hacking [2007] for a good summary). There is a fact of the matter about the distance between any two stars, but the constellations of stars, like that Cassiopeia, are our own arbitrary groupings of some stars. As Ian Hacking says, 'they are convenient for navigators but the stars are grouped together by people, not nature' (Hacking [2007], p. 225). Similarly, there is a fact of the matter about the degree of resemblance between any two property-tokens, but, to paraphrase Hacking, property-types are property-tokens 'grouped together by people, not nature'. Does this mean that there is no fact of the matter about which star belongs to which constellation? It means that there is no *mind-independent* fact of the matter about which star belongs to which constellation because constellations are our own groupings.

To use a different example, take the color-spectrum. It can be partitioned into colors in a number of ways. Speakers of different languages and even different individuals of the same linguistic community will do so differently (Hardin [1988]; Block [1999]; Tye [2006a], [2006b], [2007]; Byrne and Hilbert [2007]; Cohen *et al*. [2006]). Again, the natural way of describing this is to say that color property-tokens exist independently of us (for simplicity, I leave aside the issues about relationalist theories of color here), but the property-type 'blue' or 'green' does not exist independently of us; they come about if we group some property-tokens together under the label of 'green' or 'blue'.

Note that if someone thinks that there is a fact of the matter about where the 'true' boundaries of the regions of this property-space lie, like the proponents of (a) and (b) do, then these true boundaries are supposed to justify the pragmatic relevance of carving up the property-space this way. If there is an objective, mind-independent common denominator among some property-tokens, then treating them as instantiations of one and the same property-type will clearly be pragmatically useful. But the picture I am presenting here proceeds the other way round. Some property-tokens are pragmatically useful to group together. And this explains why we consider these property-tokens to be instantiations of the same property-type. In my picture, it is not the objective, mind-independent existence of property-types that explains why these property-types are pragmatically useful, but our pragmatically useful way of grouping property-tokens constitutes what we consider property-types.

Two aspects of this metaphysical picture need to be highlighted. First, I said that property-types are our grouping of property-tokens. But couldn't we group together really distant property-tokens, if we wanted to? We could.

But remember that these groupings are supposed to serve some kind of practical purpose. We could group together stars from various distant parts of the firmament, but that would hardly be called a constellation, let alone a constellation that could serve any practical purpose. To go back to David Lewis's distinction between sparse and abundant properties (that is, property-types), my claim amounts to denying that there is any ontological difference between sparse and non-sparse property-types; there is no fact of the matter that would tell us which property-types are sparse and which ones are merely abundant. But there is a huge *practical* difference between property-types (see Taylor [1993], [2004] and Goodman [1955] for similar distinctions).

Second, shouldn't we group together exactly resembling property-tokens? The answer is that we should. But then wouldn't these exactly resembling property-tokens constitute a property-type that exists independently of us and where there is a fact of the matter about whether a property-token belongs to this property-type? If so, wouldn't this bring us back to the less radical view of (b)?

The answer is this: If super-determinates do not exist, then there is no such thing as exactly resembling property-tokens, so we should not try to group them together. But even if they exist and there are exactly resembling property-tokens, we are in a very bad position to even try to group them together as their exact resemblance is extremely unlikely to be accessible to epistemic agents like us. Suppose that the property-tokens we are interested in are the weight of some entities. In order for two of these property-tokens to be the same, two entities would have to be of exactly the same weight—not just in grams, but in milligrams, micrograms, nanograms, picograms, etc. But then we are not in the position to tell whether two of these property-tokens are exactly resembling—we just do not have the means to measure them 'exactly'. In short, property-types that group only perfectly resembling property-tokens together have very limited uses in science.

Thus, we have a metaphysical picture where property-tokens exist independently of us, but property-types do not. I did not intend to give any argument in favour of this metaphysical picture here. I aimed to show that it is not to be dismissed in favour of the other two views, (a) and (b), about the relation between property-tokens and property-types. And, depending on how we think about the causal relevance of (determinable) properties or the existence (and epistemic accessibility) of super-determinates, it may be the most attractive such view. In any case, my aim here is to carve out a new position in the scientific realism/antirealism debate, so I will now set metaphysics aside and move on to a detailed articulation of such a position.

4 Singularist Semirealism

We have seen that scientific realism is the view according to which there is always a fact of the matter about whether the representations that science gives us are correct. How should we interpret this view if we accept the metaphysical picture I outlined in the previous section?

First of all, we need to make a distinction between singular and non-singular representations. Singular representations attribute property-tokens (to particulars). Non-singular representations attribute property-types (to particulars or sometimes to other property-types). Here is a non-singular representation: 'Japanese cars are reliable'. Here is a singular representation: 'my Honda in front of my house has this shade of grey'. Only particulars show up in a singular representation, whereas non-singular representations are not exclusively about particulars.

Now remember that according to the metaphysical picture I outlined in the previous section, property-tokens exist independently of us, but property-types do not. In other words, whether we should accept scientific realism depends on whether the concept of representation in the formulation of scientific realism is to be interpreted as singular or non-singular representation.

If it is to be interpreted as singular representation, then scientific realism is true: there is always a fact of the matter about whether these singular representations are correct. Singular representations attribute property instances to entities and there is always a fact of the matter about whether these entities in fact have these property-tokens.

But if the concept of representation in the formulation of scientific realism is to be interpreted as non-singular representation, then scientific realism is false: there is no fact of the matter about whether these representations are correct.

Should we then give up scientific realism? My proposal is that we should give up half of it, but keep the other half: we should give up scientific realism about property-types and non-singular representations but keep scientific realism about property-tokens and singular representations. That is why I call this view singularist semirealism.

Consider Carl Hempel's famous characterization of the vocabulary of science:

> The vocabulary of science has two basic functions: first, to permit an adequate description of the things and events that are the objects of scientific investigation; second, to permit the establishment of general laws or theories by means of which particular events may be explained and predicted and thus scientifically understood. (Hempel [1965], p. 139)

If we accept singularist semirealism, then these two 'functions' of the vocabulary of science need to be kept separate. The first function is a descriptive one; we can describe 'the things and events that are the objects of scientific investigation' correctly or incorrectly. But the second function is not descriptive at all; it consists of the *postulation* of 'general laws or theories'.

Singularist semirealism then makes a positive and a negative claim. The positive claim is that there is always a fact of the matter about whether singular representations are correct. And the negative claim is that there is no fact of the matter about whether non-singular representations are correct.[2]

Singularist semirealism sides with scientific realism in endorsing the positive claim; no scientific realists should have any problems with the positive claim. But singularist semirealism sides with scientific antirealism when it comes to the negative claim. In this sense, it aims to provide the 'best of both worlds'.

Of the two claims, the negative one is the harder to swallow. Scientific laws are about property-types. So are general causal claims and other generalizations. Is singularist semirealism then committed to saying that there is no fact of the matter about whether scientific laws (and general causal claims and other generalizations) are correct? The short answer is: yes. The long answer will have to wait until Section 6.

More on the negative claim. I said that there is no fact of the matter about whether non-singular representations are correct. This will do as a slogan, but there are three ways in which this claim needs to be clarified and qualified.

First, we can and do make true and false claims about non-singular representations. I have just made (a hopefully true) one myself when I claimed that there is no fact of the matter about whether non-singular representations are correct. But note that the claim singularist semirealism makes is not that there is no fact of the matter about non-singular representations at all, but a more limited one: there is no fact of the matter about whether non-singular representations are correct.

Second, some non-singular representations are correct by definition. Here is an example: particulars that instantiate the property-type red instantiate the property-type red. Here is another one: particulars that instantiate the property-type scarlet instantiate the property-type red. So a more precise formulation of my claim would be to say that there is no fact of the matter about whether not analytically correct non-singular representations are correct. For simplicity, I will go on using the shorter formulation in what follows.

Third, is there a fact of the matter about super-determinable property-types? Is there a fact of the matter about whether a property-token belongs to, or is an instantiation of, a super-determinable property-type?

[2] Anjan Chakravartty sometimes expresses very similar views. For example, he writes that 'nature is composed of distributions of property instances, only some of whose patterns of sociability we consider and investigate' (Chakravartty [2007], p. 178). But his general stance is far more realist than the singularist semirealist view I am defending. Immediately before the quoted sentence, for example, he writes that 'what is natural goes well beyond what is useful, convenient or interesting in everyday and scientific contexts' (ibid.).

Although, as we have seen, it is not at all clear whether there are super-determinates, it seems less controversial that there are super-determinables-property-types with no further determinable. It is not clear what they are; maybe being colored or having an extension are super-determinables but it is also possible that there is only one super-determinable, namely, being—and being colored, and so on are its determinates (McDaniel [2009]). Without getting entangled in these metametaphysical questions, I need to qualify my slogan: regardless of what we take to be super-determinable property-types, there is indeed a fact of the matter about whether a property-token belongs to (is an instantiation of) super-determinable property-types.

There is indeed a fact of the matter about whether the blue property-token of my left eye is a color-property (if we take being colored to be a super-determinable). Or, if we endorse the view that there is only one super-determinable, there is indeed a fact of the matter about whether the blue property-token of my left eye is a property. Is this a significant concession? I don't think so—at least not when it comes to singularist semirealism and the claim that there is no fact of the matter about non-singular representation. What representation would attribute a super-determinable property-type? The representation of an entity as colored? Or the representation of an entity as existent? Representations of this kind are unlikely to play any role in any conceivable scientific explanations. Even though there is a fact of the matter about super-determinable property-types, representations involving these property-types are unlikely to show up in actual scientific theories.

In the light of these clarifications and qualifications, a more precise formulation of the negative claim of singularist semirealism would be to say that there is no fact of the matter about whether not analytically correct and not super-determinable non-singular representations are correct.

Finally, I need to say something about the logical relation between singular and non-singular representations. One view about the relation between these two kinds of representations is that non-singular representations are logically prior: singular representations are to be derived from non-singular ones. Singularist semirealism questions the validity of this picture. According to the singularist semirealist, it is singular representations that are logically prior: non-singular representations are to be derived from singular ones—and the only way to derive them is by means of the pragmatic usefulness of the non-singular representations that are based on singular representations.

I want to now turn to some encouragement from actual scientific practice, before highlighting some potentially worrying and the potentially promising consequences of singularist semirealism.

5 Encouragement from Actual Scientific Practice

Yet another ambiguity in formulations of scientific realism that I have not highlighted so far concerns a normative/descriptive distinction. Does scientific realism make a normative or a descriptive claim? Is it a view about actual scientific practice or about what science should be doing?

I do not intend to resolve this ambiguity here. I want to merely point out that actual scientific practice is not irrelevant when trying to resolve the scientific realism/antirealism debate. It is not irrelevant, but it is not a straightforward tie-breaker either. After all these caveats, let us see what actual scientific practice tells us about the feasibility of singularist semirealism.

One important consideration comes from the experimental nature of much of what we know as the scientific enterprise. Experiments are done on particulars with property-tokens. No experiment is done on property-types. They manipulate property-tokens, not property-types (see Hacking [1983] for a thorough examination of the relevance of experiments to the scientific realism/antirealism debate). Measurement is also always the measurement of a property-token and not of a property-type. So it seems that the two main tools of actual scientific practice, experimentation and measurement, are practices involving property-tokens and not property-types. Of course, on the basis of the measurement of, or experimentation with, property-tokens, science postulates models, laws or general causal claims that are about property-types, but this process needs to start with the attribution of property-tokens.

Ian Hacking famously wrote that 'the final arbitrator in philosophy is not how we think but what we do' (Hacking [1983], p. 31). This may or may not be true of philosophy in general, but it seems to be uncontroversial as a motto about science. When we are trying to think about actual scientific practice, we should not ask what scientists think or what they should think. We should enquire about what they do (see also the famous opening sentence of Einstein [1934]). And whatever they do is done on property-tokens.

The *sine qua non* of science is the attribution of property-tokens. This is what makes the attribution of property-types possible to begin with. If science is to give us a reliable, or even pragmatically useful, picture of the world, it needs to start with correct representations of property-tokens. Scientists in general are mainly interested in property-types and in generalizations. But the only way to arrive at property-types and generalizations is by means of attributing property-tokens—by means of experiments and measurements. True, these property-tokens are attributed in order to formulate a more general scientific picture. But any such enterprise must start with, and be based on, the attribution of property-tokens.

But here is a more specific example, from a branch of science that tends to be underrepresented in the scientific realism/antirealism debate: biology.

Ernst Mayr's concept of population thinking (Mayr [1959/1994]) is widely accepted among biologists and even among philosophers of biology and it is supposed to capture the *right* way of thinking about the biological domain. But what does he mean by population thinking? Here is Mayr's characterization of population thinking from 1959:

> Individuals, or any kind of organic entities, form populations of which we can determine only the arithmetic mean and the statistics of variation. Averages are merely statistical abstractions; only the individuals of which the populations are composed have reality. (Mayr [1959/1994], p. 326.)

Mayr contrasts population thinking with typological thinking, according to which 'there are a limited number of fixed, unchangeable "ideas" underlying the observed variability, with the *eidos* (idea) being the only thing that is fixed and real, while the observed variability has no [. . .] reality' (Mayr [1959/1994], p. 326). The contrast Mayr makes is a very sharp one: population thinking and typological thinking are exclusive of each other (Mayr [1959/1994], pp. 326–7).

Mayr's distinction between typological and population thinking may appear straightforward, but in fact it has been, and could be, interpreted in at least two ways.

Population thinking could be interpreted as an ontological claim about *entities*: only the individual is real, everything else is abstraction. There are various problems with this reading. If only the individual is real, then populations and species should be thought of as groups of individuals that lack reality themselves. This would make much of post-Darwinian biology nonsensical from the population thinker's point of view. As Elliott Sober says:

> If [as Mayr claims] 'only the individuals of which the populations are composed have reality,' it would appear that much of population biology has its head in the clouds. The Lotka-Volterra equations, for example, describe the interactions of predator and prey *populations*. Presumably, population thinking, properly so called, must allow that there is something real over and above individual organisms. [It does not] embody a resolute and ontologically austere focus on individual organisms alone. (Sober [1980], p. 352).

Even worse, Mayr himself is certainly not nominalist about populations and species (Mayr [1942], p. 120, Mayr [1963], p. 19). His dictum that 'only the individuals [. . .] have reality' seems to flatly contradict his famous 'biological species concept', which does indeed attribute reality to populations and species. It is tempting to resolve this seeming contradiction by dismissing Mayr's claim about the importance of the individual in evolution as an exaggeration or even as 'rather silly metaphysics' (Ariew [2008], p. 2).

Elliott Sober chooses this route when he says that 'describing a single individual is as theoretically peripheral to a populationist as describing the motion of a single molecule is to the kinetic theory of gases. In this important sense, population thinking involves *ignoring individuals*...' (Sober [1980], p. 370). The conclusion he draws is that 'population thinking endows individual organisms with more reality *and* with less reality than typological thinking attributes to them' (Sober [1980], p. 371).

This conclusion prompted some to be 'a little confused about which one, individuals or populations, are real' (Ariew [2008], p. 8). It also opened up the concept of population thinking to many diverging interpretations, some of which seems to contradict Mayr's original claims (Griffiths [1999], pp. 209–10; Walsh [2006], pp. 432–3).

I argued recently that population thinking is an ontological claim about *properties* and not about entities (Nanay [2010a]). For the population thinker, only the property-tokens of individual organisms are real. Property-types are not real.

We have to be careful when formulating this claim. The population thinker presumably would not deny that groups of individual organisms do have properties and these properties are real. A population of 431 geese has the property of having the population size of 431, for example, and this property seems very real indeed. The distinction Mayr was making is not one between the properties of individuals and the properties of populations. Rather, it is between individual property-tokens and property-types that can be instantiated in many different entities. In short, the population thinker can acknowledge the existence of populations and species. These entities are real in the same way as individuals are real. And all of these entities have very real property-tokens. What the population thinker denies is that there are property-types.

In other words, Mayr's provocative statement, according to which 'averages are merely statistical abstractions; only the individuals of which the populations are composed have reality' should be read as '*property-types* are merely statistical abstractions; only the *property-tokens* of individuals (or of populations) have reality'. Mayr's population thinking is a version of singularist semirealism.

Note that this reading makes the apparent contradiction between population thinking and the 'biological species concept' disappear. Mayr is indeed not nominalist about species and populations; these are real entities (that have real property-tokens). But this claim is consistent with the general framework of singularist semirealism. We can accept singularist semirealism—thus avoiding the conflict with the 'biological species concept'.

Although Mayr does not talk about property-tokens too much and he is not particularly clear about the metaphysical framework he presupposes, he does write that '[a]ll organisms and organic phenomena are composed of unique features and can be described collectively only in statistical terms'

(Mayr [1959/1994], p. 326). Here Mayr talks about the uniqueness of *features*—that is, properties—and not the uniqueness of individual entities. The upshot is that individuals have 'unique features' (property-tokens): individual i_1 has property-token p_1, i_2 has p_2, and so on. Suppose these individuals form a population. The question is how to talk about the properties of these individuals 'collectively'. Mayr's point is that we can only describe them 'in statistical terms'. That is, the property-type that $p_1, p_2, \ldots p_n$ belongs to is a statistical abstraction; it is not a property-type that exists independently of the specific individuals and their specific property-tokens.[3]

To sum up, it seems that Mayr's population thinking is a version of singularist semirealism. Those biologists who accept it as one of the cornerstones of evolutionary biology implicitly use the singularist semirealist theoretical framework.

Note that I gave two arguments in this section: one about experiments and measurement being about property-tokens not property-types, and the other about population thinking. There is an important branch of science that these arguments leave untouched: theoretical physics. Theoretical physics (or, at least some branches thereof) does not postulate its theories on the basis of experiments or measurements. In other words, they do not postulate their non-singular representations on the basis of singular representations. Theoretical physics (or, at least some branches thereof) is not an empirical science. The considerations from actual scientific practice then do not support singularist semirealism in this branch of science. Further, one may wonder about whether the considerations in Section 3 about the irrelevance of super-determinates for actual science apply in the case of theoretical physics (especially if, as we have just seen, theoretical physics is not constrained by considerations concerning measurement). The reason why super-determinates, even if they exist, may not be particularly interesting from the point of view of science was that exact resemblance is unlikely to be accessible to epistemic agents like us—it would, for example, involve comparing the weight of two entities not just in grams, but in milligrams, micrograms, nanograms, picograms, and so on. But given that (certain branches of) theoretical physics can talk about exactly resembling particulars without measuring their properties, these considerations fail to apply in the case of this discipline.

Is this a huge problem for my account? I don't think so. As McMullin ([1984]) pointed out, theoretical physics is not a very plausible model for the rest of science (it needs to be noted that he exploited this fact in favour of his version of scientific realism). Further, allowing theoretical physics to be an exception may make more palatable an important consequence of singularist

3 See also (Nanay [2010b]) on why talking about property-types (or trait-types) in some domains of biology could be thought to be problematic.

semirealism, namely, that there is no fact of the matter about scientific laws: If we accept that singularist semirealism may not apply to theoretical physics, then there would be no such implications for the laws of theoretical physics—arguably the most plausible candidates for scientific laws. Those who are moved by these considerations can read all the claims about science in this article to be about science minus theoretical physics.

6 Some Potentially Worrying Consequences of Singularist Semirealism

After this encouragement, we now face a serious challenge: a consequence of singularist semirealism is that there is no fact of the matter about whether scientific laws are correct. But doesn't this just show that singularist semirealism is an obviously crazy view? I don't think so.

Scientific laws are about property-types. According to singularist semirealism, there is no fact of the matter about whether representations of property-types are correct. Property-types are our pragmatically useful ways of grouping property-tokens. Hence, it follows from singularist semirealism that there is no fact of the matter about whether scientific laws are correct. I propose that this is the right way of thinking about both property-types and scientific laws: it is true of both of them that they are not discovered but posited by us.

David Lewis famously said that 'laws and natural properties [that is, natural property-types] get discovered together' (Lewis [1983], p. 368). It follows from singularist semirealism that this is wrong. Rather, 'laws and natural property-types get posited by us together'. Scientific laws are not out there waiting for us to discover them. They are constructed by science. Does this make them any less respectable? I don't think so. After all, and here is where the positive claim of singularist semirealism saves the day, scientific laws are postulated on the basis of (presumably correct) singular representations of the world. Science postulates scientific laws that can cover *correct* singular representations and, remember, there is always a fact of the matter about whether singular representations are correct. Scientific laws have no more objective existence than the property-types they are supposed to be about.

But does not the rejection of the objectivity of scientific laws bring with it all kinds of uncanny consequences? One important example: isn't causation supposed to be closely linked to the laws of nature? Again, David Lewis seems to think so: 'it is fairly uncontroversial that causation involves laws' (Lewis [1983], p. 368). Does it? Here, we need to distinguish singular and general causal claims. Singular causal claims connect property-tokens; general causal claims connect property-types. It is indeed uncontroversial that general causal claims 'involve laws'. But why would singular causal claims involve laws?

In fact, according to singularist semirealism, there is a fact of the matter about whether singular causal claims are true (as there is always a fact of the matter about property-tokens). But there is no fact of the matter about whether general causal claims are true. The relation between singular and general causal claims is of course very complex (see Good [1961], [1962]; Sober [1985]; Carroll [1991]; Eells [1991]; Hitchcock [1995], [2001]). But if one holds that the real causal structure of the world is captured by singular and not by general causal claims, then singularist semirealism is not making a crazy claim at all.

However, a disturbing fact remains: few titles of articles in *Nature* or *Science* are singular causal claims, but many of them are general causal claims. What should singularist semirealism say about that? It is important to remember that singularist semirealism does not want to purge science from general causal claims or from property-types. Property-types, scientific laws, and general causal claims are part of science and it is difficult to imagine a scientific theory without them. What singularist semirealism says is that these property-types, laws and general causal claims are postulated and not discovered. But this does not make these property-types, laws, general causal claims and other generalizations any less respectable and any less capable of allowing us to build bridges or cure cancer. According to singularist semirealism, it is one of the most important endeavours of science to postulate property-types that are pragmatically useful for regimenting singular facts and to postulate laws that are pragmatically useful for covering singular facts. But these scientific laws and property-types are postulated on the basis of (presumably correct) singular representations of the world. Science postulates scientific laws that can cover *correct* singular representations and, remember, there is always a fact of the matter about whether singular representations are correct. We have no reason to mistrust the scientific laws just because they are postulated and not 'discovered'.

In short, attributions of property-types, most notably scientific laws and general causal, claims are part of general scientific practice. But there is no fact of the matter about whether they are true. They are pragmatically useful tools at the disposal of science to group singular facts.

7 Some Promising Consequences of Singularist Semirealism

It seems that the worrying consequences of singularist semirealism were not so worrying after all. We have to give up the claim that there is a fact of the matter about scientific laws, but that may not be such a bad thing as long as there is a fact of the matter about whether singular causal claims are true—as singularist semirealism asserts.

And there are some promising consequences of singularist semirealism. More specifically, although it is a version of scientific antirealism, it can give a convincing answer to the main ('ultimate') argument against scientific antirealism.

One important challenge for any scientific antirealist is to explain the success of science. This challenge is so significant that it has been referred to, somewhat dramatically, as the 'ultimate argument' against antirealism (Van Fraassen [1980], p. 37; Musgrave [1988]). The 'ultimate argument' is very simple and its original formulation comes from Hilary Putnam (although it could be traced back to J. J. C. Smart's 'cosmic coincidence' argument, see Smart [1968], p. 39, see also Stanford [2000], pp. 269–70; Lyons [2003]; Devitt [2008]):

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. (Putnam [1975], p. 73)

Putnam says that scientific realism is the *only* way to explain the success of science. Is it? Would singularist semirealism make the success of science a miracle? It certainly would not. Although there is no fact of the matter about whether the laws a scientific theory postulates are true, there is a fact of the matter about whether the singular causal claims a scientific theory makes are true. And as the laws that scientific theories postulate are supposed to be based on these (supposedly true) singular causal claims, it is not a miracle that scientific theories can make successful predictions and explanations.

Remember that for singularist semirealism, property-types are our ways of grouping property-tokens. Some groupings are more useful from a pragmatic point of view than others; they allow us to make better predictions and explanations. And, importantly, what is supposed to be grouped by these property-types are property-tokens and there is, again, always a fact of the matter about whether a particular has a property-token. But if this is true, then the ultimate argument against scientific antirealism does not work in the case of singularist semirealism.

It is important to clarify how the realist half of singularist semirealism can save this view from the no miracles argument. According to singularist semirealism, non-singular representations—laws, generalizations—are postulated on the basis of correct singular representations. And then we make predictions on the basis of these non-singular representations that are, in turn, based on correct singular representations. Thus, these predictions are—indirectly—based on correct representations, namely, correct singular representations. Hence, it is by no means a miracle that these predictions have a higher probability of succeeding than chance. They succeed because they are (with the mediation of non-singular representations) based on correct (singular) representations.

The realist can push the no-miracles argument even further: wouldn't these 'mediating' non-singular representations need to be correct in order to ground our better-than-chance predictions? The answer is that they wouldn't.

Remember: these non-singular representations are posited on the basis of our correct singular representations precisely in order to give better than chance predictions. They are posited for their pragmatic usefulness (for the purpose of predictions and explanations), not for their correctness.

8 Conclusion: Theory Change

My aim was to argue for a compromise between scientific realism and scientific antirealism. I argued that we should be realist about singular representations and antirealist about non-singular ones. In this sense, it could be considered to be 'the best of both worlds'.

In conclusion, I want to compare my 'best of both worlds' compromise in the scientific realism/antirealism debate with the original 'best of both worlds' compromise: structural realism. According to structural realism, we should be realist about structure but not about anything else (I leave aside the differences between different versions of structural realism and the difficulties with cashing out what is meant by 'structure'). According to structural realism, what is preserved through theory change is the structure. And this is what blocks one of the most influential arguments against scientific realism, the 'pessimistic meta-induction', according to which as our past scientific theories have turned out to be false, we have no reason to believe that our present scientific theories are true (Laudan [1981]; Psillos [1999]). As structural realists claim that the structure of scientific theories (at least sometimes) survive theory change, they can block this argument easily: at least some of our past theories were not *entirely* wrong—they did get the structure right.

According to singularist semirealism, what is (typically) preserved through theory change are singular representations. But non-singular representations are (typically) not preserved. Note that this account can also block the pessimistic meta-induction argument. As singular representations do survive theory change, our past theories were not *entirely* wrong; they got the singular representations right. I am not sure that the 'pessimistic meta-induction' is as strong an argument as it is sometimes supposed but it is worth emphasizing that my account can handle it as well as the structural realist can. The compromise singularist semirealism offers between scientific realism and anti-realism is very different from the one structural realism does.

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