Adaptive Empiricism[1]

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Abstract:
This paper presents a sketch of a moderately anti-realist position in philosophy of science that is a modification of Van Fraassen's constructive empiricism and that I call 'adaptive empiricism'. This modification is motivated by the intuition that assessing what is or is not observable should be an important element of theory choice for an empiricist. (I use cases of underdetermination as examples.) Thus I argue that Van Fraassen's distinction between what is observable and what is unobservable should be adapted to changing theoretical and experimental contexts. I close with some ideas as to how to develop this position more fully.

1. Introduction

If one takes the dividing line between philosophers leaning towards realism and philosophers leaning towards anti-realism to be whether or not they believe that explanatory power is an epistemic virtue or a pragmatic virtue of theories, then I am an anti-realist. In this short paper, I shall in fact present a sketch of an anti-realist position close to Van Fraassen's celebrated constructive empiricism. But while I shall not take a realist view of what does the explaining in scientific theorising, I shall adopt an adaptable view of what should count as the observable phenomena that are to be described by the theory. In this sense (and for want of a better name), the position to be sketched will be called 'adaptive' empiricism.

In Section 2, I briefly summarise some well-known background on scientific realism and constructive empiricism, highlighting what issues will be at stake. In Section 3, I shall then take a brief detour through underdetermination (a standard problem for realism), suggesting that the truly interesting cases of underdetermination are the ones that can potentially be resolved by appeal to the promise of empirical fruitfulness. In Section 4, I shall suggest, however, that this quasi-empirical solution to the problem

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of underdetermination puts pressure also on a constructive empiricist. This will finally lead to the sketch of the new ‘adaptive’ empiricism in Section 5.

2. Some well-known background

Under the traditional reading of logical empiricism, a debate between realism and anti-realism does not strictly make sense: given a fundamental distinction between theoretical and observational language, it is only observational language that is interpreted literally. Theoretical statements are logically constructed out of observational ones and do not correspond directly to an exterior reality. Since the mid-1930s, however (with Popper [1], cf. pp. 94–95 of the English edition), and decidedly from the 1950s (with Quine [2], Hanson [3], and Kuhn [4]) we have come to recognise that observation is theory-laden, and thus a strict theoretical-observational distinction is untenable.

The breakdown of this distinction, as is well known, has been read in two opposite ways, leading to the development of two very different directions in philosophy of science. One extreme has taken observation to be so infected by theory that realism, objectivity and rationality in science are variously but radically undermined. The other has taken theoretical and observational entities to be on a par, thus realism to be vindicated, and metaphysics as a whole to be rehabilitated. But the most interesting positions (perhaps unsurprisingly) lie somewhere in the middle.

Kuhn himself is a case in point: even though for him there is no cumulative growth of theoretical knowledge, Kuhn thinks of science as both progressive and rational (progressive, because, despite ‘Kuhn losses’ along the way, science achieves better and better overall fit to the selective pressure of the external world, which is an empirical notion of progress; and rational, because the criteria of theory choice used during scientific revolutions, first and foremost the promise of fruitfulness. are rationally justifiable given the kind of progress sought, cf. his well-known discussion in [5]).

Many other such intermediate positions have been proposed, including various realist ones – from Worrall’s structural realism (in which we can be agnostic or even antirealist about entities, but gain cumulative knowledge of the structures in the world, which form the basis for the explanatory success of science [6]), to Hacking’s experimental realism (which is a realism about entities that can be used experimentally to manipulate other known entities [7]), or ones that aim to be equidistant between realism and anti-realism (in particular Fine’s Natural Ontological Attitude [8]). My starting point, however, will be Van Fraassen’s constructive empiricism, the view that arguably commands the most interest among anti-realist ones [9].

The key insight into constructive empiricism is that while there is no reason to dispute that theoretical terms should be construed literally (scientific theories describe ways the world could be), so that the entities postulated in a theory will be putatively real, nevertheless not all entities are on a par from an epistemological point of view. One can make a distinction between observable and unobservable entities (this distinction will be vague, but then so are many distinctions of practical importance), and one can then use it to characterise the aim of science and what constitutes commitment when accepting a theory.

For Van Fraassen the aim of science is to produce theories that are *empirically adequate*, where a theory is empirically adequate if it is correct with regard to all possible observations; and acceptance of a theory involves only the belief that the theory is indeed empirically adequate. This contrasts with what Van Fraassen takes to be the best and most accurate characterisation of scientific realism, namely ([9], p. 8):

> Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.

Realist belief in the truth of a theory is typically justified by arguing that certain non-empirical criteria for theory choice, e.g. simplicity, unification, and particularly explanatory power are epistemic virtues of theories, i.e. indicators of the truth of a theory. ('No-miracles' argument: if our theories were not true, their success would be a miracle.) A constructive empiricist will also use non-empirical criteria in theory choice (on top of empirical ones), but these are only pragmatic virtues, ones that make the theory useful. (Only slightly stretching the evolutionary analogy that Van Fraassen uses: one could say it would in fact be a miracle if our theories were 'optimally adapted' to the environment of the world at large beyond the empirical selective pressure that actually takes place.)

I shall take all the above aspects of constructive empiricism for granted and include them in the present proposal.

Let us consider, however, the particular way Van Fraassen makes a pragmatic choice of what to consider clear cases of observation. First of all, he chooses the relevant epistemic community. Indeed, by 'observable' he means observable by human beings, because after all we want to characterise the aim of human science. Note that while the notion of observability refers to humans, the presence or even existence of humans will not be necessary for an object to be a possible object of observation. (The same kind of properties will make a dog, a dinosaur, or some comparable being on a distant galaxy observable in the sense of Van Fraassen, nor will these properties be affected if human beings go extinct or if they never existed.)

Van Fraassen then takes as clear cases of observation *what the theory tells us we could in principle observe under normal conditions unaided by instruments*. Note that theory-ladenness is explicitly taken into account: the notion of observability will eventually be made completely precise only within the theoretical framework of our ultimate physics and biology. (One might also add, however, that given the uncontroversial nature of most physiology of perception, Van Fraassen's notion of observability is in fact unlikely to undergo major revisions in the future.)

This particular choice for the boundary between observable and unobservable of course is arbitrary, but that need not be a problem as long as Van Fraassen can successfully argue that: (a) in our epistemological concerns we indeed make a (vague) distinction between some entities for which we *care* whether we get stuff right about them, and other entities for which we do *not* care whether we get stuff right about them; (b) his distinction between observable and unobservable entities is a reasonable precisification of this vague distinction, in the sense that it does not grossly mis-classify as observable some cases of entities we do not care about, or as unobservable some cases of entities we do care about. My suggestion will be that we do make such a distinction as in (a), but that Van Fraassen's distinction in (b) might be both too restrictive and too rigid in not adapting to what may be changing (and possibly widening) attitudes towards *what* we care to get right in our theorising.

3. A detour through underdetermination

I now wish to look at some cases of *underdetermination of theory by data*, to try and tease out some problems with both scientific realism and (I suggest) with standard constructive empiricism. Underdetermination is not essential to the argument (and not used by Van Fraassen), but will provide a good focus. I shall use cases of 'strong' underdetermination, where theories are empirically equivalent under *all possible observations*. This is itself a somewhat controversial notion, and it is easy to construct relatively uninteresting examples (Cartesian demons and the like), but I believe that it goes unappreciated how for all major fundamental physical theories of the last 300 years there exist *serious* empirically equivalent alternatives. I hope to expand on this claim in a separate publication, but the following will give a flavour of what I mean and provide us with examples for the discussion.[2]

Newtonian mechanics vs Barbour-Bertotti theory: Centrifugal effects are defined in terms of rotation with respect to absolute inertial structure, as is absolute duration (Newtonian mechanics). OR: Centrifugal effects are explained through rotation with respect to other matter, and absolute rotation does not make sense; similarly, absolute duration arises from relations between successive configurations of matter (Barbour-Bertotti theory). [The two theories are equivalent under the assumption that no phenomena require that the universe as a whole should have (in Newtonian terms) non-zero angular momentum or non-zero total energy.]

Newtonian gravity vs Newton-Cartan theory: The geometry of space-time is Euclidean, force-free bodies move uniformly along straight lines, and gravity is a force (Newton). OR: The geometry of space-time is non-Euclidean, the trajectories of force-free bodies are geodesics, and gravity determines the geometry (Newton-Cartan). [The two theories are exactly equivalent.]

Maxwell theory vs Wheeler-Feynman theories of electromagnetism: The electromagnetic field is considered a real entity that exerts forces on charged particles, but exists even where there are no particles (Maxwell). OR: The electromagnetic field is a mathematical fiction, and what is real are the forces between actual charged particles (Wheeler-Feynman). [The theories are equivalent under the assumption that every (Maxwell) field emitted by some particle gets later absorbed by some other particle.]

Ether theory vs Special relativity: There is a universal medium (the ether) in which light propagates, and which defines an absolute standard of simultaneity and of rest,

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[2] For discussions and references see e.g. [10] for Barbour-Bertotti, [11] for Newton-Cartan, [12] for Wheeler-Feynman, and [13] for disequilibrium effects in de Broglie-Bohm theory. Actual ether theories were not strictly equivalent to special relativity (e.g. even though gravitational forces are negligible at intermolecular level, without a modification of Newton's gravitation, intermolecular forces could only have been approximately Lorentz invariant), so ether theory as empirically equivalent to special relativity is a *post factum* idealisation, closer to so-called neo-Lorentzian interpretations of special relativity, for a discussion of which see e.g. [14].

but motion of material bodies in the ether is unobservable because rods contract and clocks slow down when moving through it (Ether theory). OR: The ether does not exist and simultaneity is conventional, or relative to inertial frames (Special relativity). [The theories are exactly equivalent.]

General relativity vs gravity in Minkowski spacetime: Spacetime is curved and free falling objects follow geodesics of the spacetime metric (General relativity). OR: Spacetime is Minkowski (i.e. the spacetime of special relativity), and there is a gravitational field that equals the difference between the Einstein metric and the Minkowski metric (Minkowskian gravity). [The theories are equivalent for all globally hyperbolic (general relativistic) spacetimes.]

Everettian quantum mechanics vs de Broglie-Bohm theory: The universe is described by one universal wavefunction with dynamically stable components corresponding to branching worlds (Everett). OR: The universe is composed by particles whose motion is guided by a universal wavefunction (de Broglie-Bohm theory). [The theories are equivalent under the assumption of (de Broglie-Bohm) 'sub-quantum equilibrium'.]

It is well known that such underdetermination is a problem for the scientific realist, because any two such empirically equivalent theories, if taken literally, will tell *different stories* about the world. Thus, the scientific realist is forced to accept inconsistent beliefs, or to believe that non-empirical criteria of theory choice (in particular explanatory power) must always be able to break the tie between underdetermined pairs of theories.

In the case of Newtonian gravity and Newton-Cartan theory, for instance, we have the choice between two different geometries of spacetime (flat or curved), and that seems a substantive question for the realist. The same choice can be put in terms of whether absolute acceleration exists, i.e. whether absolute inertial structure exists (Newton), or whether it does not and the split between inertia and gravity in fact does not make sense (Newton-Cartan). According to Newton himself (Corollary VI of the *Principia*) absolute linear acceleration is *unobservable*, because 'sensible motions' remain the same irrespective of the presence of a uniform gravitational field. For him this is purely a practical question (for calculational purposes one can assume the centre of the solar system is unaccelerated). But in Newton-Cartan theory, this equivalence between gravity and inertia is elevated to a *principle* (the same Principle of Equivalence as in general relativity), and there is no such thing as absolute acceleration.[3]

From the point of view of constructive empiricism the issue appears to be straightforward: one can simply see empirically equivalent theories as different ways the world could be, while caring in the first place only about their empirical adequacy.

[3] Even if the scientific realist only commits to the reality of those elements of the theory that are essential to its explanatory power (thus, for instance, not needing to be committed to any theoretical concepts that are unobservable in principle), gravitational forces or curved geometry both seem to be playing an essential explanatory role in the respective theories. Structural realism à la Worrall [6] is a position that develops this idea and may go some way towards alleviating such problems, but only if one is able to construe the differences between underdetermined theory pairs purely as differences in entities, and argue that all (essential) structure is the same.

If either of them is empirically adequate, then both of them are, and there is no problem in accepting them both. Any further choice between them will just be a pragmatic matter (even though unlike a radical conventionalist, a constructive empiricist will take it that there is indeed a matter of fact about which theory, if any, is in fact true).

4. Problems for constructive empiricism?

But now imagine that *neither* theory is empirically adequate (as indeed we might think none of the above examples are). In that case we might follow Kuhn [5] in taking promise of fruitfulness as particularly important in trying to choose between empirically equivalent theories: one of the two theories might lend itself more easily to be modified in such a way as to lead to a new theory that is empirically superior to both, at least in terms of overall fit. I believe in fact that the applicability of fruitfulness is a good criterion to distinguish in the first place between serious and unserious cases of underdetermination (but that is fairly inessential for what follows).

Newtonian gravity and Newton-Cartan theory again provide a nice example, even if ahistorical (since Newton-Cartan was in fact developed *after* Einstein's geometrical formulation of gravity in the general theory of relativity). If the choice between the two had been available at the time, then Newton-Cartan theory might have turned out to be the more fruitful theory, because it could have led straight into general relativity.

Alternatively, and even more counterfactually, Newtonian gravitation might have turned out to be more fruitful, if some theory of a gravitational field on Minkowski spacetime that was not empirically equivalent to general relativity had instead proved empirically adequate (specifically one with absolute linear acceleration, which is observable in Minkowski spacetime!). And we can give similar analyses of fruitfulness in all these examples. For instance, Wheeler-Feynman was originally developed in the belief that it would lend itself more easily to quantisation; or one might say that the ether theory outlived its fruitfulness when attempts to detect an ether wind repeatedly failed (notably the Michelson-Morley experiment).

Although *promise* of fruitfulness is strictly speaking a non-empirical criterion, one can argue that using it to resolve cases of underdetermination supports an empiricist position, because it ultimately relies on empirical differences between rival successor theories.

On the other hand, one can also argue that it supports a typically realist intuition: that the essential elements of a successful theory should be observable in principle. Indeed, recall what is so distressing for the realist about underdetermination: it is the idea that, say, absolute rotation exists, but it may not have any observable effects; that, say, the ether exists, but there may be no observable effects of motion with respect to the ether; that, say, Bohmian corpuscles exist, but there may not be any sub-quantum disequilibrium. The realist can use promise of fruitfulness to resolve cases of underdetermination *precisely* because they believe that if absolute rotation, or the ether, or Bohmian corpuscles really exist, then in principle there ought to be effects that make them observable – if not in the original theories, then at least in the hoped-for successor theories.

Put slightly differently, cases of strong underdetermination rely on there being certain substantive differences between theories (e.g. entities or notions in one or the other of the theories) that are unobservable in principle according to the realist's own standards. These cases then get resolved (according to our realist) because in the successor theories certain entities or notions turn out to be in principle observable after all.

The constructive empiricist instead can live with underdetermination, so they do not necessarily expect that there ought to be effects that are observable (in Van Fraassen's sense of the term) distinguishing between theories with or without absolute rotation, the ether, Bohmian corpuscles, curved geometry, Wheeler-Feynman absorbers, or what not. Of course a constructive empiricist can take promise of fruitfulness as a pragmatic virtue, choose one particular story that could be true about the world, and use it as a starting point for developing new theorising or experiments that may eventually lead to new effects in terms of what is observable. And of course the realist and the constructive empiricist will agree on the predictions furnished by each theory. But there appears to be at least a difference in the urgency and motivation to resolve cases of underdetermination, and in the significance of the resolution.

In fact, the problem lies in the question of what for the constructive empiricist should *count as selective pressure* on a candidate for an empirically adequate theory. We are considering the case in which two theories in a given underdetermined pair are empirically inadequate. But any theory will suffer from empirical anomalies, and the question is that of identifying which anomalies may indeed lead to development of a new theory that is a better candidate for overall empirical adequacy, i.e. of whether some anomalies will prove fruitful to investigate, and which ones (to put it again in Kuhnian terms). If we do not care in the first place about whether our theory is correct about some unobservable entity, we might fail to appreciate the potential difference in fruitfulness between different empirically equivalent theories.

To use a somewhat overworked example: a scholastic astronomer who merely wished to 'save the phenomena' and considered what Galileo saw in his telescope an observable *artefact* of the telescope itself and not an observable *phenomenon* of the heavens, would not have been convinced of the fruitfulness of the Copernican system. They would have recognised that they might be unable to explain the production of these artefacts, but they would have considered such a gap in the predictive power of their theories a relatively uninteresting puzzle, to be conveniently shelved. Or more to the point (because the heavenly bodies are observable for Van Fraassen): if one had not cared about the reality of the ether, would the null results in ether wind experiments have led to a crisis in 19th-century electrodynamics?

The sound of these examples as realist criticisms of empiricist views should not mislead: I am not arguing that the realist is correct in considering everything to be observable in principle. But the intuition I am pushing is that what we consider observable (or more generally care about) will play a role in judging promise of fruitfulness: specifically, in passages between successive theories there will be *changes* in what is assessed as counting as observable, and such assessments are part and parcel of judgements of fruitfulness.

Contrary to what the constructive empiricist would say, some notions or entities may become ones we care to get things right about even though they are not observable in Van Fraassen's sense (Bohmian corpuscles might become such entities if observable effects of subquantum disequilibrium are discovered). Denying this might be somewhat reminiscent of construing the meaning of theoretical statements merely in terms of their observable consequences, as the logical positivists did.

But contrary to what the realist would say, at no point do we need to stick out our neck further than what we consider to be the observable entities or notions in this wider and more adaptable sense. Unless we are indeed trying to go further than Newtonian gravity, there is no point in worrying about whether geometry is flat or curved. If we accept a strict reading of the molecular forces hypothesis, the truth of statements about motion with respect to the ether becomes irrelevant to theory acceptance. A distinction between what is observable and what is not, if we identify it with the distinction between what we care about getting right and what we do not, remains at all times firmly in place.

5. Adaptive empiricism – a sketch

The distinction between what we care for and what we do not care for will be a vague distinction. There are standard ways to make sense of vague distinctions, e.g. supervaluation: for each use or user, a precisification is implied (1.86m is tall, 1.85m is short), and different such precisifications coexist; or fuzzification: continuous truth values (1.72m is 93% tall); or mixed strategies: coexistence of different fuzzifications. Of course these are all controversial to some extent (do they solve the paradox of the heap? or problems with inconsistent beliefs?). But what Van Fraassen is doing is fixing once and for all a precisification, and a rather extreme one at that (2.00m is tall, 1.99m is short; but he is Dutch after all!). The above examples suggest instead that the choice should not be fixed, but should also depend on contextual elements, presumably both theoretical and experimental (I am average height in Milan, I am definitely on the short side in the Netherlands!).

As mentioned, choosing observation to be unaided by instruments is clearly arbitrary but one could make a number of such choices without making a difference to what the aim of science and acceptance of a theory means in practice. Indeed, it makes arguably no difference to what theories we accept if we consider as observable the objects we see in a microscope, or instead the images that the theory of the microscope says we should be seeing.

Thus it seems that Van Fraassen is playing it safe by adopting a very conservative reading of observability, while it does not matter much where exactly he draws the distinction. But note the latter depends on the theory of the microscope being perfectly *stable* at the present stage of the development of science. Were we to question the standard interpretation of microscopic images, then where we draw the line suddenly becomes crucial (Hanson's first example: are Golgi bodies products of faulty staining techniques ([3], p. 4])?)

To make the example more vivid, if we imagine we are doubting the functioning of the telescope *and* the possibility in principle of crossing the crystalline lunar sphere, then we are back to the familiar example of the Aristotelians vs Galileo. We are disagreeing about *what* the phenomena are in the first place, and that affects which theories we are going to accept: observations *of* the heavenly bodies (if we allow them) are much more important (and thus can exert a much greater selective pressure) than observations of patterns on an eyepiece. (A Boscovich would not make anything of the readouts of the instruments at CERN.)

In order to understand this difference in status for these 'same' observations, one must arguably ascribe a more realist commitment to Galileo (as realists standardly do), and possibly some more realist ideas to the Aristotelians as to why these observations are *not* significant. Nevertheless, there is no need to be (and in fact there are problems with being) a realist: I propose merely that the empiricist *adapt* their definition of what is observable to theoretical (and experimental) contexts. An empiricist is no less an empiricist if they occasionally 'change of spectacles'.

Here is a proposal to do justice to these intuitions, which we shall call *adaptive* empiricism, and phrase (as both realism and constructive empiricism are phrased) in terms of the aim of science and theory acceptance.

The aim of science is twofold: it aims at determining what should count as (genuine, observable) phenomena, and at formulating theories that are empirically adequate in the sense of correctly predicting all such phenomena. *The aim of science becomes not only to save the phenomena, but also to determine the phenomena worth saving.*

Acceptance of a theory then means belief that the theory *gets the phenomena right*, in the similarly twofold sense that the theory correctly determines what the phenomena, the possible observations are (in a certain domain), and that it is correct about the results of such possible observations.

This proposal combines welcome elements of both realist and empiricist views. Note that an adaptive empiricist might very well be optimistic in terms of determining (discovering, constituting) ever more new phenomena. New things will become observable, like atoms, which until the end of the 19^{th} century were pragmatically useful, but only after Einstein's explanation of Brownian motion (and Perrin's experiments) became observable. Indeed, anything *might* become observable. But it might not. Changing attitudes towards what is observable need not necessarily be widening (this is not a slippery slope towards realism!), and at any stage in the development of science there will be clear-cut cases of what is and is not observable. We may decide that there are particulate-like phenomena inside protons and neutrons, but we need not go the realist's extra mile: presumably we can accept and use our current best theories of particle physics without sticking our necks out and believing the literal truth of all theoretical statements about quarks.

Of course, this is just a sketch. One needs to specify further how one should understand the determination of phenomena, and then argue that the resulting observable-unobservable distinction has all the features mentioned above. I shall not attempt a full discussion of this question here, but limit myself to a few pointers.

On the one hand, I think we need to take theory-ladenness more fully into account, and say that what is observable is *what the theory tells us is observable, with or without instruments*. This also needs to be made more precise, and one idea that may prove useful in doing so is Van Fraassen's own discussion of measurability in terms of empirical grounding [15].

On the other hand, we may also want to take on board the now classic lessons of the debates within the 'new experimentalism'. For Hacking [7], electrons are real because we manipulate them, but not quarks. We even have theoretical reasons to doubt that quarks exist in isolation – and thus presumably that they may ever be manipulated. But maybe Hacking is too restrictive: we do appear to 'observe' some particle-like structures within protons and neutrons. Maybe we should rather follow Galison, with his notion of phenomena becoming 'direct, stable and stubborn' (see [16], Chap. 5).

Note that we need to be careful in order to avoid a predicament like with realism. If in Newton's theory non-uniform gravitational forces are observable, and in Newton-Cartan theory the curvature of spacetime is, we do not want the adaptive empiricist to accept both theories in the sense of believing that both correctly identify *different* phenomena. But careful analysis of what empirical equivalence means should be able to defuse these worries. (There is a sense in which Kepler and Tycho see different things, but also a sense in which whether the Earth or the Sun moves is unobservable; cf. [3], pp. 5–8.) As long as we consider only Newtonian gravitation, we want to be able to identify gravitational phenomena that are neutral with respect to forces or spacetime curvature, but we want the metric field to become observable (or rather the metric field possibly up to a constant Minkowski metric!) as soon as we have identified the appropriate non-Newtonian gravitational phenomena. Indeed, deciding what is observable will partly determine whether or not we rest content with Newtonian gravitation. (Cf. also the remarks on theory equivalence in [11].)

Maybe there are also issues about different scientists adopting slightly different criteria for what is observable, and changing their minds at different times, thus contributing to the vagueness of the collective notion (cf. again Kuhn's classic discussion in [5]). (Or maybe a better modelling of how the observable-unobservable distinction works requires adding some fuzziness.)

Be it as it may, it seems to me that the point of view of adaptive empiricism ought to be developed further, that analysis of underdetermination may be particularly useful in developing the view further, and that it is indeed a valuable point of view to consider in the realism-antirealism debate. We need not commit once and for all to an observable-unobservable distinction, but we can still enjoy epistemic modesty and not stick our neck out more than we need to.

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