

# Practical Inadequacy: Bas van Fraassen's Failures of Systematicity

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## Introduction

Clifford Hooker (1974:1987, cf. Sellars, 1962) has argued that any adequate philosophical account of science must be “systematic”. A systematic account of science is one that can play a part in a “coherent account of the entire human animal and the world in which he lives” (1987, p.7), able to account for the uses, methods, goals, and place of actual human science, as it exists within its institutional and industrial context. Accepting this criterion of adequacy, I argue that Bas van Fraassen’s anti-realist empiricism, in all its various guises, fails to be systematic. Because of this, van Fraassen has been forced to “advocate a strained and fragmented picture of science” (1987, p.174) in order to make his account of science seem plausible.

Van Fraassen has written several books and articles over the years, but I concentrate here on what I see as his three main works: *The Scientific Image* (1980), *The Empirical Stance* (2002), and his newest book, *Scientific Representation: Paradoxes of Perspective* (2008). In criticizing his developing views, I have naturally divided the main body of the paper into three sections: one for each of these books. Working chronologically, I show at each stage how van Fraassen fails to account for science’s social-political role as a tool for anticipating the future, which is a key feature of science in the modern democratic industrial context. This failure to provide a systematic account of science represents a major and continued deficiency in van Fraassen’s ever-developing views of science and philosophy, one that is not present in the views of his main philosophical opponent: the scientific realist.

## §1: The Scientific Image

Van Fraassen published *The Scientific Image* at the time when scientific realism seemed to have the only philosophically tenable answer to the simplest question that all philosophical accounts of science must answer: “what is science?” This book presented a new anti-realist empiricist answer to this question, a philosophical account of science that van Fraassen dubbed “constructive empiricism”.

In *The Scientific Image*, the question “what is science?” was resolved into two separate questions:

- (1) What is the aim of science?
- (2) What kind of attitude does a scientist take towards a theory when they endorse that theory?

Accordingly, van Fraassen characterized and contrasted constructive empiricism and scientific realism in terms of their respective answers to (1) and (2):

**Scientific Realism:** “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.” (1980, p.8)

**Constructive Empiricism:** “Science aims to give us theories which are empirically adequate; and acceptance of a theory involves a belief only that it is empirically adequate.” (1980, p.12)

In *The Scientific Image*, a theory is understood as “empirically adequate” when it is true with respect to everything that is both actual and observable (1980, p.197-8). A theory that is true *simpliciter* will also be true with respect to unobservable things, such as electrons, microbes, and causal structures. Introducing some terminology, van Fraassen called an endorsement of a theory as true “belief” in that theory; in contrast, an endorsement of a theory as empirically adequate was called “acceptance” of that theory.<sup>1</sup> In such terms, the claim that constructive empiricism is an adequate account of science is understood best as the claim that “in our philosophical reflection on science, we do not have to interpret science as an activity which involves search for, and belief in, theoretical truth in order to account for its salient features and for its empirical success” (Psillos, 2000, p.58); rather, the constructive empiricist’s interpretation of science as the search for, and acceptance of, empirical adequacy suffices to account for all the distinctly scientific phenomena.

### ***Constructive Empiricism and the Conjunctive Practice***

One aspect of science that anti-realists have often had trouble accounting for is something called the conjunctive practice. When scientists design experiments and treat their experimental results as significant, they are assuming not just one locally well-tested theory but a *conjunction* of locally well-tested theories. Such conjunctions will, often times, entail observable consequences that were not entailed by any of their conjuncts independently. The practice of conjoining theories together for experimental purposes, however, does not make sense *prima facie* unless we interpret scientists as *believing* the conjuncts (i.e. believing them to be true); for empirical adequacy, unlike truth, *is not necessarily preserved under conjunction*. Thus, an account of theory endorsement as anything short of belief seems to miss out on a key aspect of theory endorsement as it plays into the methodology of science (i.e. preservation under conjunction).

In *The Scientific Image*, van Fraassen anticipated objections based on the conjunctive practice by looking at the purpose such conjunctive practices serve in experimentation and theory building (1980, p.83-87). On van Fraassen’s account, scientists, *qua* scientists, never conjoin already accepted theories together in order to *believe* their novel empirical consequences; rather, they only conjoin them together to *test* the empirical adequacy of the resulting conjunction. To introduce some new terms, we can say that, on van Fraassen’s view, scientists do not need to “conjoin-to-believe”, but only “conjoin-to-test”. As Hooker (1985:1987) puts it, van Fraassen’s claim is that “the real and anyways methodologically sufficient motive for conjunction is to test the empirical adequacy of the conjoined theories” (1987, p.173). As a result, any philosophical

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<sup>1</sup> Accepting a theory, to be sure, does involve some belief, namely the belief that the theory gives an accurate account of the observable phenomena; it does not, however, involve a belief that the theory gives an accurate account of the unobservable phenomena. On such matters, those who only “accept” theories remain agnostic.

account of science need not account for the non-scientific phenomenon of “conjoining-to-believe”, when and where it does occur, in order to be adequate; constructive empiricism, easily, can account for the scientific phenomenon of conjoining-to-test. Thus, constructive empiricism is immune to objections based on the prevalence of conjunctive practices as they appear at the core of the scientific method.

Nevertheless, there is reason to doubt constructive empiricism’s adequacy as a philosophical account of science once we demand a more systematic account of scientific inquiry, for society’s goals are best served by a science that aims for true theories, rather than one that aims merely for empirically adequate theories. This is because the society in which modern science is practiced requires theories that can legitimately be conjoined-to-believe. It is only by artificially cordoning off these social-political interests from “purely scientific” interests that van Fraassen is able to maintain the adequacy of constructive empiricism.

### ***The Role of Conjunction in the Political Activities of Anticipation, Avoidance, Prediction, and Prevention***

Scientists are not *just* scientists. As citizens of industrialized Western democracies, for example, many of today’s scientists are also public policy makers. Unlike scientists *qua* scientists, scientists *qua* policy makers will often need to extend well-tested theories under conjunction into untested domains, and they will need to *believe* the novel predictions made in these untested domains as well. This is because policy makers often cannot, for practical reasons, directly test the novel predictions of such conjunctions without bringing about the undesirable consequences they wish to avoid by using science to inform public policy in the first place. As a result, “acceptance” is an inadequate characterization of the attitude that policy-makers (some of whom are, at times, also professional scientists) often need to take when they endorse scientific theories.

As a concrete example, consider crafting policy on the basis of global warming models of climate change. Global warming theory has been constructed through the conjunction of several very diverse theories—theories about the chemical properties of certain substances (carbon dioxide, water vapour, etc.) and their possibilities for radiant heat exchange, about the emissions resulting from our burning of fossil fuels, about the impact of solar flares and radiation on Earth’s climate, about the composition and dynamics of Earth’s atmosphere, etc. The resulting conjunction makes a prediction whose accuracy or inaccuracy is of immense practical import to us: it predicts that the continued burning of fossil fuels will result in catastrophic global warming over the coming century. The conjuncts, to be sure, have been well-tested independently and locally, but their conjunction in the form of global warming theory has not been. And, quite unfortunately, testing this conjunction against the null hypothesis of natural variation would require pumping gigatonnes of carbon into Earth’s atmosphere to see if catastrophic global warming would result. But this test-condition is precisely the circumstance we wish to avoid by allowing science to inform our public policies!

Pumping gigatonnes of carbon into the atmosphere and observing the climatological results would be, from the scientific perspective, an effective *test* of global warming theory against the null hypothesis. On van Fraassen’s account, the *scientific* thing to do would be to remain

agnostic about whether, in the long run, it was global warming theory or the null hypothesis that would prove empirically adequate (i.e. be empirically vindicated by such tests), for scientists only conjoin-to-test, and never conjoin-to-believe. But, clearly, pumping gigatonnes of carbon dioxide into the atmosphere works against the policy-maker's goals of *predicting, anticipating, preventing, and avoiding* catastrophic global warming *before it occurs*. Such goals are better served by doing what, to the constructive empiricist, is distinctly unscientific: conjoining-to-believe, rather than only conjoining-to-test.

Generalizing from this particular case, we can see that our modern, industrialized, public policy-driven democratic nations would be best served by a science that aimed at theoretical truth, rather than only at empirical adequacy. We should not, therefore, be surprised if many practicing scientists seek theories that capture deep truths, perhaps even at the expense of increased empirical adequacy, for given our social-political goals true theories will be much more valuable than empirically adequate ones. Policy-makers might even choose not to rely on the most empirically adequate theories available, simply because they think that theories with more deep truth to them will more reliably conjoin with other theories. The realist is in the unique position of being able to account for this; and, given how intertwined policy interests and scientific inquiry are in actuality (cf. Hooker, 1987, esp. Ch.7), it seems somewhat arbitrary to deem the useful search for true theories an “unscientific” activity, “external” to distinctly scientific interests, as the constructive empiricist must in order to remain plausible.

Nevertheless, the adequacy of constructive empiricism as a philosophical account of science ultimately comes down to a choice about what an adequate theory of science needs to account for—should a philosophy of science be able to account for the social-politically motivated goals of scientific inquiry in the modern industrial context? More simply, need a philosophy of science be systematic? I think so. So does Hooker. Van Fraassen clearly does not. But even if we agree to deem the use of science to predict, anticipate, prevent, and avoid undesirable circumstance by crafting good public policies a non-scientific phenomenon, we nevertheless have good reason, based on our non-scientific activities and goals, to make true theories our aim as scientists, over and above empirically adequate theories. Given the inevitable influence of practical concerns over scientific interests *in the actual world*, it is clear that constructive empiricism, unlike scientific realism, fails as a *systematic* account of scientific inquiry.

## §2: The Empirical Stance

In the years after 1980, it became clear just how mild van Fraassen's thesis in *The Scientific Image* was. Van Fraassen did not argue that we *need* to be anti-realist empiricists, nor that we *cannot* be realists, but only that we *may* be constructive empiricists, i.e. that constructive empiricism is an adequate and rationally acceptable philosophical account of science. Because such a thesis does not really make sense on a conception of rationality, according to which rationality is a *determinant* of belief, van Fraassen began to develop a conception of rationality according to which rationality is a *constraint* on belief, rather than a determinant of it. This account of rationality began to take shape in his response to critics of *The Scientific Image* (1985) and in his *Laws and Symmetry* (1989), but it was mostly developed in *The Empirical Stance*. This new, more minimal conception of rationality provided van Fraassen with a meta-

philosophical framework in which to evaluate, discuss, and choose between various philosophical commitments.

With this framework in hand, van Fraassen was able to leave the specific question “what is science?” behind and instead investigate the question “what is and what could be empiricism?” Van Fraassen’s answer is that empiricism is an “epistemic stance”, a kind of philosophical or methodological commitment that meets the minimal constraints of rationality. In general, an epistemic stance is a cluster of commitments, values, assumptions, and other attitudes that together constitute a set of epistemic policies for the generation of factual beliefs.

The policies of the “empirical stance” are mostly aimed at banning the epistemic practices permitted by an alternative epistemic stance: the “metaphysical stance”. Speculative metaphysicians (i.e. those who adhere to the metaphysical stance) seek and value *explanations* of the observable world, even if such explanations invoke concepts like “cause,” “reality,” “power,” or otherwise make assertions about unobservable things that we cannot perceive. Contradistinctively, empiricists, *qua* empiricists, think that the search for such explanations is useless, risky, and to be avoided. To be an empiricist in this sense is just to believe that our best method of acquiring knowledge is the scientific method of experimental testing, and that *a priori* theorizing and metaphysical speculation about the unobservable nature of reality, even in tandem with scientific theorizing, is a poor method of acquiring knowledge. Given that they both meet the minimal constraints of rationality, accepting the metaphysical stance or the empirical stance is a *choice* that all philosophers must make for themselves; *The Empirical Stance* recommends that we all choose to abandon the methods and excesses of metaphysics for the more conservative methods of empiricism.

### ***Rationality, Radical Critique, and Stance Choice***

Van Fraassen describes his new conception of rationality, which makes possible his conception of empiricism and metaphysics as epistemic stances, as “bridled irrationality”; a belief or commitment is rational just in case it is not irrational, where a set of beliefs or commitments would be irrational just in case it was a) logically inconsistent (e.g. it disobeyed the probability calculus in assigning degrees of belief) or b) self-defeating by its own lights (e.g. led one to engage in activities that it nevertheless deemed unwise). It is in this sense that rationality merely constrains belief and commitment, rather than determining them.

On this view, what determines one’s beliefs and commitments within the constraints of rationality is one’s idiosyncratic set of *values*. Since we are, for the most part, relatively free to choose what we value, our acceptance or rejection of empiricism over metaphysics is seen as an essentially free choice—an act of will or volition. Philosophers have, accordingly, begun to refer to this view of rationality, values, and stance acceptance as “epistemological voluntarism”. This “new epistemology” represents a kind of existentialist turn in epistemology and the philosophy of science, for it puts the role of *choice* and *values* in the formation of beliefs and philosophical commitments front and centre.

As Chakravartty (2004, 2007) notes, however, this voluntarist defence of empiricism prevents van Fraassen from maintaining any kind of “radical” critique of metaphysics, for he cannot

condemn metaphysical speculation as *irrational* given that it meets the minimal constraints of rationality. That is, epistemological voluntarism secures the rational tenability of empiricism only by doing the same for metaphysics *absent of any presumed set of values*. Chakravartty summarizes the situation by saying that “empiricists are not so much arguing as asserting a distaste for the metaphysical stance” (2004, p.177).

Since rationality alone cannot decide our choice of stance, we must look to our values to decide which stance will be best for us, as individuals. This makes the relevant question for anyone evaluating these two stances “which stance best serves *my* values?” I argue that, based on many of our shared and socially manifest values, we all would be best served by the metaphysical stance rather than the empirical stance.

### ***Stances and Social Values***

Consider the four mainly, though not purely, epistemic/cognitive goals, values, or desires that van Fraassen outlines in *The Empirical Stance* (p.86-90):

- (1) truthful information
- (2) freedom from error
- (3) a good ratio of true to false beliefs
- (4) not to risk error for irrelevant information

While these goals are all surely shared by empiricists and metaphysicians alike, they not *purely* cognitive or truth-directed because they also contain subjective elements of taste or preference; we must all make our own decisions about how much risk we are willing to take and what kinds of information we are willing to take risks to obtain. Once we make these decisions, we will be in a position to decide which epistemic stance is best for us.

Van Fraassen’s rejection of the metaphysical stance is based primarily on desideratum (4): his charge is that the sort of information about unobservables sought by metaphysicians is ultimately *irrelevant* information, and that the risk of being wrong is therefore not worthwhile, for there is nothing to be gained by taking on such additional risk. The total irrelevance of information about unobservables supposedly results from the fact that knowing the observable consequences of certain unobservable facts would be just as useful to us as knowing the unobservable facts themselves. However, this only holds with respect to the traditional philosophical problems van Fraassen deals with in *The Empirical Stance*, such as rationalizing scientific revolutions. As van Fraassen argues, the empiricist can still tackle these traditional philosophical problems without being overly metaphysical; but it does not follow from this alone that information about unobservables is *always* useless and irrelevant. In fact, having information about unobservables can be very useful to us when we cannot obtain information about observables directly and yet wish to predict, anticipate, prevent, and avoid undesirable future states of observables.

To continue, now in the context of *The Empirical Stance*, with the example of global warming theory, we can see how information about unobservables would be extremely relevant to our policy forming purposes. Clearly we cannot know, through direct empirical testing and with enough time to act on such information, that global warming theory is empirically adequate

where it matters most—at the point where it diverges from the null hypothesis by predicting catastrophic global warming as a result of anthropogenic increases in atmospheric carbon dioxide. If we *only* sought information about observables, such direct empirical testing would be the only way to determine the empirical adequacy of global warming theory over the null hypothesis. But, if we had information about the unobservable behavioural dispositions of carbon dioxide, the Earth’s atmosphere, etc., we would have information about the behavioural dispositions of these unobservable entities in untested circumstances; and, more importantly, this would give us information about the future observable consequences of such behaviour in the absence of direct empirical testing. In this way, information about unobservables can give us information about whether continuing to burn fossil fuels at our current pace would result in catastrophic global warming, *even if we never did any such thing!*

In general, information about unobservables like dispositions and causal structures can provide us with important information about the future state of observables, even if our actions make this information counterfactual. Such information about observables is very important for those engaged in the activities of prediction, anticipation, prevention, and avoidance; and yet, the empirical stance does not allow us to seek out the information about unobservables that would help us to know the relevant observable facts in time to act on such knowledge. Thus, insofar as one is interested in avoiding things like catastrophic global warming on the basis of currently available data, metaphysical speculation into the nature of unobservables cannot be condemned as a risky search for irrelevant information. In fact, *not* seeking such information is the risky activity.

Thus, since they are also citizens of industrialized democracies like Canada and the U.S.A., today’s philosophers and scientists will often have good reason to pursue goals banned by the empirical stance using techniques banned by the empirical stance. In this way, it is much like it was in *The Scientific Image*: van Fraassen’s brand of empiricism in *The Empirical Stance* cannot account for our epistemological needs when we are engaged in the social-political projects of prediction, anticipation, prevention, and avoidance. The empirical stance is thus deficient as a systematic approach to epistemology; that is, it is inadequate as an epistemic stance as soon as we move beyond the narrowly defined problems traditionally dealt with in the philosophy of science. The metaphysical stance does not share this deficiency, and is uniquely capable of serving the values that philosophers and scientists hold *qua* public policy makers.

### **§3: Scientific Representation**

Van Fraassen’s *Scientific Representation: Paradoxes of Perspective* has rightly been seen as “the true successor to *The Scientific Image*” (Giere, 2009). Here van Fraassen again defends an anti-realist, empiricist account of science, but he does so within the context of several newly emphasized themes in the philosophy of science: structuralism, perspectivism, measurement, and representation. Accordingly, van Fraassen gives a new name to his updated philosophy of science: “empiricist structuralism”. Despite being much more elaborate than constructive empiricism, the core claim is essentially the same: the aim of science is the construction of empirically adequate representations, where a representation is empirically adequate just in case it accurately captures the structure of the observable phenomena, but not necessarily the unobservable phenomena.

One change in van Fraassen's view is of special note here. Philosophers of science, including van Fraassen, have begun to de-emphasize "truth" as an important concept for understanding how models relate to the world (e.g. Giere, 1999, cf. Chakravartty, 2007). The relation between models and world is now understood as a *representational* relation that employs techniques of distortion, highlighting, and ultimately trades on selective resemblance and similarity of abstract structure between higher-level models and data or surface models (cf. van Fraassen, 2008, pp.166-172). With truth out of the equation, and empirical adequacy understood as representation of the observable phenomena but not necessarily the unobservable phenomena, one might think that empiricist structuralism remedied the failures of constructive empiricism, proving capable of giving a systematic account of science. Unfortunately, this is not the case; empiricist structuralism also fails as a systematic account of science.

There are two distinct but related problems with empiricist structuralism. The first regards van Fraassen's method of conceiving of science, which is far too abstract and removed from scientific actualities to be of much use in understanding the processes and achievements of *human* science. Conceiving of science as he does leads van Fraassen to saddle scientific realists with unreasonable commitments, which realists need not and should not accept. The second problem is that empiricist structuralism fails to account for the way that science is used in public policy making, despite the fact that van Fraassen clearly and explicitly states: "a view of science would hardly be empiricist if it ignored the *uses* of science, as a resource for praxis" (2008, p.88). Because his empiricist structuralism has these two deficiencies, I argue, van Fraassen has again failed to give a systematic account of science.

### ***Van Fraassen's Method of Conceiving of Science***

In *Scientific Representation*, van Fraassen conceives of science in terms of its *telos*, envisioned end-state, or its "end-in-view", a method of conceiving of science first discussed explicitly in his reply to critics of *The Empirical Stance* (van Fraassen, 2004). Using this method of conceiving of science, van Fraassen envisions the end-state of scientific inquiry as the production of a theory or model that is completely empirically adequate. Using the same method of conceiving of science, van Fraassen portrays the scientific realist as envisioning the end-state of scientific inquiry as the production of a theory or model that shows how the state of all observables results from some underlying, unobservable reality. This is how the conflict between scientific realists and empiricist structuralists is portrayed in *Scientific Representation*: each party makes conflicting claims about what would count as a *complete* science (van Fraassen, 2008, Ch.12 and 13).

The problem with conceiving of science in terms of an envisioned end-state is that any such "end-in-view" will inevitably be far removed from actual human science, especially its current practice and achievements. As van Fraassen admits, we have good reason to believe that scientific representations will always be idealized (2008, Ch.2); science, therefore, is likely to be perpetually *incomplete* in that its representations can never be completely empirically adequate, but only ever approximately so.

Today's *philosophical* defenders of scientific realism recognize the idealized nature of scientific inquiry in their philosophical conceptions of science, contrary to the kind of realist van Fraassen

characterizes. In contrast to the scientific and over-exuberant, anti-philosophical conception of science espoused by scientists like Steven Weinberg in his *Dreams of a Final Theory* (1992, especially p.123-6), today's realist philosophers of science do not dream of complete sciences, or final scientific theories; at least, they do not *need* to in order to be realists. Ron Giere, for example, explicitly distances himself from Weinberg's view that science aims to develop a complete and final description of all aspects of reality (see Giere, 2006, p.4-6); and yet, van Fraassen continues to identify scientific realism with the views of Weinberg (see 2008, Ch.4, especially p.110-1).

Another view of the aim of science with a strong family resemblance to Weinberg's is the view espoused by Planck, which sees the achievement of a unitary world picture as a fundamental goal of scientific inquiry. Just as he does with Weinberg's philosophically naive dreams of scientific completeness, van Fraassen unjustly saddles the realist with Planck's unity-based view of science (see 2008, Ch.8, especially p.194), despite the clear assertion by many modern realists that being a realist does not require a commitment to the unity of science, even as a working hypothesis (e.g. Cartwright, 1999).

In contrast to Weinberg and Planck, most philosophically minded scientific realists are more than happy with the smaller, more local achievements of current science, e.g. knowledge of many unobservable entities and their behavioural dispositions, which allows us to accurately predict many novel observable phenomena. While yesterday's realists (e.g. Sellars, 1961, 1962, 1976; Boyd, 1973; and Popper, 1956:1983) regularly conjectured about the "true" aims to science, today's realists rarely do so (e.g. Giere 2006 and Chakravartty, 2007), and would do well to reject the claims about the scientific *telos* attributed to them by van Fraassen.

Not only do the conceptions of science attributed to realists by van Fraassen not do justice to the more qualified views of modern realists, but van Fraassen's entire method of conceptualizing science is itself problematic, especially for an empiricist. To conceive of science in terms of an ultimate *telos* is to abstract away from the actual practice, achievements, and purposes of scientific inquiry as it is and has been practiced by human scientists. Conceiving of science in terms of a single completeness criteria obscures our understanding of actual human science, which is not only inevitably incomplete but also *multiply motivated*. People do science for many different reasons, and there are many different points at which scientists might be willing to view their goals as "completed"—an operationalized science, consisting not of theories or models but of tables of rules that allow us to exhaustively manipulate the phenomena towards preferred states, would likely satisfy many scientists yet fail to satisfy others (such as the empiricist structuralist).

To argue over the "true" aim of science is likely futile, for it is doubtful that science has anything like a "true" or "essential" aim. All that modern realists need to claim in order to be realists is this: we can and should attempt to know the unobservable aspects of reality, and we have had many specific successes in doing this (e.g. Chakravartty, 2007). There is nothing in this claim that has any implication of an envisioned completeness to science, a dream of a final theory, a single reason for doing science, or a scientific *telos*. Simply put, realists should not conceive of science in terms of its envisioned end-state, as van Fraassen says they do (and does himself).

## *The Conditions for the Successful Use of a Scientific Representation vs. The Conditions for the Successful Use of Science*

In *Scientific Representation* van Fraassen also explicitly addresses the question “what are the conditions for the possibility of successful use of a scientific representation?” (2008, Ch.3). The account he gives, however, only details the conditions for the successful use of a *particular* representation that is in fact empirically adequate and has already been accepted as such; he does not discuss the problem of deciding *which* scientific representation to use, in the absence of prior knowledge about which representation *is* empirically adequate. Because of this, empiricist structuralism cannot account for our social-political activities of prediction, avoidance, anticipation, and prevention through policy crafting, for these activities fundamentally occur from *outside of any particular theory or representation*. When one wishes to craft policy in anticipation of the future, the first question that one must ask is “which scientific representation should I use and why?”, e.g. should we craft policy based on global warming theory, or based on the null hypothesis?

The realist has a ready answer to such questions—use the theory that has had most of its novel predictions vindicated, for this is the one that is most likely to have an accurate picture of the unobservable world and thereby the observable future. As many realists have argued (e.g. Musgrave, 1985, 1988, and Psillos, 1999), it is the vindication of *novel* predictions made by a particular representation that warrants a realist attitude towards that representation. For, as the so-called “No-Miracle Argument” for scientific realism notes, it would be a massive coincidence if experience continually vindicated the variegated observable predictions made by supposing that the unobservable aspects of reality were a particular way, despite the unobservable aspects of reality being radically different. Rather than accept such a miraculous coincidence, one should simply accept that such suppositions get the unobservables facts (mostly) right.

Adopting a realist attitude towards a representation on the basis of corroborated novel predictions also means accepting the *untested* novel predictions it makes, absent direct empirical testing. As realists carefully note, this legitimate projection of genuinely predictive theories into novel and untested domains contrasts with the projection of theories that have had their accurate predictions “written-in” to them in order to simply “save the phenomena”. Projecting theories with such *ad hoc* modifications to their predictions is (correctly) seen by realists as illegitimate projection into untested, novel domains. Such a discrimination between legitimate and illegitimate projection, which has great practical import, is not available to an anti-realist like van Fraassen; for him, saving the phenomena, even by “writing-in” correct values, is in fact science’s “bottom-line” (2008, p.136).

Thus, realism gives us a unique ability to discriminate *legitimate* commitments to novel predictions from *illegitimate* ones. Rather than tackling the practical problem of induction head on as realists do, the empiricist structuralist must always defer to direct empirical testing of conflicting representations that make divergent observable predictions, just as the constructive empiricist would be forced to do. Thus, van Fraassen’s empiricist structuralism is unable to account for or guide the use of science in practical matters, which often requires discriminations that cannot be made on empiricist structuralist grounds.

## Conclusion

Van Fraassen discussed the possibility that his account of science as the search for empirically adequate theories would prove to be a poor account of what human scientists are doing (1994, p.191). In such a situation, he notes, he would need to either give up his belief that science exists in human culture, or else alter his philosophical account of science. Given his empiricist scruples, which lead him to believe that we need only account for what is both actual and observable, it is surprising that he admits that the choice would be a difficult one. But, I ask, what could be more sinful for the empiricist than theorizing and arguing *a priori* about the nature of something that does not even exist in the real world? Only, perhaps, valorizing that uninstantiated activity.

As van Fraassen himself says, abstraction is only innocuous under very strict conditions of pertinence (2008, p.48); it is highly unlikely, however, that abstracting science away from its social-political context will meet such strict conditions. Thus, if we are to understand this actual human activity we call science, especially in the modern industrial context, our account of it should be systematic. Unfortunately for van Fraassen, all of his various empiricisms have failed to meet this very important criterion of adequacy.

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