At several points in the reading of van Fraassen's book, I feared I would no longer be a realist by the time I completed it. Fortunately, sheer doxastic inertia has allowed my convictions to survive its searching critique, at least temporarily, and as of today, van Fraassen and I still hold different views.

I am a scientific realist, of unorthodox persuasion, and van Fraassen is a constructivist empiricist, whose persuasions currently define the doctrine. I assert that global excellence of theory is the ultimate measure of truth and ontology at all levels of cognition, even at the observational level. van Fraassen asserts that descriptive excellence at the observational level is the only genuine measure of any theory's truth, and that one's acceptance of a theory should create no ontological commitments whatever beyond the observational level.

Against his first claim I will maintain that ontological excellence or "empirical adequacy" is only one epistemic virtue among others, of equal or comparable importance.

And against his second claim I will maintain that the ontological commitments of any theory are wholly blind to the idiosyncratic distinction between what is and what is not humanly observable, and so should be our own ontological commitments. Criticism will be directed primarily at van Fraassen's selective scepticism in favor of observable ontologies over unobservable ontologies; and against his view that the superempirical theoretical virtues (simplicity, coherence, explanatory power) are merely pragmatic virtues, reducible to the instrumental fact of theory's truth. My aims are not merely critical, however. Scientific realism does need reworking, and there are good reasons for moving it in the direction of van Fraassen's constructive empiricism, as will be discussed in the closing section of this paper. But those reasons do not support the sceptical theses at issue.

Before pursuing our differences, it will prove useful to emphasize certain convictions we share. Van Fraassen is already a scientific realist in the minimal sense that he interprets theories literally and he concedes them a truth-value. Further, we agree that the observable/unobservable distinction is entirely distinct from the non-theoretical distinction, and we agree as well that all observation sentences are irredeemably laden with theory.

Additionally, I absolutely reject many sanguine assumptions common among realists. I do not believe that on the whole our beliefs must be at least roughly true; I do not believe that the terms of 'mature' science must typically refer to real things; and I very much doubt that the reasons of Homo sapiens, even at their best and even if allowed infinite time, would eventually encompass all or only true statements.

This scepticism is born partly from a historical induction: so many past theories, rightly judged excellent at the time, have since proved to be false. And their current successors, though even better founded, seem but the next step in a probably endless and not obviously convergent journey (for a most thorough and insightful critique of typical realist theses, see the recent paper by Laudan [4]).

Evolutionary considerations also counsel a healthy scepticism. Human reason is a hierarchy of heuristics for seeking, recognizing, storing, and exploiting information. But those heuristics were invented at random, and they were selected for within a very narrow, even minute, environment, cosmologically speaking. It would be miraculous if human reason were completely free of false strategies and fundamentally cognitive virtues, and doubly miraculous if the theorems we accept failed to reflect these defects.

Thus some very realistic reasons for scepticism with respect to any theory. Why then am I still a scientific realist? Because these reasons fail to discriminate between the integrity of observables and the integrity of unobservables. If anything is compromised by these considerations, it is the integrity of theories in general, of cognition generally. Since our observational concepts are just as theory-laden as any others, and since the integrity of those concepts is just as contingent on the integrity of the theories that embody them, ontological ontology is rendered exactly as dubious as our non-observational ontology.

This parity should not seem surprising. Our history reveals mistaken ontological commitments in both domains. For example, we have had occasion to banish phlogiston, caloric, and the luminiferous aether from our ontology, but we have also had occasion to banish witches, and the stary sphere that turns about us daily. And these latter items were as 'observable' as you please.

Since these sceptical considerations are indifferent to the distinction between what is and is not observable, they provide no reason for resisting a commitment to unobservable ontologies while allowing a commitment to observable ontologies. The latter appear as no better off than the former. For me then, the empirical success of a theory remains a reason for thinking the theory to be true, and for accepting its overall ontology. The inference from success to truth need not be tempered by the sceptical considerations adduced, but the failure to resist to unobservable commitment is rendered selectively dubious. Thus I remain a scientific realist. My realism is highly circumspect, but the circumspection is uniform for observables and unobservables alike.

Perhaps I am wrong in this. Perhaps we should be selectively sceptical in the fashion van Fraassen recommends. Does he have other arguments for refusing factual belief in ontological commitment beyond the observational domain? Indeed, he does. In fact, he does not appeal to historical induction or evolutionary humility at all. These are my reasons for scepticism (and they will remain, even if I manage to undermine van Fraassen's). They have been introduced here to show that, while there are some powerful reasons for scepticism, those reasons do not place unobservables at a selective disadvantage.

Very well, what are van Fraassen's reasons for scepticism? They are very interesting. To summarize quickly, he does a compelling job of arguing that realist arguments (from Smart, Sellars, Salmon, Boyd, and others) to the effect that, given the aims of science, we have no alternative but to bring unobservables (not just into our calculations, but into our literal ontology. He also argues rather compellingly that the superempirical virtues, such as simplicity and comprehensive explanatory power, are at bottom merely
pragmatic virtues, having nothing essential to do with any theory's truth. This leaves one wondering about the adequacy of any theory. Roughly, a theory is empirically adequate if and only if everything it says about observable things is true. Empirical adequacy is thus a necessary condition for truth.

However, claims van Fraassen, the truth of any theory whose ontology includes unobservables is always radically underdetermined by its empirical adequacy, since a great many logically incompatible theories can all be empirically equivalent. Accordingly, the inference from empirical adequacy to truth now appears presumptuous in general, especially since it has been disconnected from additional selective criteria such as simplicity and explanatory power, criteria which might have reduced the arbitrariness of the particular inference drawn. (This says van Fraassen, who do not need to make such wanton inferences since we can perfectly well understand science as an enterprise that never really draws them. Here we arrive at his positive conception of science.) An enterprise whose sole intellectual aims are empirical adequacy and the satisfaction of certain human intellectual needs.

The central element in this argument is the claim that, in the case of a theory whose ontology includes unobservables, its empirical adequacy underdetermines its truth. (We should notice that in the case of a theory that is completely free of unobservables, its empirical adequacy does not underdetermine its truth: in that case, truth and empirical adequacy are obviously identical. Thus van Fraassen's selective scepticism about unobservables.) For any theory T, if T is a theory with T unobservables, there will always be many other such theories, empirically inextendible with T, and empirically equivalent to it. In my view, the notions of "empirical adequacy" and its cognate relative term "empirically equivalent" are extremely thorny notions of doubtful integrity. If we attempt to explicate a theory's "empirical content" in terms of the observation sentences it entails (or entails if-conjoined with possible future background information), or with possible future theories), we generate a variety of notions which are variously empty, context-relative, ill-defined, or otherwise flawed and underdetermined. van Fraassen is entirely aware of these difficulties and proposes to avoid them by giving the notion at issue a model-theoretic rather than syntactic characterization. I am unconvinced that this improves matters decisively (on this issue see Wilson [7]). Let me sidestep the issue for now, since the matter is difficult and there is a simpler objection that we can make.

The empirical adequacy of any theory is itself something that is radically underdetermined by any evidence conceivably available to us. Recall that, for a theory to be empirically adequate, its observable things must be true— all observable things, the past, the indefinite future, and the most distant corners of the cosmos. But since any actual data possessed by us must be finite in its scope, it is plain that we here suffer an underdetermination problem no less serious than that claimed above. This is Hume's problem, and the issue is that even observation-level theories suffer radical underdetermination by the evidence. Accordingly, theories about observables and theories about unobservables appear on a par again, so far as scepticism is concerned.

Van Fraassen thinks there is an important difference between the two cases, and one's first impulse is to agree with him. We are all willing to concede the existence of some unobservables. Indeed, it is impossible to find evidence that would show us an otherwise unobservable thing to be otherwise unobservables. But the inference to entities that are unobservable is itself a matter of some difficulty. van Fraassen, however, regards the inference to unobservable entities as a matter of principle. He suggests that the inference to unobservable entities is a matter of principle.

I do not see that it is. Consider the different reasons why entities or processes may go unobserved by us. First, they may go unobserved because they are relative to our natural sensory apparatus, they fail to enjoy an appropriate spatial or temporal position. They may exist in the Upper Jurassic Period or the future, but unavailable to us because they may reside in the Andromeda Galaxy. Second, they may go unobserved because, relative to our natural sensory apparatus, they fail to enjoy the appropriate spatial or temporal dimensions. They may be too small or too large, or too protracted. Third, they may fail to enjoy the appropriate energy, being too feeble, or too powerful, to permit useful discrimination. Fourth and fifth, they may fail to have an appropriate wavelength, or an appropriate mass. Sixth, they may fail to feel the relevant fundamental forces our sensory apparatus exploits, as with our inability to detect basic background neutrino flux, despite the fact that its energy density exceeds that of light itself.

This list could be lengthened, but it is long enough to suggest that being spatially or temporally available can justify our sensory apparatus. The sensory apparatus is only one of the many ways in which an entity or process can fall outside the compass of human observation. There is perhaps some point to calling a thing "observable" if it falls only the first test (spatio-temporal proximity), and "unobservable" if it fails any of the others. But that is only because of the contingent practical fact that one generally has somewhat more control over the spatio-temporal perspective of one's sensory systems than one has over their size, or reaction time, or mass, or wavelength sensitivity.

Hume's problem is not the case for van Fraassen. We have seen that van Fraassen's appeal to a different kind of underdetermination, which is that of a gap of spatial distance, while refusing to tolerate an ampliative inference when it bridges a gap of, for example, spatial size. Hume's problem and van Fraassen's problem (or Duhem's problem) collapse into one.

Van Fraassen attempts to meet such worries about the inescapable ubiquity of speculativa activity by observing that "... it is an epistemological fact that one may as well hang for a sheep as for a lamb" ([5], p. 72). Agreed. But it is a principle of logic that one may as well hang for a sheep as for a lamb. Thus van Fraassen's lamb (empirical adequacy) is not another sheep.

Let me summarize. As van Fraassen sets it up, and as the instrumentalists set it up before him, the realist looks more gullible than the non-realists. He non-realists are willing to extend belief beyond the observable, while the non-realists insist on confining belief within that domain. I suggest, however, that it is really the non-realists who are being more gullible in this matter, since they suppose that the epistemic situation of our beliefs about observables is in some way superior to that of our beliefs about unobservables. But in fact their epis-
Let me now try to address the question of whether the theoretical virtues such as simplicity, coherence, and explanatory power are epistemic virtues genuinely relevant to the estimate of a theory's truth, as tradition says, or merely pragmatic virtues, as van Fraassen urges. His view preserves an empirical adequacy, or evidence of empirical adequacy, as the only genuine measure of a theory's truth, the other virtues (insofar as they are distinct from these) being cast as purely pragmatic virtues, to be valued only for the human needs they satisfy. Despite certain compelling features of the account of explanation that van Fraassen provides, I remain inclined towards the latter view.

My reason is simplicity itself. Since there is no way of conceiving or representing "the empirical facts" that is completely independent of some empirical assumptions, and since we will occasionally confront theoretical alternatives on a scale so comprehensive that we must also choose between competing modes of conceiving what the empirical facts are before we can even begin to make an empirical choice between these global alternatives cannot be made by comparing the extent to which they are adequate to some common touchstone, "the empirical facts." In such a case, the choice must be made on the comparative global virtues of the two global alternatives, T₁-plus-the-observational-evidence-therein-continuous-string-of-singular-beliefs-about-its-local-environment. These "intellectual intensifiers," if you will, but let us suppose that they provide him with much the same information that our perceptual judgments provide us.

For such a person, or for a society of such persons, the observable world is an empty set. There is no question, therefore, of their evaluating any theory by reference to its empirical adequacy, as characterized by van Fraassen (i.e., isomorphism between some observable features of the world and some 'empirical structure' of one of the theory's models). But such a society is still capable of science, I assert. They can invent theories and test their predictions against observations. Thus, the fact-as-represented-in-part spontaneousof facts, hazard predictions of the facts-as-represented-in-possible spontaneousover, and so forth.

In principle, there is no reason why they couldn't learn as much as we have. (cf. Feyerabend [3])

But it is plain in this case that the global virtues of simplicity, coherence, and explanatory power are lost and the only framework left for representing the empirical facts. Indeed, they even dictate how such a framework is constructed by the enriching infant in the first place. One's observational taxonomy is not 'read off' the world directly; rather, one comes to it piecemeal, and by stages, and one settles on that taxonomy which finds the greatest coherence and simplicity in the world, and most and the simplest lawful connections.

I can bring together my protective concern for unobservables and for the super-empirical virtues examined above by way of the following thought experiment. Consider a man for whom absolutely nothing is observable. All of his sensory modalities have been surgically destroyed, and he has no visual, tactile, or other sensory experience of any kind. Fortunately, he has mounted on top of his skull a microcomputer fitted out with a variety of environmentally-sensitive transducers. The computer is connected to his association cortex (or perhaps the thalamic lobe, or Wernecke's area) in such a way as to cause in him a continuous string of singular beliefs about his local environment. These "intellectual intensifiers," if you will, but let us suppose that they provide him with much the same information that our perceptual judgments provide us.

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I now turn from critc of van Fraassen's position to advocate. One of the most central elements in his view seems to me to be well-preserved and urgentely deserving of further developement in his growth and introductory chapter, his aim is to reconcile the relation of theory to world, and the units of scientific cognition, and the virtue of such units when successful. He says,

I use adjecttive 'constructive' to indicate my view that scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable. ([5], p. 5)

The traditional view of human knowlgedge is that the unit of cognition is the sentence or proposition, and the cognitive virtue of such units is truth. Van Fraassen rejects this overtly linguistic guise for his theory as a set of models (rather than as a set of sentences), and he sees empirical adequacy (rather than truth) as the principal virtue of such units.

Though I reject his particular reconception, and the selective scepticism he draws from it, I think the move away from the traditional conceptions is entirely correct. The criticism to which I am inclined is that van Fraassen has not moved quite far enough. Specifically, if we are to reconsider truth as the aim of product of cognitive activity, I think we must reconsider its application right across the board, and not just in some arbitrarily or idiosyncratically segregated domain of unobservables. That is, if we are not to mirror the naive formulations of scientific realism, we should move in the direction of pragmatism rather than in the direction of a positivist instrumentaism. Let me elaborate.

When we give a variety of cognitively active creatures on this planet—sea slugs and octopi, bats, dolphins, and humans; and when we consider the ceaseless reconfiguration in which their brains or central nervous system engage—adjustments in the response potentials of single neurons made in the microsecond range, changes in the response characteristics of large systems of neurons made in the seconds-to-hours range, dendritic sprouting and the new synaptic connections and the selective atrophy of old connections effected in the day-upwards range; then van Fraassen's term 'construction' begins to seem highly appropriate. There is endless construction and reconstruction, both functional and structural. Further, it is far from obvious that truth is either the primary aim or the principal product of this activity. Rather, its function would appear to be the ever more finely tuned administration of the organism's behavior. Natural selection does not care whether a brain has or tends toward true beliefs, so long as the organism reliably exhibits reproductively advantageous behavior. Plainly there is going to be some connection between the faithfulness of the organism's behavior. But just as plainly, the connection is not going to be direct.

While we are considering cognitive activity in terms and in all branches of the phylogenetic tree, we should note that it is far from obvious that sentences or propositions or anything remotely like them constitute the basic elements of cognition in creatures generally. Indeed, as I have argued at length elsewhere ([1], chapter 5; [2]), it is highly unlikely that the sentential kinematics embraced by formal psychology and orthodox epistemology represents or captures the basic parameters of cognition and learning even in humans. That framework is part of a common-sense theory that threatens to be either superfluous or of little help. If we are ever to understand the dynamics of cognitive activity, therefore, we may have to reconceive our basic unit of cognition as something other than the sentence or proposition, and we may also have to receive its virtue as something other than truth.

Success of this sort on the descriptive/explanatory front would likely have normative consequences. Truth, as currently conceived, might cease to be an aim of science. Not because we had lowered our sights and reduced our epistemic standards, as van Fraassen's constructive empiricism would suggest, but because we had raised our sights to see the epistemic goal even more worthy than truth. I cannot now elucidate such goals, but we should be sensibly of their possible existence. The notion of "truth," after all, is the central element in a normative theory, and pross makes progress no less than theoreia.

The notion of truth is suspect on purely metaphysical grounds anyway. It suggests that there is a single, complete, and final true theory at a minimum, the infinte set of all true sentences. Such a theory would be, by epistemic criteria, the best theory possible. But nothing whatever guarantees the existence of such a unique theory. Just as there is no largest positive integer, it may be that there is no best-theory whatever. Secondly, there is always an on-going battle among theories, and so ad infinitum. If we were thus unable to speak of the set of all true sentences, what sense could we make of truth sentence-by-sentence?

These considerations do invite a 'constructive' conception of cognitive activity, one in which the notion of truth plays at best a highly derivative role. The formulation of such a conception, adequate to all of our epistemic criteria, is the outstanding task of epistemology. I do not think we will find that conception in a model-theoretic version of positivist instrumentaism, nor do I think we will find it quickly. But the empirical brain begs unravelling, and we have plenty of time.

Finally, there is a question put to me by Stephen Stich. If ultimately my view is even more sceptical than van Fraassen's concerning the relevance or applicability of the notion of truth, why do I call scientific realism at all? For at least two reasons. The term 'realism' still marks the principal contrast with its traditional adversary, positivist instrumentaism. Whatever the integrity of the notion of truth, theories about unobservables have just as much a claim to truth, epistemologically and metaphorically, as theories about observables. Second, I remain committed to the idea that there exists a world, independent of our cognition, with which we interact, and of which we construct representations: for varying purposes, with varying penetration, and with varying success. Lastly, our best and most penetrating grasp of the real is still held to reside in the metaphysics provided by our best theories. Global excellence of theory remains the fundamental measure of rational ontology. And that has always been the central claim of scientific realism.

REFERENCES

Hilary Putnam

EXPLANATION AND REFERENCE

1. GENERAL SIGNIFICANCE OF THE TOPIC

In this paper I try to contrast realist theories of meaning with what may be called 'idestalist' theories of meaning. But a word of explanation is clearly in order.

There is no Marxist 'theory of meaning' but there are a series of remarks on the correspondence between concepts and things, on concepts, and on the impossibility of a priori knowledge in the writings of Engels (cf. Engels, 1959) which clearly bear on problems of meaning and reference. In particular, there is a passage in which Engels makes the point that a concept may contain elements which are not correct. A contemporary scientific characterization of fish would include, Engels says, such properties as life under water and breathing through gills; yet lungfish and other anomalous spe-

cies which lack these properties are classed as fish for scientific purposes. And Engels argues, I think correctly, that to stick to the letter of the 'definition' in applying the concept fish would be bad science. In short, Engels contends that:


concept is not exactly correct (as a description of the corresponding natural kind) but that does not make it a fiction. (2) The concept is continually changing as a result of the impact of scientific discoveries, but that does not mean that the concepts do not correspond to the same natural kind (which is itself, of course, also changing). Again, without attributing to Engels a sophisticated theory of meaning and reference, it is fair, I think, to restate the essential gist of these two points in the following way: concepts which are not strictly true of anything may yet refer to something; and concepts in different theories may refer to the same thing. Of these two points, the second is obvious for most realists; with a few possible exceptions (e.g., Paul Feyerabend), realists have held that there are successive scientific theories about the same things: about heat, about electricity, and so forth; and this involves treating such terms as 'electricity' as trans-theoretical terms, as Dudley Shapere has called them (cf. Shapere, 1967). But the terms that have the same reference in different theories. The first point is more controversial; the idea that concepts provide necessary and sufficient conditions for class membership has often been attacked but, nonetheless, con-

stantly reappears. Without it, however, the other point is moot. Bohr assumed in 1911 that there are (at every time) numbers p and q such that the (one dimensional) position of a particle (one-dimensional momentum is p; if this was part of the mean-

ing of 'particle' for Bohr, and in addition, part of the meaning means 'necessary condition for membership in the extension of the term'; then there are not particles in Bohr's sense, and, indeed, there are no par-


ticles in Bohr's sense'. (And no 'electrons' in Bohr's sense of 'electron', etc.) None of the terms in Bohr's 1911 theory were referred! It fol-

ows on this account that we cannot say that present electron theory is a better theory of the same particles that Bohr was referring to. I take it that this is the line of thinking that Paul Feyerabend represents. On an account like Shapere's, however, Bohr would have been referring to electrons simply when he used the word 'electron', notwithstanding the fact that some of his beliefs about electrons were mistaken, and we are referring to those same particles notwithstanding the fact that some of our— even beliefs included in our scientific 'definition' of the term 'electron'—may very likely turn out to be equally mistaken. This seems right to me. The main technical contribution of this paper will be a sketch of a theory of meaning which supports Shapere's insights.

An 'idestalist' theory of meaning, as I am using the term, might go like this (in its simplest form): the meaning of such a sentence as 'electrons exist' is a function of certain predictions that can be derived from it (in a pure idealist theory, these would have to be predictions about sensations); these predic-

itions are clearly a function of the theory in which the sentence occurs; thus electrons exist has no meaning apart from this, that or the other theory, and it has a different meaning in different theories.

The question of reference is a harder one for an idealist: the essence of idealism is to view scientific theories and concepts as instruments for predicting sensations and not as representatives of real things and magnitudes. But a sophisticated idealist is likely to say that the question of reference is trivial: if one has a scientific language containing the term 'electron', then one can certainly construct a metalanguage ML over it d la Tarski, and define 'reference' in such a way that 'electron' refers to electrons is trivial. But if different scientific theories T1 and T2 are associated with different formal languages L1 and L2 (as they must be if they have different meanings in T1 and T2), then they will be associated with different metalanguages ML1 and ML2. In ML1 we can say 'electron' refers to electrons, meaning that electron in the sense of T1 refers to elec-


3 In a letter written to Conrad Schmidt in 1895; cf. Marx (1942), pp. 52–50. My agreement is with Engels' realism, not his 'dialectical materialism'.

4 See, for example, the discussion by Hempel, in Aspects of Scientific Explanation, (Free Press, New York, 1965), pp. 217–18. A contrasting view is sketched in chapter 13, volume 1 of these papers.