The Light at the End of the Tunneling: Observations and Underdetermination

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1. Revisiting the distinction between observation and theory

The early positivists sought a way to preserve some kind of criterion for the testing of scientific theories—something for scientific theories to be *about*—while at the same time denying the *meaningfulness* of most if not all metaphysical claims. Hence Carnap (1929) writes

> Quite generally, everything that we talk about must be reducible to what I have experienced. Everything that I can know refers either to my own feelings, representations, thoughts and so forth, or it is to be inferred from my perceptions. Each meaningful assertion, whether it concerns remote objects or complicated scientific concepts, must be *translatable* into a statement that speaks about contents of my own experience and, indeed, at most about my perceptions.

Positivists such as Carnap, then, need a reasonably sharp distinction

[1*] [Thanks to various audiences deleted for review.]

between observational statements (statements about my perceptions) and theoretical statements, which corresponds (after suitable translations) to the distinction between meaningful and meaningless statements. Moreover, given their general (and independently reasonable) allegiance to the meaningfulness of scientific theories, they need some of the deliverances of theories to be (translatable into) observational statements.[2]

The search for a distinction between observation and theory was abandoned or rejected partly because of more or less 'technical' problems (e.g., the difficulty of translating statements containing dispositional terms into observational statements) and partly because of more general philosophical points, especially the so-called 'theory-ladenness of observation'.

It is becoming common to credit Hanson with the 'discovery' of the theory-ladenness of observation, and he does seem to have been the first to use the term. Referring to a drawing of an X-ray tube, he writes (1958, p. 15):

> A trained physicist could see one thing. . . . Would Sir Lawrence Bragg and an Eskimo baby see the same thing when looking at an X-ray tube? Yes, and no. Yes—they are visually aware of the same object. No—the *ways* in which they are visually aware are

[2] I am lumping all positivists together here, but in fact theory-ladenness was a contentious issue amongst the positivists, and indeed I have come nowhere close to stating Carnap's ultimate views on the matter.

> profoundly different. Seeing is not only the having of a visual experience; it is also the way in which the visual experience is had.

Later he concludes (p. 19): “There is a sense, then, in which seeing is a ‘theory-laden’ undertaking. Observation of *x* is shaped by prior knowledge of *x*.”

However, the idea that observation somehow involves theory was around before 1958. One finds, for example, the following passage from Einstein (1949, p. 669):

> What I dislike in this kind of argumentation is the basic positivistic attitude, which from my point of view is untenable, and which seems to me to come to the same thing as Berkeley’s principle, *esse est percipi*. “Being” is always something which is mentally constructed by us, that is, something which we freely posit (in the logical sense). The justification of such constructs does not lie in their derivation from what is given by the senses. . . . One may not merely ask: “Does a definite time instant for the transformation of a single atom exist?” but rather: “Is it, within the framework of our theoretical total construction, reasonable to

> posit the existence of a definite point of time for the transformation of a single atom?”

Einstein had been arguing that quantum theory is incomplete because it fails to specify (even after the fact) a precise time at which a single atom makes a decay. His statement here is in reply to the assertion that no such time exists, because it cannot be empirically determined. Einstein’s reply: what does or does not exist is not determined by what can be observed, but by what is permitted by our best theoretical attempts to make sense of the world.

To be clear, Einstein did not deny a connection between what exists and what can be observed—of course a thing cannot be observed if it does not exist. Rather, his point is that one cannot say what exists on the basis of observation alone. Instead, theory tells us what sorts of thing might exist, and from the existence of those sorts of thing, we can then say what sorts of observation are possible. To put the point succinctly, as Heisenberg did (attributing these words to Einstein), ‘the theory determines what you can observe’.

Einstein’s view agrees with Hanson’s on the general point that observations somehow implicate theories, but the manner in which theories are ‘involved’ in observations is different in their two views. For Hanson, ‘observation’ refers to a personal experience, and the point is that the content of the personal experiences that we would call ‘observations’ is determined in part by whatever theories, or more generally whatever relevant beliefs, we may have, in particular about the physical world. The passage from Hanson is therefore closely related to a passage from Kuhn (1962, p. 111):

> Paradigm changes do cause scientists to see the world of their research-engagement differently. In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world. (*Structure of Scientific Revolutions*, 111)

Kuhn qualifies his claim that ‘after a revolution scientists work in a different world’ (p. 135), and rightly so, for the point most plausibly established by Kuhn’s argument is that the content of their experiences—even in cases where we would take them to be responding to the ‘same external stimulus’—is different. We may add that practically, they live in different worlds, but the point is that their *experiences* are qualitatively different. (Both Hanson and Kuhn are also concerned with how *language* affects how we perceive the world, a concern that is not central here.)

Einstein, on the other hand, is clearly *not* concerned with the experiences of scientists. His point is, rather, that theory determines what it is, and is not, reasonable to assume to exist. Einstein's claim is not about the experiences of scientists, nor about how holding a theory causes one to organize sense data, but instead his claim is about the relationship between theory and physical fact (and therefore, ultimately, observation), independent of whether anybody believes the theory, of whether anybody observes the facts in question, and of the manner in which holding a theory influences how one sees the world.

Of course, the two points are related, but I emphasize the difference between them because, as I shall argue, Einstein's way of putting the point raises a more serious problem for understanding the existence of observationally equivalent theories.

In any case, theory-ladenness raises serious problems for any attempt to make a strong distinction between observation-statements and theory-statements, for observation-statements in either case somehow 'involve' theory-statements. For example, if meaningful statements must be translatable into statements 'at most about my perceptions', then—if Kuhn and Hanson are correct—meaningful statements will necessarily be theoretical, in part.

Van Fraassen has suggested a way around this difficulty, by expressing the distinction between observation and theory without reference to statements. He writes (1992):

> Theories are presented directly by describing their models. . . . A theory provides, among other things, a specification (more or less complete) of the parts of its models that are to be direct images of the structures described in measurement reports. . . . In general, let us call them *empirical substructures.* The structures described in measurement reports we may continue to call *appearances.* A theory is *empirically adequate* exactly if all appearances are isomorphic to empirical substructures in at least one of its models. Theory *T* is *empirically no stronger* than theory *T(* exactly if, for each model *M* of *T*, there is a model *M(* of *T(* such that all empirical substructures of *M* are isomorphic to empirical substructures of *M(*.

Hence we need not allow the existence of theory-independent observation-sentences. Rather, we need only acknowledge the existence of a translation—or isomorphism—from one theory's 'empirical substructure' to another's.

I will suggest that van Fraassen's strategy is more obviously useful in the case of the Hansonian and Kuhnian problem than in the case of the Einsteinian one. However, it will help, first, to make explicit some distinctions that thus far have been implicit. First, we may distinguish between psychologistic and scientific uses of 'observation'. In the first sense, 'observation' refers to the experience of an individual. Although observation in this sense still involves external objects—observations are always observations *of* something—the referent of the word is a personal experience, so that two people can *observe* the same object, and yet have different observations.

In the second sense, 'observation' is an achievement-word, whose referent is an event, the conditions for whose occurrence are given by a scientific theory, if only loosely or indirectly. How, for example, do we know that the usual methods for measuring spin are insufficient in the case of a free electron? Quantum theory *tells* us: in this case, the magnetic field generated by the motion of the charged electron invalidates the result. (Of course, the conditions laid down by the theory are quite general, and do not specify, for example, how to build an apparatus to make the observation. Nonetheless, to settle a dispute about whether an observation of a given phenomenon has in fact been made, one appeals more or less directly to theoretical considerations.)

A second distinction is between the observation itself and the report of that observation, an 'observation-sentence'.

We have, then, four senses of 'observation', and when discussing ‘theory-ladenness of observation’ we should say which sense of ‘observation’ is being used. Let us take them one at a time.

Reports of psychologistic observations are of course given in words, and to the extent that the words used are ‘theory-laden’, the reports are. However, as many have pointed out, such reports can be stripped of their theoretical overtones to a remarkable degree. For example, rather than reporting that she observed the diffraction of the beam to be 5 millimeters, a person can report that she observed the needle (the long skinny object) on top of the ‘5’. Indeed, as Hanson (1958, p. 20) pointed out, when scientists become confused about the theoretical meaning of an observation, they sometimes resort to such descriptions: “it is green in this light; darkened areas mark the broad end.” The point is also implicit in Duhem (1914, p. 218):

> Enter a laboratory; approach the table crowded with an assortment of apparatus, an electric cell, silk-covered copper wires, . . . ; the experimenter is inserting into small openings the metal ends of ebony-headed pins; the iron oscillates, and the mirror attached to it throws a luminous band upon a celluloid scale; the forward-backward motion of this spot enables the physicist to observe the minute oscillations of the iron bar. But ask him what he is doing. Will he answer ‘I am studying the

> oscillations of an iron bar which carries a mirror'? No, he will say that he is measuring the electric resistance of the spools.

This passage again makes clear that we *can* strip observation-reports of theory. The resulting report may continue to have different meanings, and evoke different impressions, for different people—you may think of your father who worked in an iron works every time you here 'iron bar', so that the words in the report remain, in a sense, 'laden', but they are no longer laden by the theory that the reported observation is supposed to test or illustrate.

Psychologistic observations themselves (as opposed to reports of them) are formed by theory too. Indeed, as I have suggested, the arguments of Hanson and Kuhn are best seen as arguments that psychologistic observations are theory-laden. In the end, however, we must recognize two limitations of those arguments. First, they are based on psychological theories that have since been called into question. Second, because one can recognize stripped-down descriptions of one's own psychologistic observations as nonetheless descriptions of that observation, it seems that we *can* translate from one person's psychologistic observations to another's.

Scientific observations present more difficult problems. First, reports of scientific observations are, of course, couched in the language of the theory. There is no obvious way to report that you observed the resistance of a spool without using terms whose meanings are given in large part by the theory of electrical resistance. Second, scientific observations themselves are defined largely in terms of the theory in question, precisely because theory is responsible for determining the conditions under which the observation can be said to have been made.

Let us return to van Fraassen. Because theories, on his view, are sets of models (ways the physical world could be) and not sentences, he need not worry about the difficulties raised by theory-ladenness of observation-reports. Indeed, in his own account of observation-reports (1992), van Fraassen uses the distinction between an observation and an observation-report to argue that even if observation-reports are theory-laden, still two 'incommensurable' observation-reports can report the same observation. (Hence, for example, 'Phlogiston escaping!' remains a report of fire, and we can of course recognize it as such.)

Van Fraassen is right to focus on observations rather than observation-reports. Indeed, our interest in observations is presumably occasioned by an interest in the nature of evidence, and of course sentences are not evidence (usually), but report it. Moreover, although understanding psychological observations may be important for studying the epistemology of observational evidence, scientific observations are at the heart of our concern, for they *are* the evidence for scientific theories. (I am assuming that there is such a thing as ‘objective evidence’ for scientific theories, and I may be wrong, in which case the only sort of evidence we might have consists of psychological observations.)

Hence, for example, the evidence for quantum theory consists in our having been able to confirm many of the observations predicted by the theory. In the next section, I’ll describe one such observation, perhaps clarifying the meaning of the term ‘scientific observation’.

2. Underdetermination

There is a debate about whether rival scientific theories are ever (or ‘very long’) equally supported by the evidence, but there is little debate about whether they are equally supported by the observational evidence, and good thing, because examples abound. For example, there is an alternative (originating with Lorentz) to ‘orthodox’ relativity, which clearly produces the same predictions but which differs on the ‘metaphysical’ question of the existence of a preferred frame of reference. But perhaps the most spectacular example is the alternative to orthodox (non-relativistic) quantum theory due to de Broglie and Bohm. Rather than discuss this example in detail, it will be more useful to consider just one experiment, to see how the observational underdetermination works there.[3]

The example is quantum tunneling. In classical mechanics, a particle can be 'trapped' by a potential 'barrier'. Consider, for example, a ball in a valley between two hills. Assuming the kinetic energy of the ball is sufficiently small, it will not be able to escape the valley—balls do not roll uphill on their own. In quantum theory, however, objects *can* escape similar barriers, even if they lack sufficient kinetic energy. This phenomenon is called 'tunneling'.

In quantum theory, one describes tunneling as follows. The wavefunction of the particle 'trapped' by potential barriers will extend beyond the barriers—it will be non-zero in regions beyond the barriers. But wherever the magnitude of the wavefunction is non-zero, there is a non-zero probability of finding the particle. Hence, there is a chance of finding the particle outside the barriers.

How do we understand tunneling in the orthodox quantum theory—the 'Copenhagen interpretation'? The key point is that, in this interpretation, a particle *cannot have* a precise position and momentum (hence kinetic energy, taken as momentum-squared divided by mass) at the same time. So, suppose that we localize the particle between the barriers. In doing so, we

[3] A thorough discussion is in Cushing (1994). I added the qualifier 'non-relativistic' only because I shall be discussing non-relativistic quantum theory. There do exist, in some cases, Bohmian alternatives to relativistic quantum mechanics, and quantum field theory.

render momentum (hence energy) not merely unknown to us, but in fact unpossessed at all by the particle. But then there is no paradox; there is no need to explain how the particle could get past the barrier, for there is no energy that it has, and specifically it has no energy less than what is 'needed' to get past the barrier. Strange? Yes, but that's the Copenhagen interpretation.

Bohm's theory tells a different story. According to it, particles have two kinds of energy, the usual (classical) energy, and energy due to a 'quantum potential'. (Particles also *always* have a definite position and velocity, hence momentum.) This quantum potential has no source, and does not dissipate with distance. (Strange? Yes, but that's the Bohm theory.) Moreover, it provides enough energy (to some particles) to get past the potential barrier.

Both theories predict tunneling. Indeed, examination of the details reveals that they predict the same relative frequency of tunneling-events. The same situation holds for other quantum-mechanical phenomena. We seem compelled, then, to admit that the Bohm and Copenhagen theories are observationally equivalent.

But given the claims of the previous section, there is evidently a problem in saying so. First, we must ask, in what sense of 'observation' are the two theories observationally equivalent? Second, how are the 'observations' of the Bohm theory 'the same as' the observations of the

Copenhagen theory, given the theory-ladenness of observation?

I have already indicated my preferred answer to the first question: observational evidence for (or against) a scientific theory is garnered by our ability (or inability) to confirm the scientific observations predicted by a theory. Moreover, the point of calling two theories 'observationally equivalent' is to suggest that the observational evidence is the same for both. But this answer to the first question only makes an answer to the second the more difficult, for we have also seen that theory-ladenness of scientific observations is the most difficult to overcome.

Perhaps, then, we should reconsider the claim that scientific observations are evidentially important. Perhaps psychological observations are what scientific theories ultimately predict? If so, we would be in a happy position, for we would simultaneously ease the present troubles about understanding observational equivalence *and* eliminate the epistemic problem about how psychological observations provide reasons to believe in scientific observations.

And there may be some reason for hope. After all, consider again the tunneling experiment, described above. One way to perform it is to place particle-detectors (e.g., photographic plates) outside the potential barriers. If the plates are shielded from extraneous particles, then any light showing up on the plates indicates that a particle tunneled through the barrier. (Actual experiments are more complex, but the essential points are the same.) Now, could we not say that both the Copenhagen and the Bohm theories predict that the light will (with a certain frequency) shine on the detector, so that no matter what their account of the tunneling itself, they agree about the light at the end of the tunneling?

If 'the light at the end of the tunneling' is taken to refer to a psychological observation, then 'no'. We my well wonder (along with, e.g., Cartwright) whether either theory can be used by itself (without the aid of questionable approximations or extra phenomenological laws) to predict the effect at the detector, but in any case we can be certain that neither theory predicts anything about psychological observations.

Perhaps, then, at least each of these theories together with some theory of perception does the job? Perhaps, but it is hard to see how, for any such theory of perception must 'recognize' the very observational equivalence we seek to understand, by recognizing that a Bohmian flash and a Copenhagen flash are—at least as far as their effect on perception is concerned—the same thing. The most optimistic hope we could have is that someday, some theory will accomplish this feat, but again, it is hard to see how, without the theory's being told (by us) that Bohmian and Copenhagen flashes are 'the same'.

But perhaps I am being unfair. After all, both theories say that 'a particle' gets through the barrier, right? So while they may differ on the explanation of how, or why, a particle gets through, they agree on the conclusion anyway, taken now not as a psychological observation but as a scientific observation. But the point is that they *do not* obviously agree on the conclusion, because the very notion of a particle is different in the two cases. In the Copenhagen theory, for example, a particle is the sort of thing that can have a position at one time, but not at another, while Bohmian particles have a position at every time. How do we know that a Copenhagen particle's 'hitting the detector' amounts to the same thing as a Bohmian particle's 'hitting the detector'? And of course the problem is even worse, because what it means to 'hit a detector' (or indeed, to *be* a detector) is itself different in the two theories.

At this point in the argument it is easy to become impatient and to assert that *of course* Bohmian flashes and Copenhagen flashes are 'the same'. I agree, it *is* obvious. What remains to be understood is what grounds we have for believing in this sameness.

Note that van Fraassen's account does not help with this question. He says that theories have 'empirical substructures' (a controversial assumption) and that two theories are empirically equivalent just in case their empirical substructures are isomorphic. But what counts as an isomorphism? How are the requisite isomorphisms defined? How do we recognize them?

3. Towards a solution

Another way to ask the question of the previous paragraph is to ask how one identifies 'relations' amongst the observations of one theory with those of another. This way of putting the question indicates both where van Fraassen's account helps, and where it needs help. It had been thought that empirical equivalence must amount to agreement on observation-statements. I have suggested that (scientific) observations themselves are where agreement must lie, but even that way of putting the point is misleading. Indeed, given that scientific observations are more or less definitionally theory-laden, agreement on them in the most straightforward sense is in general impossible. As van Fraassen's account makes clear, however, all that is *really* needed is some 'legitimate translation' from the scientific observations of one theory to those of another. To put it somewhat more mathematically, we require a 'structure-preserving map' from one theory's (scientific) observations to another's.

The problem now is twofold: what is the structure of a theory's (scientific) observations, and how can we identify 'structures' in one theory with those in another? Without an answer to these questions, we cannot say what an isomorphism *is*, much less whether one exists.

Thus far I have implicitly restricted attention to physics. I have done so to respect the limitation of my own expertise, but also because physics may well be unique in this respect: theories in physics have (in the modern era, at least) an explicitly stated (and studied) mathematical formalism. For example, the theory of Riemannian manifolds is the formalism of general relativity, and the theory of Hilbert spaces is the formalism of quantum theory.

The central and somewhat programmatic suggestion of this section is that physical theories may be understood as formalisms plus interpretation, and that observationally equivalent theories must have in common a formalism, by which I understand 'mathematical theory'. (They clearly must also have something else in common, but it remains unclear what that something else might be.) I am proposing, in other words, to search for a distinction not between observation and theory (nor between observable and unobservable), but between formalism and interpretation.

There are two claims involved in this suggestion: first, that such a distinction can be made, and second, that observational equivalence has as a necessary condition shared formalism, in the sense that each of the observationally equivalent theories *can* (though perhaps not conveniently) be expressed in the same formalism. I finish my suggestion with a few reasons to support each of these claims, and a recognition of the major remaining problems.

First, why should we believe that a principled distinction can be drawn between (mathematical) formalisms and their (physical) interpretations? For one thing, it is clear that one can *think* about the mathematical formalism without thinking about the physical interpretation. For example, mathematicians can easily think about the theory of Hilbert spaces without considering (in some cases not knowing) the use to which that theory is put in quantum theory. This fact highlights the 'one-way' nature of the 'distinction' between mathematical formalism and interpretation (which, therefore, one may not wish to term a 'distinction'), for it may turn out that while one can think about the formalism without considering the interpretation, one cannot also think about the interpretation without thinking, in some way, about the formalism.

Another reason to believe that such a distinction can be drawn is that formalism often leaves open interpretation. We have already seen a spectacular example: both the Bohm theory and the Copenhagen theory can be stated (though the former not conveniently) in the language of the theory of complex analysis and Hilbert space, and yet they provide distinct, indeed in some ways contradictory, accounts of tunneling (and the world in general).

There are also some reasons to believe that observational equivalence has as a necessary condition a shared formalism. I have already given, somewhat elliptically, one such reason. *If* you believe that observational equivalence does not (because of some form of theory-ladenness) amount to having shared observations, then something like van Fraassen's strategy (of finding an isomorphism from the observations of one theory to those of another) seems required. But, the very notion of an isomorphism does not seem to make sense outside of a formal context, where one can define the notion of 'structure' in a way that makes it clear what it would mean for a map to 'preserve structure'. In other words, the objects that are mapped by the isomorphism must be mathematical, or be represented mathematically, so that they are capable of having 'structures' that can be said to be preserved by the isomorphism.

Of course, none of the reasons that I have just cited are compelling, and indeed there are substantial problems facing the proposal I have made in this section. I will finish this essay with a brief account of those problems.

First, concerning finding a distinction between formalism and interpretation, there remains the problem of saying *how* one distinguishes mathematical from physical content. It seems clear that the theory of Riemannian manifolds is a mathematical theory, while that theory plus the proposition 'particles follow geodesics' is not a mathematical theory, but by what *principle* do we make this assertion? It is correct, but unhelpful, to note that mathematical theories are about mathematical objects while physical theories are about physical objects. One wants to know what makes the difference between them. The problem is, in the end, a problem for the philosophy of mathematics. It is, after all, the problem of what characterizes mathematical theories.

Second, concerning the claim that observational equivalence requires a shared formalism, a great deal more needs to be said. One might suppose, for example, that the relevant sort of isomorphism exists between just the 'observational parts' of the 'observationally equivalent' theories. Of course, this supposition requires, again, some way of distinguishing observational from non-observational parts of a theory, and part of the value of my suggestion (that that observational equivalence requires a shared formalism) is that it may eliminate the need for drawing such a distinction. Nonetheless, I have thus far provided only a rather convoluted reasons for accepting this suggestion. I will add here only that every alternative to special or general relativity can be expressed in the same formalism as those theories, and again, every alternative to the standard 'Copenhagen' version of quantum theory can be expressed in the language of Hilbert space.

To review: I have supposed, in this paper, that the following two theses *are* true: observations (in each of the senses described) are theory-laden, in some way or other; and there are observationally equivalent, but otherwise distinct, theories, where 'observation' here means 'scientific observation'. I have argued that it is not clear how both of these theses could be true, and I have suggested that the solution to this puzzle lies in agreeing with van Fraassen that observational equivalence amount to the existence of some sort of translation from one theory to another (as opposed to ‘identity’ of observations). In particular, I have suggested that the proper relation is expressibility in a shared formalism.

I have run quickly past several subtleties that would have to be addressed in a longer account. For example, I have not considered the case where two theories are only partially distinct, so that there is a more or less straightforward ‘translation’ from one to the other in the case of at least some observations. Nor have I considered observationally equivalent theories that are each underwritten by more fundamental theories. Investigation of these and related points must await another day.

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