Preface to the Greek edition

It is both an honor and a pleasure for me to add some introductory remarks to this book, on the occasion of its publication in the Greek language. These remarks are introductory in one sense, in that they can properly preface a new edition. But in another sense they are not: they should in fact be read after the book. For they are written with the hindsight of two decades of dialogue.

In seminar discussions, after lectures, and in symposia there are three questions that I am most frequently asked about The Scientific Image. The first is whether I have changed my mind, or amended my position of 'constructive empiricism' on what science is. I answer that I have changed my mind on several points, but so as to strengthen rather than take away from its central argument. The second is how I respond to various interpretations of that argument, that have been offered in articles and reviews, which would convict me of outrageous violations of common sense, not to mention charity. Finally, the third question, never lacking, is how I can maintain a principled distinction between what is observable and the rest of nature, in the light of contemporary advances in technique and instrumentation. I will respond to all three of these questions here, but in reverse order.

On the topic of observability and its role in the philosophy of science much has been written over the past two decades. Many of the arguments and my responses appeared already in Images of Science (edited by Paul Churchland and Clifford Hooker in 1985). But new challenges appeared in a quite recent symposium, The Scientific Image Twenty Years After, with contributions by Arthur Fine, Paul Teller, and myself (Philosophical Studies volume106, 2001). For now I will just briefly indicate the two main challenges so far that pertain to observability.

To explain my view of what science is, and specifically what is its aim, I need a feasible distinction between what is observable and what is not. That distinction is most definitely related to what we are like. If we were differently constituted, then different things would be observable. So the distinction is anthropocentric (perhaps even anthropomorphic) since this "we" encompasses, for now at least, only human beings. But the role this notion plays pertains to the aim of one of our own enterprises (the enterprise of science) and to the questions we face about what attitudes to adopt toward products of this enterprise of ours (scientific theories). Therefore it is precisely right that the distinction should not be in absolute terms but in terms relating to us.

Moreover it is a factual distinction, in fact one of empirical fact. The limitations to our sense organs and whatever else is involved in the physical and physiological conditions for observation are all within the scope of empirical scientific investigation. In response to this point some authors have intimated that there must be a circularity in this account of what science is. The account assumes the intelligibility of certain assertions, concerning what we can and cannot observe, which are the proper deliverances of science as opposed to for example armchair reflection.

But there is no circularity. There are logical niceties to observe in the spelling out which I shall neglect here (they can be found in my responses in Images of Science), but we can picture the situation roughly as follows. We should understand a scientist who puts forward the scientific image of our world as saying in effect: here is what all there is, and here is how it is divided into what is observable and what is not observable. Then we can distinguish the two distinct positive epistemic attitudes: belief that the account he offers is entirely true, or acceptance only, an attitude which involves only the more modest belief that what this account says about the observable part is true.

When we think about the limitations to what we can observe, I should add that we ought not to think of unobservable as simply a matter of being too small. Cosmology, which describes the universe as a whole, describes it as having parts that do not fit into the absolute past cone of any space-time point, and are therefore unobservable for that reason. These are literally too large to be observed, even in principle, by any being at all like us, no matter how superior. Notice, by the way, the pattern of argument here: I do not assume that such a cosmology is true, but the scientific realist has to acknowledge that if contemporary science is true then there are such cosmic structures.

The other challenge concerning observability brings in the extent to which our discourse is theory-laden. Aren't the scientists' observation reports these days often couched in language that no previous generation could have understood? Certainly! Pierre Duhem already remarked on this in his own time, a century ago. But there is no simple relation between what is observable and what is stated in an observation report. Let's take a very simple, everyday example of this. We can easily see radios. Yet the assertion that something is a radio is laden with implications which can only be understood within recent physics. This very example brings out the crucial point that observability is not theory-relative: everybody can see those radios. Only the concepts we use, and the terms that we have to describe the things we see, are theory-laden. Whether someone can see those things, touch them, or taste them, is independent of that. It is the same for those of us who have not mastered the relevant concepts or discourse, no less than for those who have.

The entire topic of the observable/unobservable distinction has tended to be confused when put in terms of statements that report on what is observed. Let's not ignore that of course such statements are proper and spontaneous responses to acts of observation, and that they play a role in scientific inquiry. The typical 'observation statement', if you would like an example, is something like "Lo, phlogiston escaping!" That is what someone would say in the presence of fire, if she had been educated in the chemistry of a certain era, with the phlogiston theory of combustion. If Lavoisier, who refuted that theory, had heard someone screaming "Phlogiston escaping!" he would have been right to say to himself "Definitely a false statement!", but also well advised to leave the premises. To focus our questions, consider the following statements:

(1) X's utterance of "Phlogiston escaping!" was an observation report

(2) X observed that phlogiston was escaping

(3) X observed a phlogiston escape

None of these can be equated with any of the others. For example, an observation report is only symptomatic of its own truth, and symptoms don't guarantee more than high probability. Therefore, if the observation report was issued, it does not follow that the reported phenomenon was indeed observed. If (1) is true then X will think that (2) and (3) are true too, and she will think that Lavoisier observed phlogiston escapes as well, even though he did not think so -- but we know better.

There is good reason to keep this terminological break. We want to cite the observation report as <u>evidence</u> that a certain phenomenon occurred. That means that we need to be quite confident that there were such observation reports, before we have established our conclusion that there were any such phenomena.

But is there some sort of theory-dependence in any or all of them? Statement (2) cannot be true unless X could on that occasion give us the observation report "Phlogiston escaping!" This requires that it seems to him that phlogiston is escaping, which requires a belief in phlogiston, and even more, the concept of phlogiston. But (3) is quite different. You and I do not believe in phlogiston. But we can suppose for a moment that the phlogiston theory is true. On that supposition, all those fires we see are phlogiston escapes, and therefore, still on that supposition, we see phlogiston escapes. What this supposition says, what it implies about how the world is, does not at all entail, however, that we believe in the phlogiston theory, or even that we have the concept of phlogiston. Therefore, whether or not (3) is true is quite independent of what theories or concepts X has.

Let me restate this crucial argument in another way as well. From my present, post-Lavoisier point of view, the following three premises could all be true together: the <u>first premise</u> is the phlogiston theory, the <u>second</u> is that people see fires, and the <u>third</u> is that no one has heard of or understands the phlogiston theory. Only the second premise is really true, as you and I know, but the conclusion I want to draw from this example will follow simply from the fact that they are "jointly satisfiable". That is, that they could all be true together, there is no contradiction in imaging a world in which they are true. Now, in a world in which all three premises are true, there are people who have no concept of phlogiston but are observing phlogiston escapes. Since the three premises are jointly satisfiable, it follows that what is observable is not theory-relative or theory-dependent in any important sense.

So the demarcation of what is observable and unobservable concerns not (2) but (3). The question of the conditions under which such statements as (3) are true is not a philosophical question but an empirical one. If anyone wants to frame opinions about just what is observable, I would urge him to draw on physiology and psychology, and empirical science in general, and not to ask philosophers at all.

Of course, we should expect then that our opinions about what is observable will change as science changes. But that does not mean that what is observable changes too. A critical paper by Laudan and Leplin (Journal of Philosophy 88 (1991), 449-472) fell into exactly that confusion. They point out quite correctly that what we regard as observable is not constant across the history of science. But then they conclude that the line between what is and is not observable has shifted right along with it. Well, our opinion of the amount of water present on Mars is also not constant across the history of science. Yet the mass of water on Mars has not been shifting along with this shift in opinion. Our judgements of empirical adequacy of theories will of course vary. But whether those theories are empirically adequate -- just like whether or not they are true -- is a characteristic which they do not lose when we begin to think differently.

Let's leave the challenges pertaining to observability now, and go on to the second point, namely the impression that this book advocates views which outrage common sense. There are certainly some obscure passages in The Scientific Image, and places where I may have been carried away by my enthusiasm. (I am not even counting here the last chapter; I am still asked from time to time whether that is meant to be taken literally!) So if the book's intent has sometimes been misinterpreted that is entirely my fault for not being clear enough.

One way in which it has sometimes been understood is as maintaining that we ought to believe only that our science is empirically adequate and to disbelieve that unobservable entities exist. But how could it be, in view of the permissive view of rationality to which I subscribe? Rationality is but bridled irrationality: rational is whatever stays within the bounds of rationality, and those bounds are very wide. So while I argue strongly that there are no rationally compelling arguments for the reality of any unobservable part of nature, no one is convicted of irrationality for believing in them. On the constructive empiricist view of science, however, such belief, though not irrational, is at best an addition to what science tells us about the world we live in.

Constructive empiricism is not an epistemology, in the sense of a philosophical view of what are knowledge, belief, and opinion (or the criteria of rationality pertaining to them). It is a view of what science is, namely that (a) science is an enterprise in which the bottom line criterion of success is empirical adequacy, and (b) accepting a scientific theory involves the belief that it fulfils this criterion of success, but also has a pragmatic dimension (commitment to approach phenomena in the conceptual framework of that theory). Here is what does follow that on this view: to accept a theory which postulates something unobservable does not need to involve belief that the whole theory is true. But it does not follow that it is irrational to believe the whole theory! Just that such a belief is supererogatory as far as science is concerned.

Readers of the book have also sometimes taken it to involve or subscribe to two arguments against scientific realism which have been salient in the literature. These are the so-called “pessimistic meta-induction” and the argument “from underdetermination to unbelief”. But these arguments do not occur in The Scientific Image. Nor do I subscribe to them; but that would be more obvious from my later writings. The pessimistic meta-induction points to the way in which past scientific theories, though quite successful, have always given way eventually to rival theories which did better. Its conclusion is then that no theory we have, except for down to earth empirical knowledge, is likely to be true at all. Since I do not believe in Induction (as philosophers conceive of it), I certainly do not subscribe to this particular induction. The second argument points to the extent to which all the data we have or could have are insufficient to establish the truth of any one scientific theory, and are therefore compatible with many (perhaps never constructed) rival theories. The conclusion is then that we should not believe in any extant such theory. Where does the “should” come from? Apparently from certain traditional epistemological principles which William James famously refuted, to mention only one example.

We may draw the distinction here, adapting Peter Forrest's apt term "scientific agnostic" ("Why Most of Us Should be Scientific Realists," The Monist 77 (1994): 47-70). A scientific agnostic is someone who believes that the science s/he accepts is empirically adequate but does not believe it to be true. So a scientific gnostic is similarly someone who believes that the science s/he accepts does not only save the phenomena, but is true all told. Thhis is a very different distinction from that between constructive empiricist and scientific realist. The latter are two types of philosophers, who have differing views of what science is. Scientific gnostics and agnostics need not be philosophers at all. The scientific gnostics' beliefs are always changing, as science changes, but the scientific realist's view of what science is stays the same throughout these changes. The two types of philosopher have corollary views about scientific gnostics and agnostics, to be sure. The scientific realist thinks that the scientific gnostic truly understands the character of the scientific enterprise, and that the scientific agnostic does not. The constructive empiricist thinks that the scientific gnostic may or may not understand the scientific enterprise, but that s/he adopts beliefs going beyond what science itself involves or requires for its pursuit. As Forrest also pointed out in this connection, there is no disagreement about rationality involved here; it is not part of constructive empiricism to say that the adoption of such additional beliefs is irrational -- just that it is more than what is involved in scientific theory acceptance.

But this is still a good deal more than most philosophers today seem willing to grant; and in this they take themselves to follow the opinions of the scientifically literate public. Among philosophically literate scientists, on the other hand, we see no such consensus favoring scientific realism, if we look back over the past century and a half or so, in which contemporary science took its form. The background views that do favor scientific realism over empiricism belong rather to our heritage of traditional epistemology, which all philosophers -- myself include -- carry with them initially to any subject they broach. So in the time since The Scientific Image I have concentrated largely on the revision of that traditional epistemology which I see as no longer adequate for the understanding of contemporary culture and science.

This brings me then to the last of the three perennial questions: how have I changed my mind, what is there in the book that I have found it necessary to revise or reject? Only the fourth chapter explicitly addressed issues in epistemology proper. But the point of the book would be lost if the force of reason and evidence compelled us to be scientific gnostics. So the success of my argument requires that is not so. Accordingly a good deal of my effort since then has been devoted to developing specifically empiricist alternatives concerning rational belief and opinion. Large parts of my later Laws and Symmetry and The Empirical Stance are devoted to this. In this effort I have been guided by a change of mind on one central subject: probability. Chapter Six of The Scientific Image, is on that subject but mistakenly (as I now see it) conflates two questions. The first is what exactly an indeterministic theory says about what the world is like, and the second is what it means to accept such a theory as empirically adequate. With respect to the first, I still consider that chapter illuminating, through its modal frequency reconstruction of probabilistic models of natural processes. An example would be the law of radio-active decay. There should be a logical and conceptual connection between the probability of 1/2 that a given radium atom will decay in approximately 1600 years and the relative frequency with which such decay events happen. (More empirically: between that theoretical probability and the proportion of given radium samples that decay over various periods of time.)

But in its answer to the second question the book failed. The acceptance of such a theory does not involve non-trivial beliefs about what the actual frequencies will be in fact. Needed here is a more nuanced concept of opinion, which does not stop with the simplistic trichotomy of belief, disbelief, and "I have no opinion at all". The reason for this failure therefore, as I now see this, is that I had not yet accepted the idea of subjective probability as a representation of opinion. That idea, masterfully promoted by Bruno De Finetti and more recently by my colleague Richard Jeffrey at Princeton (where I moved shortly after The Scientific Image appeared) provided for me the correct answer to that second question. To accept an indeterministic theory which gives us probabilities for natural processes is to appoint this theory as a sort of expert to guide one's subjective expectations for those processes.

Scientific Realists did not, in their disagreement with constructive empiricism, focus on the subject of probability in physics. Their main focal point was, and still is, the idea of Inference to the Best Explanation, which I rejected at so many points in The Scientific Image. So my embracing of subjective probability may seem at first like a technical change on a rather abstruse or at least abstract matter. But probabilism in epistemology changes the shape of every question there. Specifically it allows for a further, additional, detailed critique of that supposed rule to believe the best explanation of our evidence. My website (http://www.princeton.edu/~fraassen/) contains a bibliography of relevant articles and reviews of The Scientific Image, and also contains some of my more recent articles pertaining to these issues.

In closing I wish to express my thanks to my Greek colleagues, and especially Stathis Psillos and Kostas Stergiopoulos. It is very gratifying for me to witness the appearance of this book in Greek, the first language of the philosophical traditions in which we still work today, and I hope of course for many sympathetic Greek readers as well as for illuminating critical response.

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Princeton, December 2004