

Critical Notice

BAS C. van FRAASSEN. *Scientific Representation: Paradoxes of Perspective*. Oxford: Oxford University Press 2008. Pp. xiv + 408.

For three decades, Bas van Fraassen's constructive empiricism has been the most influential anti-realist position in philosophy of science. When logical empiricism lost its place as the leading philosophy of science, support for the anti-realism associated with it also waned. Van Fraassen's brand of empiricism, although significantly different from that of the logical empiricists, has kept alive the anti-realist spirit of the earlier enterprise. In *Scientific Representation: Paradoxes of Perspective*, the author develops and defends a position he calls 'empiricist structuralism.' He agrees with the slogan that 'all we know in science is structure.' His challenge is to explain how this slogan can be meaningfully interpreted in the absence of commitment to any corresponding real structure in the world. Because my disagreements with *Scientific Representation* are fairly deep and because this is not an easy book, I shall begin with an account of its contents and leave the assessment to a subsequent section.

I Summary

Some Background: Many readers will know about van Fraassen's constructive empiricism chiefly through *The Scientific Image*, published in 1980. There he argued that the aim of science is empirical adequacy rather than truth:

Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. This is the statement of the anti-realist position I advocate; I shall call it constructive empiricism. (1980, 10)

He went on to say that ‘a theory is empirically adequate if what it says about the observable things and events in the world is true — exactly if it “saves the phenomena”’ (1980, 12). The notion of ‘observability’ employed here is narrow insofar as it is tied to our sensory apparatus; it should be understood as perceptibility. Detectability with the aid of instruments is not observability; so accepting a theory as empirically adequate does not require us to believe in any of the theoretical entities it postulates, even those that seem to make themselves known to us through instruments. On the other hand, ‘observability’ is broad enough to cover dinosaurs and distant stars that no one has or ever will observe — because if members of our epistemic community *were* properly situated, those members would be able to perceive such things without augmenting their sensory apparatus.

Somewhat more technically, constructive empiricism has been developed within the context of a semantic view of theories so that:

To present a theory is to specify a family of structures, its models; and secondly, to specify certain parts of those models (the empirical substructures) as candidates for the direct representation of observable phenomena. (1980, 64)

Given that empirical adequacy is the sole aim and criterion of science, theory acceptance does not require belief that the theory is true. The only *belief* required by acceptance of a scientific theory is belief that its observational consequences are true. Acceptance also requires that we commit to *using* the theory, but that is a *pragmatic* rather than an epistemic consideration. To the predictable complaint that his view strips science of its explanatory power, van Fraassen has consistently replied that explanation is no part of the aim of science. Explanation serves pragmatic rather than epistemic ends, and the search for explanation tends to produce metaphysics rather than science.

If you personally want to believe that the theories you accept are true and that their unobservable components refer to real things in the world, you are free to do so. Constructive empiricism is permissive in that respect. It does not *advocate* agnosticism about theoretical entities or about the truth of theories (although it has often been understood that way). Constructive empiricism simply says that once you decide to become a believer, you have gone beyond science into the realm of interpretation or metaphysics.

Van Fraassen has modified some of his views since 1980. Two of these changes bear mentioning here. One concerns his understanding of what it is to be an empiricist. He now describes empiricism as a *stance* rather

than a set of beliefs (van Fraassen, 2002). He repudiates his earlier claim that empiricists maintain that experience is our one and only source of information about the world. Indeed he expresses amazement that he could ever have said such a thing and acknowledges that he had tried to create an empiricist philosophy of science without seriously considering what empiricism is. (See, for example Monton, 2007, 366.) To adopt the empiricist stance is to display certain *attitudes* that include taking the empirical sciences as a paradigm of rationality and resisting demands for further explanations that lead to the introduction of metaphysics.

Van Fraassen has also abandoned any objective notion of probability. He had once subscribed to a moderate frequentist interpretation and thought that empirical adequacy could be defined for probabilistic theories in much the same way as for others. The challenges of quantum theory led him to outline a 'modal frequency' interpretation of probability, but that approach failed. By the time he published *Laws and Symmetry* (1989), van Fraassen had embraced a subjectivist approach, arguing that belief must be replaced by the nuances of gradated opinion, modeled as personal probability. The probabilities offered by an accepted theory guide personal expectations differently depending upon whether one believes everything the theory says or only what it says about observable phenomena. The realist will accept the theory's guidance for all theoretical probabilities, whereas the 'scientist *qua* scientist' will be guided only by the theory's probabilities for observable phenomena.

A third point worth mentioning is not so much a change of position as a continuing preoccupation. Van Fraassen's notion of rationality as nothing more than 'bridled irrationality' has been further developed in ways that sometimes tap into explicitly existentialist themes. An individual's decisions about what to believe are constrained only by the requirement to avoid incoherence, reason providing no further guidance. In science as in questions of morality, the individual is condemned to freedom. The emphasis in *Scientific Representation* on the first-person singular pronoun fits rather well with the existential hero who figures in many of the author's less technical writings.

Overview

In his new book, van Fraassen's main objective is to define an empiricist version of structuralism. If science is said to describe only structure and nothing more, a position that is once again attracting considerable attention, there remains room for disagreement about the types of structure that are described and the nature of the structures that are the proper object of scientific inquiry. Van Fraassen remains a *constructive*

empiricist, but he hopes that his structuralist program will appeal to empiricists of all stripes. Because it is obvious that a version of structuralism compatible with constructive empiricism cannot seek to describe 'the underlying structure of the world,' an obvious question to ask is what it might do instead. What could an empiricist structuralism look like?

As prelude to developing an answer to this question, van Fraassen introduces a new distinction: 'appearances' vs. 'phenomena.' This distinction will be required to carry a great deal of weight in the ensuing argument; so it must be attended to carefully. Whereas before, we might have spoken interchangeably of 'saving the appearances' or 'saving the phenomena,' for example, van Fraassen introduces stipulative definitions that will henceforth sharply distinguish his technical usage of the two words. '*Phenomena* will be observable entities (objects, events, processes). ... *Appearances* will be the contents of observation or measurement outcomes' (8). Thus phenomena possess a kind of reality, and appearances are *representations* of phenomena. Note well that appearances do not tell us what phenomena are like but only what they *look like in a given measurement or observation set-up*. This emphasis on *representation* defines the book's central theme. Theories are representations; models are representations; the contents of measurements and observations are representations (up to a point). As we shall see later, however, not all these representations are of the same type. Some are perspectival; others are not.

Part I. Representation

The first part of *Scientific Representation* draws heavily on references to art history and aesthetics. The main point is that in art, a 'representation is made with a purpose or goal in mind, governed by criteria of adequacy pertaining to that goal, which guide its means, medium, and selectivity.' Not all representation is mimetic, but even in cases that trade on (selective) likeness, there is 'no valid inference from *what the representation is like to what the represented is like overall*' (7). Similarly in science, our representations of phenomena will reflect our aims, purposes, instrumentation, and other pragmatic factors. These representations will not tell us what the phenomena are like, only what they *look like* under certain conditions.

Our evaluation of a representation as accurate or inaccurate is highly context-dependent. 'What does it represent?' should be understood as elliptical for a more complicated question to which the proper answer has the form 'Z uses X to depict Y as F' (21). *Use* presupposes intentional activity, and it determines our selection of similarities and dissimilarities. Van Fraassen underscores its importance this way: 'The notion of

use, the emphasis on the pragmatics rather than syntax or semantics of representation in general, I give pride of place in the understanding of scientific representation' (25).

Not all representations are images, and not all images are pictures. Images are representations that are effected through resemblance, and pictures are those images that bear the hallmarks of perspectivity. In science, the most important of these hallmarks of perspectivity is explicit non-commitment. Being committal in some respects precludes being committal in some other respects.

It is important for tracking the argument that follows to note that although we adopt certain perspectives in constructing and using a theory and its models, the 'official formulations of general scientific theories are *not* perspectival, and their models are generally not perspectival representations' (86). This reference to the 'official formulations' and to the distinction between process and product, or as van Fraassen now has it, the distinction between the view 'from within' and the view 'from above' is crucial, marking an important theme in the arguments that will follow.

In addition to perspectivity, there is another pragmatic consideration that plays a critically important role in the argument, and that is indexicality. For more than 15 years, van Fraassen has been using maps as a way of introducing the importance of indexicals, and he does so again here. The human beings who use maps must locate and orient themselves relative to the map, but this requires something that is not in the map itself. The 'you are here' marker must be supplied from outside. It is part of pragmatics rather than semantics, not part of the map's content but a crucial addition from outside.

As with maps, so in science: 'the scientist or scientific community must supply something extra, which does not come with that body of science, but serves to locate the user with respect to it. ... The extra is the self-ascription of location' (82-3). In the end, 'the entry of the essential indexical will prove to be the crucial clue to how science can depict at all' (88). It will be the linchpin in van Fraassen's solution to the problem of coordination: how to make the theory and its models count as *empirical representations*, not just pieces of pure mathematics. So it is no exaggeration to say that the success of this book's argument depends upon whether van Fraassen succeeds in using indexicals to establish the necessary linkages.

Part II. Windows, Engines, and Measurement

The focus in Part II is on the view 'from within' rather than the view 'from above,' on the journey that leads to scientific theories rather on the range of models that the theory eventually offers as candidate represen-

tations of the phenomena. The first major point concerns a distinction between 'windows' and 'engines.' Instruments do not 'open a window on the invisible world'; they do not extend the range of human perception. Instead, says van Fraassen, we should think of our instruments as *engines of creation*. They enlarge the observable world by creating new phenomena, including some that might never have occurred otherwise. And once a phenomenon is created, it becomes part of what science must account for. Experimentation expands the range of the observable, and it is *theory writing by other means*, since it allows us to *fill in blanks* in theories that presently are under construction. This is what van Fraassen has dubbed the 'Clausewitz doctrine of experimentation.'

The measurement question is intimately tied up with the problem of coordination, the central challenge that must be addressed if empiricist structuralism is to be plausible. Van Fraassen's aim is to develop 'an account of measurement itself, as it appears in the process of joint experimental-theoretical development and as it appears through the eyes of the eventually achieved theory' (113). Note once again the distinction between process and product, expressed as views from two different vantage points.

The problem of coordination is that of establishing a linkage between theories expressed mathematically and the phenomena they are purported to account for. 'The theory would remain a piece of pure mathematics and not an empirical theory at all, if its terms were not linked to measurement procedures' (115). So the empiricist version of Kant's dictum that 'concepts without percepts are blind' is that 'theory without coordination is empty' (184). Previous attempts to establish this linkage have failed, argues van Fraassen, because they proceeded ahistorically — as if coordinations could be meaningfully introduced outside the historical context in which previous coordinations are already in place. There is, however, no presuppositionless starting point for the process of coordination; instead 'measurement procedures and theory evolve together, in a thoroughly entangled way' (184).

This point is illustrated through two case studies. The first is Mach's history of the thermometer, which emphasized the *choices* that were made in selecting instruments appropriate for thermometry. The chief lesson van Fraassen derives from this account is that the conditions of possibility for successful coordination involve both 'empirical regularities that are contingent and choices subject to conditions of coherence' (130). The second case study is Poincaré's analysis of time measurement. The take-home lessons are similar, but with a small twist at the end. Though mostly sympathetic to Poincaré's account, Einstein stressed that choice can enter at *multiple* points in the process of working out how to measure time. Van Fraassen suggests that this is a result of 'the inevitable holism in jointly constructed theory and coordination' (135),

and he stresses that we cannot single out any one result as the product of choice while treating other results as forced upon us. We must satisfy coherence conditions in deciding what is to play the role of physical correlate to measurement, but there is a great deal of leeway in how we do so. We have no recourse to any independent criteria beyond coherence. Poincaré's appeal to simplicity, for example, was an appeal to a cluster of *pragmatic desiderata*, says van Fraassen, and we may sometimes choose to sacrifice simplicity in favour of other pragmatic gains. There is only one bottom line in science, and that is empirical adequacy. Pragmatic factors can be 'reasons' for acceptance, but only in the sense of ulterior motives; they carry no epistemic weight.

Given the thorough historical entanglement of measurement and practice illustrated by the case histories, 'Somewhat hesitantly one might say that the measured parameter — or at the very least, its concept — is *constituted* in the course of this historical development' (138). Evaluating this suggestion brings van Fraassen back to the distinction between process and product, between the view 'from within' and the view 'from above': The parameter that is measured 'is identified in the historical process by the *envisioned eventual* stable measuring practice, while it is differently identified *in retrospect* by the theory that draws on that history for its credentials.'

[This] does not presuppose an impossible god-like view in which nature and theory and measurement practice are all accessed independently of each other and compared to see how they relate to each other 'in reality.' The two ways of looking at the matter we must combine in a synoptic vision. The first is 'from within' the historical process in which measurement procedures and theory are stabilized. The second is 'from above' with the theoretical description of the domain including the measurement interactions already in hand, a stage achieved in that historical process but no longer involving any explicit reference to its own history. (139)

Whether these claims can be justified is a question to be considered later.

The last two chapters in Part II deal with 'Measurement as Perspective,' treating first the physical correlate of measurement and then measurement as information content. The important point is that we must not confuse (1) the occurrence of the physical interaction with (2) measurement as representation of the value of a parameter. 'For measurement is information gathering, a measurement is something that has meaning, is in fact a representation of what is measured, and that point does not reduce to a physical condition' (156).

The final state of the apparatus in the interaction between object and measuring apparatus is not the measurement outcome; it is just the *physical correlate* of the measurement outcome. This outcome 'can't mean anything relevant unless it appears at the end of a suitably structured

physical interaction, and takes a particular sort of physical shape' (143). We cannot generalize properly from a measurement outcome unless we keep in mind that 'what was found was how things appear *in the measurement set-up*' (149).

In abstract terms, measurement locates an item that is already classified as part of given theory's domain in a logical space or 'field of possibilities' provided by that theory to represent possible characteristics or states of such items. '*The act of measurement is an act — performed in accordance with certain operational rules — of locating an item in a logical space.*' This abstract characterization is not, however, sufficient to solve the problem of coordination. It cannot tell us which measurements, if any, are 'genuine.' The distinction between what is and is not genuine measurement is answered *relative to the stabilized theory* that evolved jointly with the measurement processes (165). The 'stabilized theory' is what we see 'from above.'

The joint evolution of measurement processes with theory is visible only in the view 'from within.' Here pragmatic factors play a major role, and measurement outcomes are described as relational, intensional, intentional, indexical, selective in their resemblance, and perspectival. The view 'from within' reveals complex historical processes rather than abstract mathematical structures.

Although other pragmatic factors are important and perspectivity was the focus of earlier attention, it is indexicality that plays the most important role in van Fraassen's argument. In Part III, it looms especially large as the discussion moves toward a solution of the problem of coordination, a solution that will depend critically on indexical considerations.

Part III. Structure and Perspective

This is the most important part of the book. Van Fraassen's attempt to solve the problem of coordination and to develop a viable form of empiricist structuralism stands or falls with the success of the arguments developed here. He sets the stage by saying that conditions for the possibility of scientific representation 'can be brought together under one heading: the crucial role of use and practice' (189). This, he says, points to the study of *pragmatics*: 'the study of language in which word-thing relations are seen as abstractions from word-thing-use relationships.' (It should be noted that what is referenced here is not the pragmatics of Peirce, but the much more austere, formal, empiricist-friendly pragmatics of Charles Morris.)

Van Fraassen constructs a narrative, a sort of potted history, of structuralism in philosophy of science. His story begins with Boltzmann's *Bildtheorie* and moves through Planck, Maxwell, Hertz, and Mach (with a backward glance at Descartes) before coming to Poincaré and Duhem.

The details of the story are not as important as the conclusions that van Fraassen draws from the various episodes that make up his story. He grants that the *Bildtheorie* view of science (the ‘picture theory’ of science) is compatible with structural realism:

If what science gives us by way of theories and models is to be conceived as pictures, as representations, then the question is opened as to just what the relevant and appropriate criteria of adequacy are for them. An extreme view would be that a good representation of that sort is one that corresponds in every respect to what it is representing. That is not what even an extreme scientific realist would say, since there is always a good deal of mathematical artifice present. ... But a different criterion is found in the varieties of structuralism in the philosophy of science that we shall go on to examine — criteria to the effect that scientific models trade for their success on resemblance with respect to structure alone. And there is a still more liberal empiricist conception according to which the base line criterion is just empirical adequacy. (198)

In Poincaré’s *Science and Hypothesis* (1952, xxiv), we encounter the claim that ‘the aim of science is not things themselves, as the dogmatists in their simplicity imagine, but the relations between things; outside those relations there is no reality knowable.’ Duhem (1954, 151) reacts to Poincaré’s claim by asking how mathematical propositions can represent experimental laws (which have terms that apply to physical objects and processes). If we don’t have a simple reference relation, how do we bridge the gap between representation and what is represented? Van Fraassen sees here a gesture toward what Reichenbach would later call ‘the problem of coordination.’ He quotes Weyl (1934, 19): ‘A science can never determine its subject matter except up to isomorphic representation. The idea of isomorphism indicates the self-understood, insurmountable barrier of knowledge. ... [T]oward the “nature” of its objects, science maintains complete indifference.’

What ‘additional knowledge’ is needed to create the appropriate coordination or representational linkage? Weyl himself seems to conclude that scientific representation is not purely mathematical — or (perhaps) that science is necessarily incomplete in a way we can know it to be incomplete. It is the second of these options (necessary incompleteness) that van Fraassen chooses to pursue. So he asks (210): Is there something that I could know to be the case, and which is not expressed by a proposition that could be part of some theory? The attentive reader already knows that the ‘additional knowledge’ in question will be indexical in character.

An entire chapter is devoted to ‘The Longest Journey: Bertrand Russell.’ The climax of the story comes with Russell’s 1927 statement of structuralism in *The Analysis of Matter* and M.H.A. Newman’s apparently devastating objection in his review of the same book. Russell had written that

...whenever we infer from perceptions it is only structure that we can validly infer; and structure is what can be expressed by mathematical logic.... The only legitimate attitude about the physical world would seem to be one of complete agnosticism as regards all but its mathematical properties. (Russell, 1927, 254, 270)

But Newman, in his review (1928, 140), pointed out that 'given any aggregate *A*, a system of relations among its neighbours can be found having any assigned structure compatible with the cardinality of *A*.' In other words, the equality of size between the two sets means that their 1-1 correspondence can be used to pick out a copy in the one set of *any* relational structure displayed in the other. So Russell's 'extreme structuralism' collapsed. Carnap's *Aufbau* also offered a structuralist philosophy of science and ran into essentially the same problems Russell faced, on van Fraassen's analysis. His conclusion is that we cannot solve the problem of coordination, which is at the heart of structuralism without taking into account the role of indexical and ostensive reference (223-4). He describes Hilary Putnam as having put a Newman-style argument to good use when he developed his critique of metaphysical realism, beginning in the mid-1970s. Van Fraassen adds an additional interpretive twist to this story when he suggests that Putnam's argument emphasizes 'the crucial role of the indexical' and the importance of self-location with respect to theory content.

In the final chapter of Part III, we arrive at van Fraassen's account of empiricist structuralism. It is a view *not of what nature is like but of what science is*; the structuralism in 'empiricist structuralism' refers solely to the thesis that all scientific representation is at heart mathematical (238-9). And the slogan that 'all we know is structure' should be interpreted thus: Science represents the empirical phenomena as embeddable in certain abstract structures (i.e., theoretical models), and those abstract structures are describable only up to structural isomorphism.

This characterization is obviously a good fit with constructive empiricism, but it leaves us wondering exactly how van Fraassen thinks indexicals will save the day. He says that to feel 'the proper impact of the move to indexicals, we must revisit Reichenbach's problem of coordination in a more abstract form' (240). For the empiricist structuralist, the problem of coordination takes the form of what may appear to be a serious loss of contact with reality.

Bear in mind that phenomena are not abstract objects; they are (on van Fraassen's present account) observable entities: objects, events, and processes. So we cannot embed the phenomena themselves in theoretical models. Nor can we embed *copies* of phenomena, as some realists might have claimed. Instead our hope is to embed *representations* of phenomena: i.e., surface models that have idealized the data models that have smoothed out the data sets that were themselves the content outcomes of measurement processes that were relational,

intensional, intentional, indexical, selective in their resemblance, and perspectival.

How can we, under these circumstances, establish the appropriate kind of referential relationship between the models in question and the phenomena they are supposed to represent? The data model and surface model are mathematical structures, and of course, the theoretical model is also a mathematical structure; so this is not where van Fraassen's problem of coordination lies. The problem instead lies in establishing the relationship between the data model and the phenomena, between a mathematical structure and the physical objects, entities, or processes that it is said to represent. If he cannot solve *this* problem, empiricist structuralism does in fact succumb to 'loss of reality' and will thus fail to solve the problem of coordination where it really matters.

The short answer to the problem, says van Fraassen, is this: 'construction of a data model is precisely the selective relevant depiction of the phenomena *by the user of the theory* required for the possibility of representation of the phenomenon' (253). No semantic word-thing relation can establish the link to reality; instead we must take into account the role of the user and the context of use. This, by now, will sound familiar. But *exactly how* is it supposed to provide the linkage between two domains as heterogeneous as empirical phenomena and abstract mathematical structures?

Van Fraassen's solution depends upon the establishment of a pragmatic tautology, a statement that is logically contingent but nonetheless undeniable. Moore's Paradox provides the classic analogue to the case at hand: I cannot consistently assert '*p*, but I don't believe *p*.' The inconsistency does not involve a straightforward logical contradiction but seems to depend upon the act of *first-person assertion*. By analogy, a pragmatic tautology would be a sentence the denial of which would be inconsistent, not because of syntactic or semantic considerations, but because of the conditions under which it is used.

To the objection that, on his account, empirical adequacy is not adequacy to the phenomena, but to the phenomena as represented in our mathematical models, van Fraassen replies:

'For us the claims

- (A) that the theory is adequate to the phenomena and the claim
- (B) that it is adequate to the phenomena as represented, i.e., *as represented by us* are indeed the same!

'That (A) and (B) are the same for us is a pragmatic tautology' (259). That they are the same for us restores contact with reality and solves the fundamental problem of coordination.

Everything turns on whether this argument succeeds. I shall maintain later it that it fails. For the present, however, the task is not to evaluate but to try to understand how it is *supposed* to work. Van Fraassen offers this analogy to help us out: When I say, 'The graph represents the phenomena I have been measuring,' it is analogous to saying that a certain point on the map is *where I am*' (257). This is not very helpful because (a) it is not entirely clear how van Fraassen intends the analogy to work and because (b) on the most natural reading, it seems to work against his position. (Although I cannot be mistaken when I say 'I am here' with respect to the world, I can easily be wrong when I say 'I am here' with respect to a map.)

So perhaps it is best to go back to the specific example van Fraassen offers when he sets up his 'pragmatic tautology.' Imagine that I am a researcher who is making a presentation about deer population growth in Princeton. There is a theory *T* about this phenomenon, and I have spent much of my presentation in showing how my data model, the outcome of diligent and responsible measurements, fits *T* rather nicely. Still there is an audience member who persists in saying that 'Yes, I can see that *T* fits your data model quite well. But does *T* fit the actual deer population growth in Princeton?'

In the end, the only sense that van Fraassen can make of this question depends on attributing to the audience member some sort of metaphysical realism. He must be asking what is really the case 'independently of our knowledge and of any procedures followed to gain knowledge' (255). Van Fraassen acknowledges the logical leeway toward which the audience member is pointing but maintains that '*there is no leeway for me in this context*, short of withdrawing my graph altogether. Since this is *my* representation of the deer population growth, there is for me no difference between the question whether *T* fits the graph and the question whether *T* fits the deer population growth (256). Hence the 'pragmatic tautology' and with it van Fraassen's answer to the problem of coordination. It is the indexical that creates the link to reality: not a semantic word-thing relation, but a '3-place (sic) relation of *use of something by someone to represent something as thus or so*' (258).

Part IV. Appearance and Reality

The book's last part focuses on criticism of various completeness criteria that van Fraassen suggests have held sway at different times in the history of science. These criteria reflect a desire for unity, intelligibility, explanation — for something that goes beyond empirical adequacy to *account for* what we observe. Van Fraassen dubs the most recent version the *Appearance from Reality Criterion* and maintains that it is presently accepted quite generally, 'at least among philosophers and by the general public' (280).

The Appearance from Reality Criterion is presented as the successor to three previous completeness criteria, each of which had to be abandoned in the face of scientific progress. Van Fraassen argues that the present version must now be given up in the light of quantum theory. The basic idea behind all four completeness criteria is that 'even if a scientific theory or model represents how things really are, the scientific account is not finished unless it entails what their appearances will be like under realizable conditions' (269). The reader must bear in mind throughout this discussion that the word 'appearances' has been redefined in this volume. It now refers to the contents of our measurement outcomes.

The Aristotelian version of the Criterion held that '*science must explain how things happen, by demonstrating that they must happen in the way they do*' (277). Science, in other words, was required to offer explanations that held *necessarily*. This version, says van Fraassen, gave way at the time of Newton to the weaker requirement that science explain all appearances *deterministically*. Subsequently, in response to the challenges of quantum mechanics, Reichenbach proposed the *Common Cause Principle* as a way of accommodating indeterminism while maintaining 'lawlikeness.' Once it was shown that quantum mechanics violates the Bell inequalities, the common cause principle had to be abandoned. What has replaced it, according to van Fraassen, is the Appearances from Reality Criterion, which requires physical science to demonstrate 'that, and how, these appearances are *produced as a proper part of the depicted reality*' (281). Although this requirement does not preclude indeterministic or stochastic accounts, van Fraassen maintains that, like its three predecessors, the Appearance from Reality Criterion is not satisfied by derivation and prediction. It is instead 'a demand for explanation which is satisfiable only by connections deeper than brute or factual reality... a connection of the order of explanation through necessary and/or causal mechanisms to be displayed which *produce* the appearances' (283).

Van Fraassen maintains that the Copenhagen development of quantum theory exemplifies a clear rejection of the Criterion and that, as a result, we are once again in a position in which science forces us to surrender the reigning completeness criterion. He argues that quantum theory is unqualifiedly adequate if the surface models of phenomena generated by measurement outcomes fit the theoretically specified Born probabilities. Nothing more is required. This is based on the *prima facie* shocking claim that 'to say that the theory must save the phenomena is *not the same* as saying that the theory must be in accord with the experimental and observational results' (289). Empirical adequacy does not entail '*that what happens in the actual situation must be displayed as entirely identifiable in the theoretical model*' (305). Saving the phenomena is now a matter of saving surface models.

This is the point where, if it was not obvious before, it becomes clear that van Fraassen's new definition of 'appearances' was motivated largely by the need to accommodate the quantum situation. More specifically, his earlier point that measurement outcomes show only what the target looks like in a particular measurement set-up rather than what it looks like *simpliciter*, although intended to be general in scope, was shaped largely by the need to accommodate the quantum measurement problem. In classical physics, it is possible to vary and combine simple measurements into complex joint measurements so that, in practice, we can largely overcome the limitations of a single perspective by looking at the target phenomenon in different ways to gain a fuller and fuller (though not complete) picture of what it looks like (312). This, however, is not possible at the quantum level.

Even if the measurement problem is eliminated by future developments, 'our view of science must be forever modified in the light of this historical episode' (291). Why? Because there has been an episode of genuine scientific progress in which neither determinism nor the Appearance from Reality Criterion was satisfied. 'Since these criteria were violated in actual practice it is possible — and in fact incumbent upon us — to conceive of science as not so constrained' (292). We cannot legitimately require science to go beyond empirical adequacy. We cannot require explanation, and we cannot require any structure other than the mathematical structures provided by our theoretical models.

There are six short appendices that follow the main text. These are important, and I have drawn on them to varying degrees in the summary above.

II Assessment

Scientific Representation is a brilliant book in many ways. It goes directly to the heart of the challenges that empiricist structuralism must overcome, and in assessing those challenges, it greatly enhances our understanding of the role of measurement in science — especially measurement's intimate relationship with theory construction and theory use. The book introduces further refinements of van Fraassen's anti-realist approach to understanding science, and it reflects his characteristic effort to take into account the valuable insights of alternative approaches to understanding science. (Indeed there are passages in which the casual reader may think the author is channelling Donna Haraway.)

In light of this, it likely seems foolish (and perhaps churlish) to focus on what I believe to be the fundamental failure of the book's central arguments. Nonetheless that is what I propose to do here. I shall also try to show that at the heart of many of the book's difficulties lie the

same problems that have plagued constructive empiricism from the beginning.

Finally, and rather more tendentiously, I shall try to explain these failures by suggesting that, despite the frequent invocations of (roughly) Kantian and pragmatic insights, van Fraassen's philosophy of science has not fully assimilated the implications of pragmatism or (more fundamentally) Kant's Copernican Revolution. I begin, however, with a very specific problem, the failure of the indexical argument (in its 'pragmatic tautology' form) and then try to situate this failure within larger contexts.

Failure of the Indexical Argument

Recall that van Fraassen points toward Moore's Paradox to set up his indexical argument. Just as I cannot consistently assert '*p*, but I don't believe *p*,' so also I cannot consistently assert, 'The theory is adequate to the phenomena *as we represent them*, but it is not adequate to the phenomena.'

A point to be noted in passing is that although van Fraassen couched the Princeton deer population example in the first-person singular, he then switched to the first-person plural in setting up the more general argument. This is a move that deserves more scrutiny than it can receive here. On the one hand, the switch to the plural pronoun helps to underscore the point that private sense data are not the issue here. On the other hand, the change to the plural may lend to the claim a quasi-Kantian (or quasi-Putnamian) flavour that is very misleading — as if allowing the possibility of a gap between our representation of the phenomena at any given time *t* and the phenomena themselves were the same as allowing the possibility of a gap between phenomena and a Reality that is eternally and in-principle inaccessible. This is perhaps too bizarre to mention, but it would offer one explanation of van Fraassen's claim that the fellow in the audience who keeps trying to distinguish between the data model and the deer population must be a metaphysical realist inquiring about noumenal deer.

Leaving that aside for now, it may seem that the very points that van Fraassen stresses about the nature of perspectival representation will also undermine any analogy with Moore's Paradox. Sentences and beliefs are true or false, but pictures are assessed differently. They are perspectival and are judged with respect to the specific sorts of resemblance that are relevant to the purpose for which they were created. More specifically, appearances do not tell us what phenomena are like, only what they *look like* in certain measurement set-ups. Phenomena are objects, events, and processes in the world. They can be represented in diverse ways, and it is not generally the case that a single set of

representations can capture all aspects of the phenomena in question. A theory that is able to accommodate the surface model of all the appearances that went into the construction and maintenance of that surface model may nonetheless be unable to accommodate other appearances of the relevant phenomena. This may suggest that van Fraassen's analysis makes it only too easy to assert without inconsistency that 'the theory is adequate to the phenomena as represented by us but not to the phenomena themselves.' Future measurement contents may not fit the present theoretical model, and in any event, we are pretty confident that science will eventually offer alternative representations of the phenomena that are much better than our own, which will then be discarded as inadequate. This response is, however, a little too fast, since we must take into account van Fraassen's technical definition of 'empirical adequacy.'

Recall that in the wake of the new distinction between 'phenomena' and 'appearances,' empirical adequacy continues to be defined in terms of adequacy to the phenomena, *not* the appearances. (Indeed we saw above that to say the theory must save the phenomena *is not the same* as saying that the theory must be in accord with the experimental and observational results.) Note, however, that adequacy to the phenomena just *means* that the mathematical model used to represent the phenomena can be isomorphically embedded in a model of the theory. This makes it *true by definition* that the theory *T* is empirically adequate (saves the phenomena) if and only if the *representation* of those phenomena fits the theory. Van Fraassen is correct, therefore, to say that (A) the claim that the theory is adequate to the phenomena and (B) the claim that the theory is adequate to the phenomena *as represented by us* are the same. But they are the same in virtue of his newly refined definition of empirical adequacy, and there is no need to invoke the notion of pragmatic tautology or the associated focus on first-person assertion. More importantly, the definitional equivalence of the two claims does nothing to establish a link with phenomenal reality. Instead it makes clear how tiny is the argumentative circle that begins and ends with van Fraassen's austere, commitment-minimizing definition of the aim of science. The existentialist commitment to facing bravely the need for decisions is coupled here, as elsewhere in van Fraassen's technical writings, with a philosophical strategy of playing it safe by minimizing epistemic commitment.

The Misleading Map Metaphor

As we have already seen, van Fraassen's preoccupation with indexicals is closely tied up with the 'you are here' marker associated with some maps, and more generally, with use of the map metaphor to describe

scientific theories. Maps are particularly apt analogues for scientific theories insofar as they combine the functions of *representation* and *usefulness*. In both cases, the intended use (of the map or the theory) plays a large role in determining what will be represented, how it will be represented, and what will be left out altogether. I shall argue (A) that most uses of maps do not involve self-location and that this is also true of scientific theories, and (B) that the element that coordinates mathematical structures with phenomena must be part of the scientific theory, *not* something added from outside. In this light, we can see the specific failure of the ‘pragmatic tautology’ argument in relation to underlying difficulties in van Fraassen’s use of the map metaphor. Recall his earlier claim: When I say, ‘The graph represents the phenomena I have been measuring,’ it is analogous to saying that a certain point on the map is *where I am*’ (257). This is problematic *not only* because it fails to support the alleged pragmatic tautology but also because it misconstrues the scientist’s role in the pragmatic account of representation.

For many uses of ordinary maps, self-location is not an issue. I do not need to locate myself in order to use maps to find out the distance from Houston to London, the elevation of Fayetteville, or the place of the sun in the Milky Way. To do this, I must be able to locate (representations of) *those places* on the map, not myself. Maps make this possible by *including*, for instance, the names of various features as well as a key or an index that allows the user to interpret other features of the map. Without such (implicit or explicit) interpretive features, a ‘map’ is not a map at all; it is, for example, nothing but marks and colours on a piece of paper. As with maps, typical uses of scientific models do not involve finding *my* place in the theory, but rather the places of phenomena as represented by some of their appearances.

This is not to deny that there are some uses of maps that do require a kind of self-location. If I am trying to use a map for the purpose of finding my own way in unfamiliar territory, I will need to find the point on the map that represents my location in the relevant physical environment. There would seem to be at least two different cases in which I might need to locate myself with respect to a map. In one case, the map is *fixed* at a location within its own domain. So, for example, if I am in a large and unfamiliar mall, I will look for the big map. It may say ‘you are here,’ or if it is fixed to a location in front of the multiplex cinema (for instance), it may simply highlight that location on the map itself. Under either description, the map *locates itself* within its own domain. The second case is more common. The map is not permanently fixed to a location in its own domain; perhaps instead it is in the glove compartment of my car. I need it to figure out how to get to my destination. In this case, there can be no marker on the map itself that tells me where I am. Instead I must use the map, *including* the interpretive devices that

are part of it, coupled with what I know about my location in the real world, to find the representation of that place on the map.

It is not obvious that the first case, in which the map is fixed to a location within the domain it represents, has analogues in sciences that rely upon mathematical structures for representation. What would fixity of location amount to? What would it mean to permanently fix a mathematical structure within the physical domain it represents? The classic 'you are here' seems to be irrelevant for the task at hand. But what about the 'I am here' case? Are there instances in the physical sciences in which I must locate *myself* (qua scientist) in the theory's mathematical structures? It is hard to know what that could mean; it seems like a category mistake. On the pragmatic account of representation, Z uses X to depict Y as F . *Qua* scientist Z , I use model X to represent phenomenon Y as it appears under certain conditions. But that is not *self*-location at all; it is the location of mathematical representations within other mathematical representations. (The passage van Fraassen quotes from Kant regarding the determination of direction involves self-location in *nature*, not in science.)

But what if I am Frank Jackson's Mary, the colour scientist raised in a totally monochrome environment? Mary knows everything there is to know about the mathematical structures of colour theory but is unable to coordinate these structures with colours as other people experience and describe them. In David Lodge's novel *Thinks* (Lodge, 2001), Mary is finally allowed out of her room and sees coloured objects in daylight, an experience that allows her to say, in van Fraassen's parlance, 'this, in front of me, is *there* in the colour space,' thus effecting the requisite coordination.

There are problems with this story and (especially) van Fraassen's use of it. I shall mention just two: (1) Focusing on Mary and her personal 'color experience' is an unfortunate distraction, since it suggests that perhaps private sense data are what effect the requisite coordinations between mathematical structures and phenomena. (2) Van Fraassen moves beyond mere distraction into red herring territory when he presents the moral he takes from Lodge's story about Mary: 'This [way of coordinating experience with mathematical structures to cut through the problem of isomorphism] sounds like a way to success, but note how crucially it relies on the indexical: what Mary must be able to say at this point is "these are my fingernails; my nails are pink"' (212). The first-person personal pronouns are simply irrelevant. Before Mary saw the pearly pinkness of her fingernails, she almost certainly saw the dull brownishness of the pigskin gloves she had been required to wear until that point (and had heard described many times). If she were described as saying 'the pigskin gloves are dull brown,' the novel's narrative quality would surely suffer; the poignancy associated with delicate colours

and self-discovery would be sacrificed. But the account of coordination would be just the same; the first-person pronoun in ‘my fingernails’ does no special work. Self-location does not break the limits of isomorphism, and as long as we insist that science (in its official formulation) is a purely formal (aperspectival) representation, there is no solution to the ‘loss of reality’ problem — regardless of whether or not we accept van Fraassen’s definition of empirical adequacy.

A set of mathematical structures is not a scientific theory; it’s just math. A set of models says nothing about the phenomena unless it comes with information about the theory’s target and about how the mathematical structures are to be coordinated with phenomena. The bridge connecting the theory’s models to the physical world must use the language that scientists employ to describe the phenomena and measurements in question. This is not the ‘observation language’ of the logical empiricists but the community’s theory-laden language in use: the language its members employ to describe events, entities, and processes — including measurement events.

Van Fraassen offers quite lovely descriptions of the historical process through which these coordinations are worked out, the mutually entangled evolution of theory and measurement discussed above. The problem with his account is that this process is visible only in the view ‘from within’ and seems to leave no trace in the product itself. Science is defined in terms of abstract structures, and the aim of science is defined in terms of the embeddability of surface models in theoretical models. The reference relations that were established through the historical process are no part of the product that is science. The parameters are no longer defined by measurement but by their role in the stabilized theory. It is entirely sensible to say that a theory does not contain its own history; it is much more problematic to say that the referential relationships that were developed through that historical process are no proper part of the scientific theory ‘in its official formulation.’ It is problematic because it is those relationships, not self-location, that allow us to maintain the theory’s connection with reality.

Vision Problems

Van Fraassen, as we have already seen, advocates a ‘synoptic vision’ that combines the view ‘from above’ with the view ‘from within.’ It is difficult to know what this could mean or how it could address the problem just discussed. In what sense would van Fraassen’s vision be synoptic? Not, it seems, in the sense of the Synoptic Gospels. These include the gospels of Matthew, Mark, and Luke, which are said to adopt the *same point of view* in developing their narratives. John’s gospel is excluded because it adopts quite a different perspective.

So perhaps van Fraassen means 'synoptic' in the sense of 'summary.' This doesn't seem quite right either because nothing is being left out. Instead we are asked to *combine* the two views. So perhaps what is intended is an 'overview.' But what would count as an overview of the view 'from within' and the view 'from above'? Van Fraassen says that there is no god-trick involved, but it is hard to know how else to describe such a meta-position, especially since the view 'from above' is aperspectival and therefore already bears an uncanny resemblance to the view from nowhere.

The issues here are not simply or primarily linguistic. A parameter (or at the very least, its concept) was constituted in the historical process, but now it is defined only by its role in the theory. At the same time, the theory is said to draw on that history for its credentials. This is a strange sort of synopsis indeed! What is this 'drawing on' relationship between a theory and its own history? It suggests that while the established theory and its models are to be regarded as aperspectival representations, the theory nonetheless depends for its credentials on a historical process driven by pragmatic considerations and shot through with perspectivity. What can justify a definition of science and its aim in terms that make everything that connects science to the world disappear in the 'official formulation'? Is this perhaps a legitimate case of using a ladder to ascend to a solid resting place, then kicking it away because it is no longer needed at the summit? It appears not — because without the ladder we have no continuing connection to the phenomena. We need the pragmatic dimensions of science not only for purposes of creation and application but also to give the theory-as-product the meanings that will allow us to judge empirical adequacy in any sense that is truly empirical.

Van Fraassen, by insisting with Neurath that we must always start where we are (both linguistically and theoretically), offered an admirable account of how coordination is established within a pre-existing context. But following through consistently on this approach would have forced him to concede that there is more to science than math, a concession he is clearly unwilling to make. To re-establish the 'link with reality,' he turned to the indexical argument. Even assuming the success of that argument, however, he still had to draw upon the view 'from within' to establish the theory's empirical credentials and to test its empirical adequacy. What accounts for all these epicycles?

The Unfinished Copernican Revolution

I shall suggest that despite the frequent invocations of (roughly) Kantian and pragmatic insights, van Fraassen's philosophy of science has not fully assimilated the implications of pragmatism or (more fundamentally) Kant's Copernican Revolution.

One of the most striking 'disconnects' in van Fraassen's work is that even as he devotes increased attention to the pragmatic dimensions of science, he continues to insist that epistemic and pragmatic issues must be kept entirely separate. He does not, of course, deny that scientists make choices based on explanatory power, consilience, simplicity, and a variety of other value-laden and purpose-driven considerations. He does, however, insist that there is only one aim of science and one criterion against which to judge its success: empirical adequacy (defined now in terms of mathematical structures). Pragmatic considerations, as noted earlier, count as 'reasons' for theory choice only in the sense of ulterior motives. To insist on this separation between truth and belief, on the one hand, and human cognitive practices, on the other, is to deny one of the defining characteristics of pragmatism.

The pragmatist denies that the use-oriented, problem-solving, explanation-seeking, purpose-driven, value-laden, down-and-dirty sphere of pragmatism co-exists with a Separate Sphere of Pure Truth. Truth, for the pragmatist, cannot be defined independently of human epistemic activity and capacity. Both Kantian and pragmatic conceptions surrender the idea that truth is defined in terms of a uniquely correct representation of a Reality that exists independently of all human epistemic activity, a sort of pre-defined Target at which we aim. Metaphysical realist conceptions open up the possibility of an unbridgeable gap between our best shots and the Target. The gap, in fact, is so big that (for all we know) we could be aiming in the wrong direction; we could be mistaken about everything. Giving up metaphysical realism allows us to banish this sort of skepticism and (I would suggest) makes 'agnosticism' a less attractive option. Giving up metaphysical realism frees us to embrace more modest forms of realism about truth, unobservable entities, and perhaps even some 'modalities.'

Van Fraassen's commitment to mathematical representation replicates many of the problems that plagued logical empiricism with respect to the treatment of theoretical entities, modalities and coordination. The earlier program foundered partly because the formalism to which it was so tightly bound could not accommodate disposition terms or distinguish among various types of regularity, for example. Although van Fraassen's approach is in the 'semantic' rather than 'syntactic' tradition, his commitment to formal (in his case, mathematical) representation creates similar problems. Aspects of the scientific process that cannot be represented mathematically are absent from the product that is science itself.

A long time ago (Okruhlik, 1986), I argued that the conception of truth van Fraassen employed in *The Scientific Image* was pre-Kantian and that he was mistaken to treat 'empirical adequacy' as an unproblematic baseline for his conception of science. On a Kantian account,

there is no empirical criterion of truth apart from the projected unity of nature. It is the search for systematicity that defines scientific activity and underwrites the notion of empirical truth. There can be no definition of empirical truth anterior to or independent of our search for systematic representations of nature.

It is not clear to what extent such a criticism remains relevant. It may still tell against the view of language that informs van Fraassen's 'common sense realism.' If, however, empiricist structuralism has in fact lost its grip on reality, then the 'truth' involved in the definition of empirical adequacy is mathematical truth, not empirical truth. This avoids the Kantian objection regarding empirical truth, but only by replacing empirical with mathematical truth.

This is surely not what van Fraassen intends. His increased attention to historical and pragmatic concerns in recent years provides evidence to the contrary, but his commitment to separating the mathematical product that he identifies with science from the pragmatic and historical processes that generate meaning leads inexorably to this result. Purity comes at the price of empirical meaning.

This approach has social implications as well, although these are clearly not intended either. The idea that all the choices, perspectives, purposes, and values that go into the making of science are absent from the finished product has been the source of 'scientific' justifications of social difference, for instance. (As a thought experiment, rethink van Fraassen's indexical example, substituting chimpanzees or bonobos for the Princeton deer population.) Our discipline's love affair with purely formal representations of empirical science has too often been employed to discredit social critics. Those who criticize accounts that represent science as transcending the historical process through which it was created are too often dismissed as insufficiently philosophical, as unable to grasp the genetic fallacy. Technical philosophy of science has an important role to play, but only if it does not define the scientific product in terms of aperspectival representation.

None of this should be interpreted as a defense of structural realism against empiricist structuralism. The word 'realism' in this context leads to the same sorts of confusion it has engendered in debates about 'scientific realism.' Some advocates of structural realism are diehard metaphysical realists, committed to the belief that there is exactly one way to 'carve nature at the joints.' Others deny claims of this sort while maintaining that if we look at science, we will see that what survives, even across revolutionary upheavals, are the parts that represent *structure*. Although these philosophers may not speak of 'carving nature at the joints,' some of them do seem to maintain an unhealthy fascination with bones. A theory's ontological, interpretive, and explanatory commitments are soft flesh that rots away while the structure-defining bones endure.

I believe that van Fraassen is right when he suggests elsewhere (in van Fraassen, 2006, for example) that the structural realist's identification of 'structure' must be *post hoc* and revisionist. If something endures, it is identified as structure. If it rots away, it must not have been structure after all (however bone-hard it may have seemed at an earlier point in the history of science). Structural realists share some of van Fraassen's problems, but not others; they also have problems of their own.

Scientific Representation is a difficult and sometimes frustrating book, but one that should be widely read. It is frustrating because it addresses the problem of coordination head-on and lays the groundwork for a historical/pragmatic solution to the problem. Then, however, it backs away on account of the prior and deeper commitment to a particular *philosophical* perspective: one that embraces voluntarism at the meta-level but sides with Clifford rather than James in minimizing the epistemic risk required in scientific theory acceptance; a perspective that rejects central tenets of pragmatism while drawing freely on its other resources; an empiricism that represents scientific theories (in their 'official formulations') as purely mathematical structures.

Received: July 2009

KATHLEEN OKRUHLIK
The University of Western Ontario
London, ON N6A 3K7
CANADA

Acknowledgements

I am grateful to Lucien Lamoureux and Jim Brown for useful discussions, to Bill Demopoulos for pointing out an infelicity, and to an anonymous referee for two good questions.

References

- Duhem, Pierre. 1954. *The Aim and Structure of Physical Theory*. Princeton, NJ: Princeton University Press.
- Lodge, David. 2001. *Thinks*. New York: Viking.
- Monton, Bradley, ed. 2007. *Images of Empiricism. Essays on Science and Stances, with a Reply from Bas C. van Fraassen*. Oxford: Oxford University Press.
- Newman, M.H.A. 1928. 'Mr. Russell's causal theory of perception,' *Mind* 37: 137-48.

- Okruhlik, Kathleen. 1986. 'Kant on Realism and Methodology.' In *Kant's Philosophy of Physical Science*, Robert E. Butts, ed. Boston: D. Reidel. 307-29.
- Poincaré, Henri. 1952. *Science and Hypothesis*. New York: Dover.
- Russell, Bertrand. 1927. *The Analysis of Matter*. London: Allen & Unwin.
- Van Fraassen, Bas C. 1980. *The Scientific Image*. Oxford: Oxford University Press.
- _____. 1989. *Laws and Symmetry*. Oxford: Oxford University Press.
- _____. 2002. *The Empirical Stance*. New Haven: Yale University Press.
- _____. 2006. 'Structure: its substance and shadow.' *British Journal for the Philosophy of Science* 57: 275-307.
- Weyl, Hermann. 1934. *Mind and Nature*. Philadelphia: University of Pennsylvania Press