
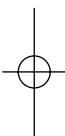




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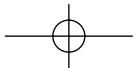
From a View of Science to a New Empiricism

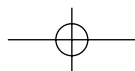
Bas C. van Fraassen



I am deeply thankful for this sustained and thoughtful critique. I almost wrote ‘deeply and humbly’ but that would have been inaccurate, for actually I am very proud to be its recipient. These essays have helped me to think about the issues in new ways and also, I hope, to refine my views so as to improve as well as extend them. In my response I will note several ways I have changed my mind in the years since *The Scientific Image*. Of each chapter I have taken up some part at length; all deserved more. If by being selective in my response I avoided some crucial problems, they will of course come home to roost sooner or later.

Two themes emerged as especially important. The first is the role of values in epistemology. This had initially appeared for me in the distinction between epistemic and pragmatic factors in how we evaluate scientific theories and the conditions in which we could accept them. Later that role became much more salient as I turned from theory choice in science to rationality of opinion and opinion-change in general. The second is the turn I see in the empiricist tradition, which leaves many ‘central’ problems of traditional epistemology behind. That there was a major sea change in philosophy during the first half of the twentieth century, everyone seems to agree. In the second half though, as I see it, much of this philosophical revolution was obscured and even lost again. When I tried to say what empiricism can be now, I took myself to be





consolidating the insights of that earlier time; but of course that is contestable (starting with the word ‘insights!’). Well, let’s see.

Although I am naturally concerned to parry criticism, I have attended especially to possibilities of conciliation, for matters both epistemic and metaphysical.

1. Epistemology

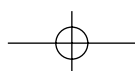
Are there aspects of *The Scientific Image* that I have found it necessary to revise or reject? Yes, certainly; specifically, two.

The first was guided by my larger change in view about probability after I came to Princeton. Chapter 6 of *The Scientific Image* mistakenly conflates (as I now see it) two questions: (i) what exactly an indeterministic theory says about what the world is like, and (ii) what it means to accept such a theory as empirically adequate. I still consider that chapter illuminating with respect to the first question. But it was a mistake to try and straightforwardly adapt a notion of acceptance designed for deterministic theories to the case where probability is involved. Even for a simple theory such as that the half-life of radium’s most stable isotope is 1602 years, it does not do to say ‘I believe it to be empirically adequate.’ In *Laws and Symmetry* I proposed a broader notion of acceptance of theories that applies first of all to probabilistic theories, and then can in special cases come down to belief in empirical adequacy *tout court*. But this was an issue not addressed here, so I shall leave it aside.

The second true modification I offered at the beginning of *Quantum Mechanics: An Empiricist View*. It is pertinent to what is here challenged by Philip Percival, so I can relate it in context of his discussion.

1.1 Empirical Adequacy versus Strength: Percival

Percival distinguishes evaluation of scientific activity from that of scientific theories. He points out that in *The Scientific Image* I seem to take it that science considered as activity is successful precisely if the theories produced meet the criteria of success for theories. That is indeed too narrow a conception of scientific practice. I would say now that I was then too immersed in the traditional rather myopic view of science ‘from above’, rather than ‘from within’ the processes and practices that lead to scientific success. Lately I have been trying to mend my ways, but not yet in print.



Empirical adequacy, as I characterize it, Percival says is so weak a property that even tautologies possess it, and so he renames it 'weak empirical adequacy'. Then he introduces also 'strong empirical adequacy' which takes (empirical) informativeness into account. I will stay with my own terminology, for I think his is somewhat misleading. Does he have reason to call empirical adequacy a weak notion? Then the same goes for truth, since tautologies are true; and there would be the same reason to introduce 'weak truth' and 'strong truth'. But better, it seems to me, to distinguish different senses of *weakness* rather than different senses of empirical adequacy or of truth:

- A criterion is weak in sense 1: easy to concoct pertinent examples that meet it
- A criterion is weak in sense 2: given any pertinent example, it is easy to show that it meets the criterion
- A criterion is weak in sense 3: given any pertinent task, it is easy to complete it in such a way as to meet that criterion

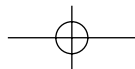
Truth and empirical adequacy are weak in sense 1, but not 2 or 3, and those seem to me the senses that matter more.

But Percival asks: is a tautology, which is empirically adequate, a better scientific theory than Newton's theory, which is not? No—I agree it is not! But here I see context-sensitivity located in 'better'. It seems to me that Percival's—just and reasonable—call for a more nuanced approach to evaluation may be better served in a somewhat different fashion from what he proposes. As he points out, in effect, if we do not take a theory to be true [empirically adequate], then we cannot offer it in answer to the question of what the [observable] world is like. At the same time, if the theory does not have substantial empirical strength (informativeness) we cannot present it as a substantial answer to that question.

But now I have come to the second point on which I later entered a real change of mind after *The Scientific Image*. I stonewalled a bit, but ended by qualifying my view of how theories are to be evaluated:

The empiricist takes this aim to be to give us empirically adequate theories; the realist says that it is to give us true ones. Now, we identify a theory as a class of models. So is not that aim at once satisfied, in either case, by someone who says: 'I have a nice theory. It has as models exactly those structures which are isomorphic to the real world in the following respects?' (*Quantum Mechanics*, pp. 7–8)

Obviously the real world, properly conceived, *is* (represented by) one of the models of that theory, so we believe it to be empirically adequate. But that

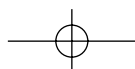
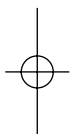


does not mean we accept that theory. We have no reason to make the other commitments that go into acceptance, to design a research programme for it, to use it to answer why or how questions, or to reclassify phenomena in its terms. The relevant pragmatic factors are missing because, however informative it is in a strictly objective or semantic sense, it is not informative *for us*.

The instrumental role of non-epistemic values in theory choice has been emphasized often by now. Larry Laudan, for example, exhibits a wide array of factors in theory choice in his 'The Epistemic, the Cognitive, and the Social'. To the dictum that 'a theory does not have to be true to be good', he writes, 'We can add . . . a new twist: a theory does not have to be false to be bad. A theory may be bad because it fails the test of possessing the relevant nonepistemic virtues.' And he adds 'Such values are constitutive of science in the sense that we cannot conceive of a functioning science without them, even though they fail to be intelligible in terms of the classical theory of knowledge' (p. 19).

My response in *Quantum Mechanics* also implies a qualification of how I glossed 'we want informative theories' in *The Scientific Image* (pp. 67–8), to the effect that we want empirical strength, which I characterized as a semantic feature, independent of pragmatic factors. The qualification is that, as with other virtues characterizable semantically, whether they are detectable depends on the formulations of the theory that we actually possess, and they are of no use to evaluation in practice unless they are detectable. Such detectability is a pragmatic factor as well, and relates to the informativeness issue at Percival's focus. The point applies equally to truth. We can't advocate acceptance of a theory on the basis that it is true (or empirically adequate)—that puts the cart before the horse—but only on the basis of what we can point out, here and now for anyone to see, that could be taken as a reason for acceptance.

Finally, I do fully agree that the scheme for evaluation requires such exploration as Percival provides to give context-sensitivity, as well as the communal character of scientific practice, their due. Percival carefully delineates versions of the criteria that are absolute and comparative, as well as ones that are non-comparative and context-sensitive, in a way that I did not. There is a crucial role for informativeness there, and the desire to have an empirically adequate theory may be in tension with the desire to have an empirically strong one. That point derives from the more general insight that desire for truth and desire to avoid error (to use William James's terms) pull in opposite directions.



1.2 Acceptance: Lipton, Cartwright, Ladyman

Peter Lipton, Nancy Cartwright, and James Ladyman all address acceptance. Lipton sees my specific version of it as instantiating a broader notion that has other applications, even to the problem of inconsistency in our beliefs. Cartwright and Ladyman turn to the rationale one could or could not have to limit belief in certain ways when accepting a theory.

Lipton

Lipton points out that we may often wish to accept something that is admittedly too informative, to the point even of self-contradiction. This is indeed why acceptance needs to be distinguished from attitudes that involve full belief. Both philosophers and scientists will feel this need. Entity realists and structural realists, for example, demarcate what in a theory is to be believed, if it is accepted, from the part on which one may remain agnostic or even disbelieving. The nice option of ‘reducing content’ may not be practically available. Specifically—contrary to the fable of a hygienically ‘observational’ vocabulary at the heart of science—it is in general not feasible to specify the conditions of empirical adequacy for a theory without recourse to that theory itself. Scientists who realize that their several theories with overlapping domain cannot be consistently combined—historically not an uncommon predicament—do not simply discard them or throw up their hands. Both general relativity and quantum theory, not consistently combinable as they are, are used in astrophysics, without waiting for the day when aspects to be retained can be explicitly specified.

In all of this Lipton is surely right. I will only demur a little at the point where he demands

that [what is to be believed] have an effective specification, in the sense that one can tell in practice whether or not some consequence of the full set is or is not to be believed. Otherwise one literally does not know what to believe: the attitude of acceptance would be indeterminate.

(I take it that ‘effective’ is meant here in the sense of the logician or computer scientist.) First, I would wish to emphasize the practical aspect. To decide, for example, to draw on a theory for certain purposes and to draw on a rival for certain other purposes, is a case in point, and does not involve knowing what to believe with this sort of hard-to-achieve specificity. On a more theoretical

level too, it seems to me that a qualitative specification alone may suffice. For example, the structural realist's specification, of what is to be believed if we accept a physical theory, is not often as clear-cut as it can be with classical electromagnetism, where Maxwell's later formulation—not relying on the character of a mechanical ether—was a major theoretical advance.

But while wanting to weaken Lipton's general scheme for acceptance in this way, I think also that it can be strengthened in another. Logicians have paid much attention to the extreme case on which he focuses, where the belief involved must be curtailed on pain of inconsistency. While 'relevance logic' has its egregious moments, the less ambitious parts—for example, the part that David Lewis (1982) could reconstruct as a 'logic for equivocators', or the parts reconstructed in my (1969) and (1983)—could be useful in elaborating Lipton's insights here.

Cartwright

Ladyman's 'The Epistemology of Constructive Empiricism' was presented at the same conference as Cartwright's 'Why be Hanged for Even a Lamb?' Her argument can practically be read, it seems to me, as an answer to Ladyman's Section 2 ('Empiricism and Constructive Empiricism'), though she took her challenge from Paul Churchland. But Ladyman's Section 3 ('The Pragmatic and the Epistemic') opens a new front, almost as if in response to Cartwright.

Cartwright addresses the case of acceptance in the very specific case of science and offers a partial defence of my submission that it involves belief only in the theory's empirical adequacy. The case I made was just that, as far as science and being scientific are concerned, no more belief is involved in acceptance of a scientific theory. The reason I gave was that the only belief that is *ipso facto* involved in acceptance is that the active criterion of success is met—and that the criterion of success is empirical adequacy. This is a view of what science is—where the question of what science is, is understood as asking what is the point, the *telos*, of that activity. That view is of course, as any view of what something is, contestable. But the objection by Churchland with which Cartwright began does not contest it:

Suppose, for the sake of argument, that there is a distinction between what is and what is not observable. Van Fraassen tells us that the proper epistemic attitude is to believe in what theory tells us about what is observable. But what is epistemically so good about what is observable? Perhaps there is epistemic justification for believing

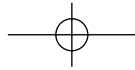
in what is observed, but after that, what is so special about the *observable*? It is an extension beyond what is observed and there seems no good epistemic reason to stop there.

This objection does not contest any view of what science is; instead it addresses a question in epistemology, in the ethics of belief.¹ There is an equivocation on ‘the proper epistemic attitude’. It could mean *the epistemic attitude involved in acceptance of a scientific theory*, but in Churchland’s objection it means *the epistemic attitude we ought to take to a theory we accept*. I do *not advocate* agnosticism about the unobservable, but claim that belief is supererogatory as far as science is concerned: you may if you like, but there is no need.² This distinction was already made quite clear in Bourgeois (1987), but Churchland’s objection is often raised. Yet this claim too is at odds with certain epistemological positions, in whose context the point of *The Scientific Image* would be moot. So even though it was not part of constructive empiricism to speak to that, Churchland’s objection is not irrelevant.

I am happy to see Cartwright’s answer for several reasons that have to do with my later attention to epistemology. It begins with reasons to believe what we observe [to be the case], but adds that this justification cannot simply be extended beyond what is observed, to what is observable. I’ll just say why I agree: I do not think that there is such a thing as Induction, in any form, and I would also express this in more or less her words: *there is no purely epistemic warrant for going beyond our evidence*. If there is to be a rationale—let alone justification—for selecting the observable as the range for the proper epistemic aspect of acceptance, that will have to be *something that is*

¹ In his contribution Maarten van Dyck makes the point quite clearly: ‘since SI van Fraassen has been stressing that constructive empiricism should be seen as a view on science, not as an epistemological position: it doesn’t tell us what we should (dis)believe, but it gives an answer to the question “what is science?” by indicating the criteria that determine what counts as success in science. Moreover, this view of what the debate on scientific realism is about is not a retraction on van Fraassen’s part, but clearly lies at the heart of SI.’

² Lipton (2004: 146) argues that this is inconsistent: you will believe that the table is a swarm of unobservable particles, but not believe that unobservable particles exist. But if you only accept the pertinent theory you do not believe that the table is a swarm of particles—only that it is theoretically classified (classified relative to that theory) as a swarm of particles. (See *The Scientific Image*, 58, regarding molecules, crystals, and tables.) The problem comes from taking too seriously the *informal* characterization of empirical adequacy on page 12 rather than the concept as it is defined within the semantic view of theories later on, when construing what it means to believe that a theory is empirically adequate.



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not, or not purely, epistemic warrant. Enter then, to use Harry Frankfurt's phrase, the importance of what matters to *us*, the community in whose terms observability is characterized:

I propose to end-run Churchland's objection by supposing that what is special about the observable is not an *epistemic* virtue at all. . . . Rather, it depends on the fact that we are creatures bound in a world of sensation.

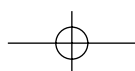
True, as Churchland retorted, we may care about things above and beyond the effects they could have in our experience, and want to arrive at some beliefs about them independently of how they affect what we will or can observe. But Cartwright sees a dividing line there, and adds to the above 'and, unlike other facts about us, this is not a matter of choice.' At this point she makes contact, it seems to me, with my classification of further beliefs in what our accepted theories say, as rationally permissible but *supererogatory* as far as the scientific enterprise is concerned.

As Cartwright emphasizes, her exploration of the views in *The Scientific Image* are not to be confused with her own views, which include that causings and properties are observed as well as such concreta as things, events, and processes, so that in her view my conception of the observable is debilitatingly impoverished.³ But the aspect of her argument that is of special interest for me, and where I can draw on her support, is her showing how values enter here into epistemology. This is also a theme in *The Empirical Stance*, and I will return to it below.

Ladyman

Ladyman's Section 3, 'The Pragmatic and the Epistemic', opens a new front however. He poses a challenge that has force precisely if, in agreement with the direction of Cartwright's solution, we give a large role to pragmatic factors. Ladyman writes, not unfairly, that I am still quite willing to speak about *knowledge* and indeed scientific knowledge. Nor is he unfair in the comment that I can't very well appeal to either of the typical internalist or externalist accounts of knowledge. Quite right, but as I see it, the internalist/externalist debate, like the foundationalist/coherentist debate and much else in traditional epistemology, has already been left behind (cf. my 2000). Writers in contextual epistemology and virtue epistemology of recent decades have

³ See further the reply to Cartwright in my (1993a).



argued similarly. But Ladyman's challenge goes beyond that, and he offers a dilemma:

Van Fraassen faces a dilemma. If a true belief can count as knowledge even though it has been adopted for pragmatic reasons, then scientific realists' entitlement to claim knowledge of electrons seems just as good (assuming there are electrons) as constructive empiricists' entitlement to claim knowledge of the empirical adequacy of a theory (assuming it is indeed empirically adequate). On the other hand, if it is a necessary condition for a true belief to count as knowledge that it has been adopted for purely epistemic reasons, then it seems that the only scientific knowledge we have is of what has so far been observed.

It would be a bad dilemma to face if 'know' were not so extremely context-sensitive, or if knowledge rather than belief and opinion should be the focus in epistemology.

To begin, let us not equate reasons why a belief is adopted with anything like justification for holding the belief. Even if you come to believe something for all the wrong reasons, or with ulterior motives, it is still one of your beliefs, and you still adopted it for those reasons. The only *good* reason (notice the equivocation!) to offer for holding a belief is an epistemic reason, that is, something that makes it more likely to be true. For you cannot coherently advocate believing theory T because of feature F while simultaneously saying that this feature does not make it more likely to be true. (You could perhaps truly point out even then that you came to this belief because of T's having F, but you would not be shoring up or supporting that belief by saying so.)⁴

Pragmatic factors, in fact values, do inescapably play a role in how we manage our opinion. For we must inevitably stick our neck out and form beliefs that go beyond our evidence—the extent to which we do so, the risk we take, can only be up to us, there can't be anything in the evidence to dictate *that*. That a risk is worth taking is a value judgement. And so, although we have to rephrase it, Ladyman's dilemma must still be faced here. Given this view of rational opinion management, can we ever rightly claim or attribute knowledge? And if so, aren't beliefs in the unobservable on a par with beliefs in the observable, in that respect?

I could again say, as with respect to Churchland's objection, that constructive empiricism is a view of what science is and not a normative position in the

⁴ I will come back to this in my response to Psillos below. For a helpful discussion in a more traditional context, compare Kelly (2002; 2003).

ethics of belief. But given that I was happy to have Cartwright's defence against that objection, I should be unhappy if there were no good response to Ladyman's dilemma. I think though that there is, and I take it from contextual epistemology. There are contexts in which we can claim or attribute knowledge, with complete warrant, to person X about subject S say, while in another context we would not be able to do so at all. The matter is not settled by how X came to the belief about S, though that is a relevant factor. What is more crucially relevant are the assumptions or presuppositions or implied standards in force in that context. David Lewis (1996), Keith DeRose (1992), and many others have detailed this; I'll just mention a simple example that conveys much of what is wrong with the old, different ways of thinking about knowledge. It's due to John Hawthorne.⁵ *Why is it quite in order for me to say that I know I am going to spend time in the Netherlands this summer, but not in order to say that I know that I won't die before then?*

I don't mean to be cavalier or dismissive on the subject of knowledge—as opposed to traditional approaches to knowledge. Belief and opinion are what guide action and planning, they provide a starting point wherein we forge our conceptions of how things are or could be. As to knowledge, all the philosophical puzzles that pertain to it specifically seem best transposed to philosophy of language, to investigation of the grammar, the logic, and most of all the pragmatics rather than semantics, of the term 'know'. But there is more to be said about this in response to Alexander Bird.

1.3 'Argument from Underdetermination': Van Dyck and Bird

Maarten Van Dyck and Alexander Bird address the so-called Argument from Underdetermination, formulated by Van Dyck as

- (1) All theories have empirically equivalent rivals.
- (2) Since empirically equivalent theories are equally supported by all possible evidence, all of them will always be equally believable.
- (UD) Belief in any theory must be arbitrary and unfounded.

and they agree that constructive empiricism was not advocated in that way. As Bird points out, some commentators 'employ a caricature of constructive empiricism that takes it to employ [that] argument', and Van Dyck notes that

⁵ Hawthorne (2004); for discussion see Gilbert Harman and Brett Sherman (2004).

even such perspicacious writers as Kukla and Psillos have at times succumbed to this misunderstanding.

Van Dyck

Van Dyck provides in addition a thorough explication of how the issue of underdetermination does play a role in *The Scientific Image*, which is different from that, to me, quite unacceptable argument.

I am quite proud never to have relied on the so-called Pessimistic Induction either, any more than on this Argument from Underdetermination—though the former has also at times, quite wrongly, been associated with *The Scientific Image*. Neither would be at all in harmony with the views I went on later to defend in epistemology, but whose beginnings are, as Van Dyck documents, traceable from *The Scientific Image* onwards. However, both Van Dyck and Bird add new challenges related to their discussion of underdetermination.

Van Dyck's challenge adds to the above points about the notions of acceptance and belief. To begin, he says, the distinction lies in what the reasons can be for these epistemic attitudes, referring to the following argument:

that a theory is more informative in some respect can be a reason for acceptance, but it cannot be a feature that makes the theory more likely to be true and hence not a reason for belief. If the reasons for belief and for acceptance are not the same, then belief and acceptance are not the same either.

This argument, he says, fails for the reason that Teller gave: that a theory is more informative in some respect can in fact be a reason for belief. It seems that I have to admit that, because I agree that pragmatic factors play an inescapable role in the rational management of our opinion. In my (2001) reply to Teller:

A belief held for ulterior motives is still a belief. It does not become acceptance instead of belief that way. The distinction between what a person believes and what s/he merely accepts is not made on the basis of why s/he has that attitude but on the basis of what that attitude is.

Van Dyck sees a collapse of the above argument.

I do not agree, because of the equivocity of 'reason' that I mentioned above. A belief may be held for reasons that are not good reasons to hold a belief. What I must admit is that, precisely because of this ambiguity, my own statement was ambiguous as well. The distinction is now sometimes made verbally in this way: someone 'has reason' to X versus s/he 'has a reason' to X. If I am short of

money I have reason to seek employment; but this may not be a reason I have, for example if lack of money does not bother me as much as it should.

So I should add above: an informational virtue may provide at best an ulterior motive for belief; in this way belief is distinguishable from acceptance.

Bird

In his last section, 'Cautious constructive empiricism', Alexander Bird argues that, although I did not use the Underdetermination Argument, I am nonetheless 'committed to the sceptical conclusion, and . . . constructive empiricism is implicitly committed to the above argument.' Bird's reasoning hinges on considerations about knowledge, and is thus linked to the part of Ladyman's chapter that I addressed above. Even for limited doxastic goals, he writes, one may lessen one's chances to attain them if one doesn't believe some proposition that one is in a position to know. Therefore refusal to believe something, for example, what a theory says about the unobservable, is reasonable only if backed up by the view that it would not be known even if it were to be believed—a sceptical view about knowledge with respect to that subject.

That is already one argument all by itself, but a still nicer one follows. We can't very well hold that a theory could be acceptable yet known to be false—so if theories that go beyond the observable are ever acceptable, we must hold that they cannot be known to be false or their rivals to be true.

My first reaction is that at the very least several theories could be acceptable while it is known that at least one of them must be false. For if two theories, mutually incompatible, are empirically equivalent they could both be acceptable—but obviously they could not both be true. (Whether both are acceptable depends of course on other factors, such as whether we believe them to be empirically adequate and also whether they are worthy of certain commitments of a more pragmatic sort as well.)

But in view of the context-sensitivity of 'know' I would like to go further still. In our present context of discussion, can we not claim with good warrant that for example there are no classical atoms while adding that a carefully circumscribed version of classical gas theory is empirically adequate? And indeed acceptable, for use in that carefully circumscribed domain? Our reason for claiming to know that there are no classical atoms would of course come from the strong belief that with the careful circumscription removed, the resulting larger theory, and any feasible variant thereof, would be at odds with empirical evidence we already have.

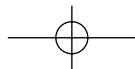
This may not be very satisfactory to anyone holding a more robust view of knowledge. For I am assuming that the question of whether a knowledge claim is ‘really true’ is not in order, but gives way to the question after contexts in which it is a warranted claim or attribution. Contextual epistemology, where this semantics-to-pragmatics shift is practised, has had to face counterarguments from ‘invariantists’. But it seems to me that if anywhere, the context-dependence of ‘know’ is most salient in the history of science, and especially in those episodes where scientists have found reason to be quite conscious of their methodology. Are there contexts in which it would be quite appropriate to say ‘we know now that there are free quarks’? Yes. But I think such a context is not one in which one could also say that we hold the initial claim about quarks as a hypothesis, subject to the fortunes of future evidence.⁶ And that, I submit, along with numberless scientists defending the scientific spirit, is the proper epistemic attitude in any context of scientific research.

1.4 Rationality and Revolution: McMullin

McMullin begins with a far-reaching critique of the epistemological liberality I see as needed for empiricism. The transitions displayed in scientific revolutions are rational. To this McMullin and I agree. That they are both revolutionary and rational I see as a challenge to contemporary epistemology. Does our epistemology need amending? Well, that depends on what our epistemology is, where we start from—and here we differ. Perhaps epistemology as I conceived of it, in my critique of the idea that there could be rationally compelling ampliative rules, is challenged, while McMullin’s view of ‘the inference that makes science’ is not. McMullin sees me as admitting an element of irrationality, since I allow for free choice and the impetus of emotion in places where traditional patterns of rational deliberation fall short. But is that so?

When I first heard McMullin on this issue, I noticed with surprise that I had never stopped to ask myself whether Galileo’s or Copernicus’s contemporaries had suffered the sort of epistemic despair that I was describing. This is telling, yet I don’t take it as a criticism! The actual persons involved may not have been as painfully insightful as a Pascal or Kierkegaard into their own true epistemic condition. If I so depicted them then that, I admit, was a literary conceit. The

⁶ For the claim to know entails the claim to have eliminated all contextually pertinent alternatives—as argued for example in Lewis’s seminal (1996).



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challenge I see to epistemology comes with the absurdity deriving from the logical relationship between the old, replaced theory and the new one that replaced it. It is not so surprising—and perhaps even for the best!—if the actors involved in the revolution lack a clear understanding of those logical relations.

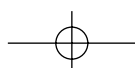
That logical relationship is not simply one of mutual contradiction, not simply a disagreement about the right predictions of observable phenomena. It extends to the conceptual framework in which both the observable and the unobservable are characterized, and the language in which each is described. Look for instance at John Wheeler's take on the conceptual change introduced by Everett:

It is difficult to make clear how decisively the 'relative state' formulation drops classical concepts. One's initial unhappiness at this step can be matched but few times in history: when Newton described gravity by anything so preposterous as action at a distance; when Maxwell described anything as natural as action at a distance in terms as unnatural as field theory; when Einstein denied a privileged character to any coordinate system, and the whole foundations of physical measurement at first sight seemed to collapse. (Wheeler 1957: 463)

Yet such transitions belong to our most valued scientific developments.

In a well-defined decision context, the options are clear—but they may all be bad, and then reasoning within that context cannot lead us out of them. The decision problem itself must be changed, in that case, if there is to be a way out. This sort of transformation is precisely the role that Sartre ascribed to emotion—that is, the role of changing the decision-situation itself on the subjective side. Taken in that sense the term 'emotion' does not, or at least should not, carry connotations of irrationality, and it still seems apt to me, as long as we understand it to refer to that role.

McMullin replies in effect that scientific revolution does not at all present a difficult challenge to *his* epistemology. Note that he explicitly takes his distance from the more formal sorts of epistemology that I have criticized elsewhere, such as Inference to the Best Explanation. His characterization of the process of epistemic updating has room for all the moves that are required in such revolutions, and classifies them as rational. We may therefore be in substantial agreement if the processes of rational change he discerns are ones that (a) fail to fit either Bayesian or epistemic decision theory forms, and involve both (b) free choices and (c) relevant factors on the side of actors' values and interests. Then



the decisions involved in rational change of opinion over time are not purely theoretical. Then we are making common cause against the more simplistic forms of naturalism or Bayesianism in epistemology.

1.5 Toward a Reconciliation in Epistemology: Psillos

Stathis Psillos and I have, it seems to me, been moving toward each other in epistemology. I have become more careful to distinguish epistemic criteria for rationality from those for rationality overall, and to moderate my language by saying that incoherence is ‘a defect of reason’ rather than ‘irrational’. If I ignore the arithmetic mistakes in my own account of my finances, that is a defect of reason, but I’ll readily admit that it would hardly be rational overall to assign high priority to straightening out my bankbook.

Similarly I have become much more observant with respect to Bernard Williams’s point which Psillos relates here. There is a pragmatic if not logical incoherence in saying that I believe something but that I do so because I *decided* to go beyond my evidence, let alone that I do so for ulterior motives. But as Psillos spells out, voluntarism in epistemology does not have to involve the idea that we can believe at will. That making up one’s mind involves some choice along the way, or that there is an inescapable role played by value judgements in managing our opinion, does not imply that it is simply a matter of free choice made ‘at will’, let alone admit the possibility of random variations in one’s opinion produced at will.⁷ On his side, and related to this, Psillos details his liberal conception of Inference to the Best Explanation. When I say that IBE can’t be a *rule*, and he says it can be a rule but not *algorithmic* (‘The algorithmic conception of rationality is nothing more than a phantom’), it is mainly words that separate us. That we have a core agreement here he notes, and rightly points out that we should all have learned the point long since from Pierre Duhem, who is certainly one of my heroes too.

⁷ Would anyone ever suggest this for a change in behaviour? Whether or not to murder one’s mother is surely a matter of the will, and involves an element of choice, but it is presumably for a sane person an extremely difficult point to reach, and would involve large scale, difficult to achieve, alterations in behaviour, circumstances, personal relations, inclinations, beliefs, attitudes . . . In this respect, changing one’s opinion is not so different from changing one’s behaviour. With respect to Williams’s point we may observe that there is also a pragmatic incoherence in the person who has a goal while realizing that he does not value its achievement, or does something while reflecting that he has only bad reasons for doing it.

I am not entirely clear on the distinction between structure and content for opinion, in the sense Psillos means it. If the orthodox Bayesian identifies epistemic rationality with coherence in the sense purely of (1) never violating the probability calculus, and (2) always changing opinion only by conditionalizing on something taken as evidence, we presumably have there a paradigm of a structural conception of rationality. But if coherence includes pragmatic as well as logical coherence, and never sabotaging oneself by one's own lights, and commitment only to policies for opinion change not at odds with epistemic integrity, then 'structural' may not be as obviously apt. Policies coherent in this sense can certainly include such respect for one's own prior opinions (though not the orthodox Bayesian slavishness) that Psillos mentions as contentful. ('Yet, according to van Fraassen, the subject's beliefs (that is, the content of her belief) will be constrained by the subject's *prior opinion*. This is contentful. Hence his conception of rationality is *not* purely structural.'))

The distinction seems to be blurred further when Psillos considers calibration, for he writes 'the demand for calibration shows how the *content* of an opinion matters to its rationality' and 'if a belief is (perfectly) calibrated, then it is not *just* right (that is, correct). In a sense, it is also the *rational* belief to have.' I find this puzzling, precisely because of the analogy to truth. Could we say that if a belief is true then it is also the rational belief to have? I would be inclined to place on the side of rationality only the concern not to have opinion of a sort that could not even in principle have a hope of being perfectly calibrated. Whether or not one's opinion is well calibrated is in the end a matter of luck: nature cooperates and fulfils our expectations or it doesn't, and no degree of rationality will determine that.

What I especially appreciate in Psillos's contribution is how he brings together the various elements of my voluntarist epistemology, gleaned from their scattered locations. The main form in which I see expressions of opinion is that of probabilism: qualitative, comparative, quantitative, vague or precise, absolute or conditional subjective probability or expectation value. But I insist that the judgements formulated in terms of subjective probability must be understood as *expressions*—not attributions—of opinion. They are therefore in the first person; but not to be confused with the autobiographical statements made in the same words to for example one's therapist. There is a rough and ready criterion for the distinction. Suppose I say 'our president seems to me to be acting more and more strangely.' Ordinarily if I say that, there

is an implicature that I am giving you information, relevant to your own actions and/or opinion management, on which you can rely. If I say it to my therapist, however, there is no such implicature, and it is to be taken as simply a report on my mental state. This distinction parallels that for value judgements—imagine ‘I greatly value my father’s approval’ said in different contexts.

Secondly, the form in which I see changes of opinion is, as Psillos also details, as acceptance of constraints on one’s posterior opinion—constraints that can take many forms. That is to be contrasted with the orthodox Bayesian’s picture of evidence coming as it were with the voice of an angel and hence inducing immediate full belief, and also with the ‘robot’ model of stimulus and response.

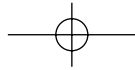
Thirdly there is the conception of rationality as bridled irrationality: what is rational is anything that is rationally ‘within bounds’. This must be taken in the context of reflection on agents freely exercising their rights while respecting their commitments and obligations, that is, agents who actively adhere to epistemic policies that they can stand behind. Watch out for the confusion of even a Bayesian *agent* with a Bayesian *patient*, that is, someone whose opinion over time just happens to be representable by a properly evolving probability function! With a bit of ingenuity and no fear of hidden variables practically anyone can be represented as being even an orthodox Bayesian patient—criteria of rationality do not, it seems to me, really apply in such a case.⁸

Psillos makes a strong argument that to stop here leaves us with too liberal an epistemology. To indicate how our criteria must go beyond coherence he points to how we should deal with evidence:

The claim that an agent shouldn’t disregard the evidence for her beliefs is not an empty dictum. A rational agent should regard *all* evidence that bears on a certain belief (or hypothesis) judiciously, try to take it into account in coming to adopt a belief (or a hypothesis) and then form her judgement in its light. This principle (let’s call it *the principle of evidential support*) goes far beyond the demand of coherence.

and again,

⁸ In the mainly technical literature on this subject, there are confusions or conflation on all these issues. The distinction between expression and attribution, between accepting constraints and being a stimulus responder, and between being an epistemic agent or a patient, tend not to be observed—see for example Weisberg, forthcoming.



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It is not a reply to the argument above that the evidence can and does have a bearing on how beliefs *change*. This should of course be granted. But this is another substantive principle of rationality that goes *beyond* coherence. Someone who takes coherence to be sufficient for rationality needs an extra principle in order to make evidence *count* in belief revision.

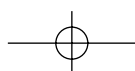
This would be straightforward if, on the one hand coherence is conceived of in the narrow sense of the orthodox Bayesian, and on the other hand, there were an opinion-independent status for the notion of evidence. Neither is right, it seems to me.

When we evaluate how someone else is doing, we will judge how well s/he responded to the evidence that came his or her way—using our own classification of what was evidence and what was not, as well as of what did or did not come that person's way. Timothy Williamson cut at least one Gordian knot by arguing that our evidence is all and only what we know. But again, our attribution of knowledge to someone is not only based entirely on our opinion about what is and what is not the case, it is also dependent on other contextual factors. If the person in question has quite a different opinion of what it is that s/he knows or has received by way of evidence, we can say that s/he is mistaken, but not convict of irrationality on the basis that evidence did not receive its proper response.

What then of such evaluations in the first person, of oneself? In one sense, there can't be any such thing: we can evaluate our past, or envisaged future, from our present point of view, but when it comes to evaluating our present opinion from our present point of view, no more than coherence can be pursued. Psillos's point then becomes similar to the point of Moore's paradox: since 'evidence' is an endorsing term, I can't coherently maintain that something is evidence I have but I am framing my posterior opinion without taking it properly into account. To maintain that would be to convict myself of self-sabotage by my own lights.⁹

Thus, given the way in which I broadened my concept of coherence in the years since my turn to probabilism, it seems that by now I am in substantial agreement with Psillos's insistence that something more than

⁹ The point is even easier to make if with Williamson we equate our evidence with what we know: I can't coherently maintain that this is something I know, but not part of my opinion henceforth!



(narrow) coherence is required for epistemic rationality, but I can still just call it coherence, broadly conceived.

2. Metaphysics

2.1 Materialism Left Behind, Transcendentalism Ahead?

Bitbol and McMullin

Michel Bitbol challenges my views on materialism, where he sees me as at best overly charitable, and follows it with a diagnosis of how an empiricist needs to leave traditional realism still further behind. I relate this to a challenge by Ernan McMullin to acknowledge the realism to which I do still adhere.

Bitbol

David Lewis wrote that there was no need to shift in our terminology from ‘materialism’ to ‘physicalism’. As he saw it, there was no real shift in the position which, though named at a time when physics had a certain form, was after all never wedded to a specific view of what matter is: ‘Now our best physics acknowledges other bearers of fundamental properties. . . . But it would be pedantry to change the name on that account.’¹⁰ But on the one hand, the prevailing shift in nomenclature has surely a rhetorical component that should not be ignored. And on the other hand Bitbol, a physicist profoundly engaged in the exploration of quantum theory, does not see our current physical world-picture as so simply related to the world of traditional materialism. Perhaps Lewis’s insistence on continuity in the rhetoric is just the explicit side of contemporary physicalists’ attempts to hide a fundamental problem in their view.

All observable phenomena, including the verbal, intellectual, and emotional, fall within the domain of the sciences. We don’t know what future sciences will offer us in their world-pictures, but it is a good bet that many philosophers, still wary of the mind–body dualism spectre, will call all of it ‘physical’. That this spectre is still so scary should be surprising—as far as I can tell, we in Western philosophy have not been mind–body dualists since the late seventeenth century, and outside Western philosophy, perhaps no one ever

¹⁰ See *The Empirical Stance*, footnote 25 to page 56.

was. So the cultural phenomenon of so much physicalist piety in contemporary analytic philosophy seems better understood as evidencing not any substantial thesis about what the world is like, but a certain pattern in attitudes and commitments.

Bitbol bids us look into the role that a certain physical world-picture still plays when the physicalist seeks for support, and attempts to fashion a metaphysics that can 'go well' with his philosophical stance. That picture, of a world ordered by substance, causality, and interaction among substances as alone intelligible, still has strong grip on the contemporary imagination. Even after much time spent on the foundations and interpretation of quantum theory, it still had a considerable grip on mine. So in *Quantum Mechanics* I argued, in accord with some and in opposition to others, for an equivalence of field-pictures and many-particle-pictures, at least for the beginnings of quantum field theory (Fock space formalism). This shows some wish to see the 'persisting substances' view of nature preserved as at least one feasible way of seeing the world.

Bitbol is right to criticize this. In fact it was through the arguments of among others Michel Bitbol himself that I changed my mind on this point. As Bitbol writes, the equivalence argued for will work only for a restricted domain of validity, where particle-number itself need not be treated as a quantum observable, subject to superposition. And he is quite right to add 'An interpretation should stand up alone as a self-coherent whole, not as a verbal appendix of a formal method; especially when the interpretation cannot acquire any autonomy with respect to the method, or when it irresistibly transforms into another interpretation as soon as one attempts to endow it with the sought autonomy,' which he then shows to be the case, in some detail. Some logical manoeuvres are always possible in formal bookkeeping, but they are indulged at some cost, and the cost is too great in this particular case.

So by the time of *The Empirical Stance*, I had at least come round to Bitbol's side on this issue in the philosophy of physics. What of metaphysics? I do not mean, what metaphysics do or should I embrace?—rather, what should be our view of the cluster of materialist and physicalist positions that so persistently crop up in current philosophical literature? After his searching critique of the particulate conception of matter, even in the abstract forms that a philosopher may go to, Bitbol shows how it has tended to cluster with certain values affecting methodology. That philosophical views tend to cluster in that way was Dilthey's theme in his discussion of differences in *Weltanschauung* and can

be seen already in Kant's discussion of empiricism (versus dogmatism, in the 'Antinomies', and versus noologism in the 'History of Pure Reason'). 'Is' does not imply 'ought' and yet what appear to be kinds of factual opinions tend to cluster systematically with certain kinds of value judgement. From both Bitbol's and my own perspectives, the values he discerns in the materialist camp are hampering rather than beneficial. Really, it is the physicalists themselves who should be delving into this side of their passionate convictions. . .

But then Bitbol challenges me to delve similarly into my own limits. In his last section he describes the limitations in my empiricism—as seen from a transcendentalist point of view—that prevent me from a still further remove from analytic metaphysics. This concerns our different understandings of a view we seem to have in common: 'Rationality will consist not in having a specially good starting point but in how well we criticize, amend, and update our given condition' (*The Empirical Stance*, p. 139). Concerning our 'given condition', our prior opinion, from which we must start at whatever moment we set ourselves the task of responding to new evidence or new theories, Bitbol writes:

here arises a major point of disagreement, which bears on the intensity and scope of the criticism. A neo-Kantian philosopher of science (whose model is Cassirer) would say that criticizing our 'given condition' can mean nothing less than considering it as a hidden source of *interpretation*. This implies a generalization of the hermeneutic circle.

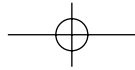
And it is true, I do not see interpretation as something going all the way down, but rather starting from where we are. Under the heading of interpretation I do not address perception, for example, but the theories science offers us.

This view of where interpretation leaves off relates to a recent discussion between Ernan McMullin and myself (McMullin 2003; van Fraassen 2003).

McMullin

McMullin appreciates that constructive empiricism is set squarely within a common sense realism that was foreign to much of the empiricist tradition. This common sense realism I do not see as leading either to a dictate to believe the theories we accept, or to, for example, a correspondence theory of truth, or other such metaphysical position.¹¹

¹¹ I have recently replied along these lines in another discussion: see Rosenhagen (2006) and my reply to that paper, pp. 152–9 in my (2006).



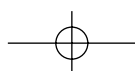
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What does it consist in? When I elaborate a philosophical view, I do so in a form of discourse that I trust for that role: to function as a common basis for participants in the dialogues in which this view can be proposed and defended. The common basis I assume is language in which reference is unproblematic to trees and mountains, people and books, to lightning and car crashes, as well as to the processes of ageing, burning, and flooding. It is a part of our language learned, or at least learnable, before we are corrupted by our teachers' poorly assimilated but oh so arrogantly presented book learning.

To trust something is to regard it as reliable for its use, though defeasible. What alternative do I have? Only to choose some other part of language that I and others both can understand. When phenomenologists (were there any, ever?) spoke of a more basic language, they spoke of something that did not exist yet, something—as so often happens in philosophy—baptized before it was born. Similarly for sense-data theorists, operationalists, and all their kin, trading in castles in the air and pie in the sky.

The privileging involved is not based on belief in a metaphysical theory about how this part of the language is 'grounded'. (In what language would *that* theory be presented?) To see philosophy as always conducted in a trusted language-in-use does not imply seeing it as oblivious of the language it lives in. But in this, as elsewhere, we always start from where we are; we can't step out of where we are into a presuppositionless discourse any more than into a view from nowhere.

I do see a use for the technical concepts and categories found in traditional metaphysics, namely as resources for *interpretation* of scientific theories. These notions can include for example substance, causality, or haecceity, as well as rival notions introduced by metaphysicians who take those categories to lack application at all. The enterprise of interpretation, its flagships being twentieth-century interpretations of relativity theory and quantum mechanics, is not a pursuit of truth. The nearest I can say is that it pursues a sense of understanding in which the question of truth is bracketed, in order to give us a handle on the conceptual structure of a theory, seen from various perspectives, and to give us some assurance of its coherence. Thus conceived, interpretation must of course start in a language we accept as our own (at present), which we speak without a 'bracketing' reservation, but to which we can then add technical devices. The criteria and norms for this activity are nowhere explicit, unfortunately. In the similar case of interpretations of literary or artistic works,



the hallmark of success is perhaps best taken from Oscar Wilde's essay 'The Critic as Artist'.

Paradoxes, such as what David Lewis called 'Putnam's paradox' result if we abstract from the explicitly indexical distinction between *our* language and other possible or actual languages. The language that I count as ours, in the context of philosophy of science and elsewhere, is one in which reference is unproblematic at least to things, events, and processes that we can observe. Bitbol is not unfair to point to its structure and charge that

[w]e can now better understand why Constructive Empiricism tends to be indulgent towards the attempt at building ontologies (such as the particulate one) by extrapolating from the archetype of macroscopic observable bodies: most likely because this archetype coincides with the *de-facto*-absolute starting point of Constructive Empiricism itself.

While that is fair enough, I think I can still add something that may lessen the bite. I do not see our own language as an *absolute* starting point for interpretation, but just as *the one we have*. And to take it as our starting point, as the language in which we can discuss physics and nature, is not to commit ourselves to the superiority of theories that can be interpreted in similar form. But a crucial difference does remain: while our starting point is ever changeable, it also marks at its own moment the line where interpretation begins, and I do not conceive of interpretation as already involved in judgements expressed in our own language.

2.2 Secular and Sacred: McMullin, Lipton¹²

When in his own contribution Ernan McMullin turns from epistemology to metaphysics, it is to urge me to add clarity to how I see our relation to the divine. Peter Lipton too addresses the question of how empiricism might relate to philosophy of religion. McMullin worries, I think, that I might slide into a theological agnosticism where God, the divine, the sacred are all just an 'I know not what', while Lipton might be suggesting precisely something of that sort. At first sight my distance from Lipton's view of acceptance of a faith

¹² I am very indebted to Eleonore Stump for her comments relating to this part of the reply. Especially her discussion of Aquinas's view concerning positive knowledge of what God is not, and the possibility of analogical attribution, gives me some confidence that I am not talking myself into a trivializing 'I know not what' position.

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appears to be as great as it is from natural theology, and as great as the distance of those two to each other. But closer inspection lessens this impression.

McMullin

The language of the Creed is not exactly free from metaphysical concepts, which were historically central to the philosophical stance of the Church Fathers, or so it would seem. Thus the Nicene creed has 'being of one substance with the Father', and of course the traditional explanation of the real presence of Christ in the Eucharist involves the concepts of substance and accident. But even orthodox Catholics will not require Aristotelian, Thomist, or neo-Thomist metaphysics for entering the Church. Without such a metaphysics, though, how to explain what we mean when reciting the Creed? But what, I want to ask in turn, is presupposed in this demand for explanation?

McMullin asks 'what concepts, if any, *do* we have available for the God who is met in encounter?' and submits that the distance between the God of Abraham, Isaac, and Jacob and the God of the philosophers must not be allowed to become a gulf:

If it does, what happens in the encounter of which van Fraassen speaks might well escape conceptualization entirely, including the responsible use of terms like 'God' and 'Creator'. Some basic features of our world, notably its very existence, would in that event have to be denied any particular religious significance.

No experience escapes conceptualization entirely, but this is a matter of more and less. The extent to which we can describe a given experience, the extent to which it falls under concepts available, may be quite shallow.¹³ Is it really the case that unless theological categories relieve this shallowness, the world we experience is denied any particular religious significance?

In the last chapter of *The Empirical Stance* I took up what it is to be secular, a question that should be answerable in purely secular terms. I meant to limit myself to that, while convinced that the similar question about the religious cannot be answered adequately in such terms.¹⁴ When I did happen to go

¹³ Think here of John McDowell's response to Hume's 'missing shade of blue'. I may not have a specific colour word already, but pointing with the words 'this colour shade' I form and apply a demonstrative concept, which can be used in re-identification, both by myself and by the listening onlookers.

¹⁴ I have sometimes expressed my own orientation more directly, for example in my (1988; 1993c; 1999).

somewhat beyond that, even Father McMullin shied away, writing ‘In the face of so personal and so powerful a testimony, merely philosophical query tends to shrivel.’ Indeed, that sort of response I should keep out of academic discussion.¹⁵

But then, what is to be done? Not metaphysics, according to me, but also not nothing. We can approach the subject obliquely, through concerns the religious have in common with the secular. One place is philosophy of art—Arthur Danto’s characterization of art as the *transfiguration of the commonplace* I would like to adapt in reference to religious experience: the world made strange. Though the secular and religious see the same mountains, the seas, the stars, the same people around them, they do not see them the same way. To see the world as created is not, I think McMullin would agree, a matter of believing that there is a Demiurge. So what is it, to see the world as created? For the religious there are moments, not quite caught up in the task at hand, when inanimate objects also aren’t simply ‘at hand’ or ‘to hand’, when not only the liturgy or the sacraments are outward signs of grace. But now I am perhaps already transgressing into poetry. The difference is one I don’t see how to describe adequately in secular discourse, to which all of traditional metaphysics belongs.¹⁶

Lipton

My view here seems certainly at first sight hardly even comparable with that expressed by Peter Lipton. He writes that Descartes found a balance

by being a realist about religion and something like a constructive empiricist about science. My own preferred resolution would run the other way, with a realist attitude towards science and a kind of acceptance of religion, retaining the content of one’s own religious tradition but only believing part of it, a part that is compatible with our best science.

¹⁵ There is more leeway for this in a book than in a journal or conference, I think, since the latter tend to involve a narrowly defined context of participation; books can afford a variety of contexts with different model readers (to use Eco’s term).

¹⁶ Jaeger (2006) includes a detailed critical response to that chapter of *The Empirical Stance*, in some respects not far from McMullin’s critique, but quite different in its suggestions for an alternative view that a philosopher could have. I am not yet in a position to express more than vague senses of agreement and disagreement with either alternative, and I have some doubt that it is possible to do more in ways that would not presuppose something which would place it outside dialogue with the secular.

At first sight, to me both the problem and the offered solution are unreal, or focused on something inessential or irrelevant. St Augustine, it seems to me, set the right precedent for latter day discussions of Genesis and the Big Bang or Genesis and Evolution. But I have quickly to qualify that impression: we cannot take recourse to allegory or metaphor with respect to much more central claims, that have a special role in liturgy and prayer, and matter in different ways. Since Lipton did not spell this out very far here, I would like to read his remarks in the context of another paper of his, 'Science and Religion: The Immersion Solution'. There he warns the reader:

I do not want to encourage the common and primitive practice of presenting a picture of religious life that would reduce it to religious doctrine. My intention is closer to the opposite: I want to make more room for a religious form of life in the discussions of the relation between science and religion, and I do not suppose for a minute that religion is reducible to religious claims: there is much more to religion than that. (Lipton, forthcoming)

We can see in that essay that Lipton is considering an attitude, in part epistemic, in part directed to norms and values, that may be called 'acceptance rather than belief' but importantly different from the notion of scientific acceptance in constructive empiricism. Accepting a religious text, on Lipton's construal, involves believing some but not all of its claims, 'but which claims we believe is largely externally determined, by moral reflection, and in some cases by science' (ibid.).

If there is a difference between us, it lies in the word 'largely'. I am inclined to think only the less central claims can be seen in this light. Perhaps even when I assert that concepts of God salient in philosophy offer but a misleading simulacrum of the divine, I am already making a positive claim to know something that is hard to place with respect to Lipton's view.

But Lipton goes further when he emphasizes *immersion*. There is again an analogy, but not identification, with acceptance of a scientific theory: for the practising scientist that comes with immersion in its conceptualization of the domain of inquiry. Perhaps my own attempts to address the tension on which Lipton focuses has mainly been an exploration of such immersion, both in my (1993c) and in *The Empirical Stance* where I take up the views of three theologians, one Protestant and two Jewish, Bultmann, Fackenheim, and Buber. But as I say in the appendix about Bultmann, their theologies are not to be read as philosophy.

2.3 Unacknowledged Metaphysics? Chakravartty

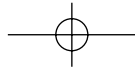
While some of Anjan Chakravartty's current essay (and even more of his previous writings) relates to the idea of philosophy as a stance, it also includes a strong argument that all empiricism already includes a sizeable element of metaphysics. The name 'metaphysics' covers a multitude, of course, of views if not of sins, and he is careful to refer to the kind of metaphysics that I see as anathema to empiricism. That is the sort of theory-spinning that pretends to the mantle of science while driven solely by the demand for explanation and satisfaction with explanations-by-postulate. So the question is then: can an empiricist position do without belief in the results of such theorizing?

The question will be answered differently depending on how it is made still more precise, and depending on the extent to which empiricism today must continue with features characteristic of its past.

Philosophical positions are always identified, by their opponents, with their past mistakes—happily adherents can also learn from the past. So I am not entirely happy with his characterization of the range of possibilities open to us. *Weak empiricism* he associates with the idea that sensory experience is the *source* of all knowledge of the world, which does not preclude substantive beliefs about the unobservable. *A stronger empiricism* takes weak empiricism for granted and adds that all knowledge of the world is *about* experience.

I would not subscribe to either. The very idea of 'source' brings in one sort of metaphysical frame for epistemology, with no explication that I can see as tenable for an empiricist (cf. my 1995). But it also makes no sense to me to say that our knowledge is solely about experience. I know a lot more about rocks and seas than I know about experience; and I can remember much more about what happened in the town where I grew up than I can remember about what my experience was like. This way of setting the stage saddles us with conceptions of experience and knowledge that should have no place in what empiricism must be now.

Chakravartty follows this with an analysis of the anti-foundationalist view of experience and knowledge of chapter 4 of *The Empirical Stance*. He begins with the announcement 'I would now like to consider whether empiricism itself is metaphysical. The surprising answer, I believe, is that it is.' The metaphysical element he finds is precisely in this view of the role experience can play in supporting knowledge claims. I would caricature my own view if I tried to summarize it again here in a few words. But we can note that I see the role of



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'*sola experientia*', the call to base our knowledge on experience alone, as having a double role: it demands a conservative policy with respect to belief change, but also allows for a use in challenges to that policy. That is possible precisely because *what the deliverances of experience are* has sense only within the context of a background that cannot be captured in or equated with a 'text'. That background includes, as Chakravartty notes, 'an understanding or tradition exemplified by a community', 'a generally tacit understanding that those who are members of an epistemic community share, and that unifies their practices of empirical investigation.' It is precisely at this point that he sees me as embroiled in metaphysics after all:

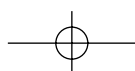
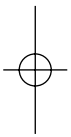
What sorts of things are tacit understandings, or traditions, or paradigms? They are unobservable, cognitive, cultural, heuristic entities, underlying the phenomenon of observation on which empiricism is partially grounded. Like many complex social entities they are posited for important explanatory reasons, to account for the phenomena we do experience.

In other words, he takes me to be postulating something beyond our ken, to satisfy a demand for explanation of an empirical phenomenon.

Is that really what it is? If it is, I have indeed failed blatantly in what I set out to do. In chapter 3 I had outlined two forms of epistemology, the first ('objectifying epistemology') being the attempt to come up with a theory of cognition, whether naturalistic or metaphysical. I rejected this as a form I did not wish to pursue, and contrasted it with an inquiry into the explication and evaluation of various forms of the 'enterprise of knowledge', concentrating on norms and values that guide rational management of opinion.

Chakravartty's criticism implies that in the next chapter, on his understanding, I was engaged in objectifying epistemology. I was offering a theory of cognition, with place assigned to certain unobservable complex social entities to explain how what we call relying on and learning from experience is possible.

I think I can reasonably resist that reading. The arguments I explored following Feyerabend tend to show that certain conceptions of experience, and of opinion responsive to experience, *make no sense*. On such conceptions experience speaks with the voice of an angel, and our response consists in passive assimilation of what it tells us. The *sola experientia* rule makes one kind of sense on this conception, and there implies that our task when managing



opinion is simply to ensure that our belief is proportionate to the evidence of our senses.

The problems that this rule runs into, when its presuppositions are scrutinized, show that conception to be untenable. The very meaning of what we encounter in experience relies on understanding which cannot be equated to knowledge that this or that is the case. Our experience consists precisely in all that happens to us of which we are aware. But the question of *what that was*, how it is to be understood, and hence of what constraints it must place on posterior opinion, will not receive the same answers ‘from within’ and from a third person observing us. What it means is in each case understood within a certain background that cannot be presuppositionless, but is also not capturable in a set of presupposed propositions.

The *sola experientia* rule, or Newton’s Fourth Rule, can play its role precisely because *what the deliverances of experience are* has sense only within the context of a background that cannot be completely captured in or equated with a ‘text’. Our experience consists precisely in all that happens to us of which we are aware. Suppose we ask now ‘just what was Peter’s experience in this barroom fight?’ The question of what that was is distinct from the question of how he responded to it with altering opinions during its course. As onlookers we may characterize it very differently: that is our response to something happening to us. If our cultural differences are great, so will be those responses. What really happened? Was he physically humiliated, or did he perform a truly adept defence-and-escape? We and Peter are both aware of what was happening to him, but we may see it quite differently. Peter of course makes no distinction at the same time between *what was happening to him* and *what he took to be happening to him*. The onlookers’ view is not dissimilar to the way later scientists see earlier experiments. The report of what happened in the laboratory comes in theory-laden language, as Duhem correctly emphasized. The later scientist, speaking within a different theoretical context, will say ‘what really happened, what they really observed, was . . .’ and follow that with a description couched in new-theory-laden language.¹⁷

¹⁷ See further the third part of my (1993b). The issue does not come up in Chakravarty’s chapter, but perhaps it is as well to emphasize again that this point does not remove objectivity from the question of what happened, what was observed. That the question of what the deliverances of experience are—i.e. the information gained thereby—is answered differently depending on the tradition or context in which it is asked and answered is something almost too obvious to state for a common sense realist, and does not imply that phenomena are theory-dependent in some

What do we gain by these reflections? Not a rival ‘objectifying’ theory of cognition, but a display of inadequacy for a theory of cognition that seems to have misled empiricists—not them alone, I think. After this destructive argument, what should we do? Not, it seems to me, frame a rival theory of the same kind—but rather leave the enterprise of ‘objectifying’ epistemology, whether metaphysical or naturalized. We can still work with models of opinion and of opinion change—evaluation requires preliminary description that is acceptable pro tem—but our focus can shift to understanding the norms, the criteria of evaluation, that are (partly) constitutive of what opinion is.

Chakravartty can now point out that in doing so, we will advocate some models over others—models that limit propositional representation for example—and in those models we can see features that are underdetermined by a person’s actual behaviour and discourse. Certainly—one thinks at once of for example Isaac Levi’s conception of confirmational commitment, and my reference to acts of acceptance of constraints on one’s posterior opinion. Evaluation of something starts with a notion, if only pro tem, of what it is like—value judgements involve factual judgements and are inconceivable without them.¹⁸ But whatever is involved here, in epistemology developed in such a more pragmatist vein, it is surely not a factual postulate submitted to explain the empirical phenomena of cognition.

3. Stance Empiricism: Preamble on ‘Experience’

3.1 My Mistake

My own thinking about empiricism involved to begin a great mistake. Throughout the 1980s I remained in thrall to one of the great deceptions in textbook philosophy. Both in my reply to critics (1985: 286) and in *Laws and Symmetry* (p. 8) I wrote of empiricism as the position that experience is our one and only source of information about the world.

That is amazing—as I suppose our mistakes perhaps always are in retrospect. We should see this idea in the perspective of how the term ‘experience’ is

non-trivial sense. The sort of example from science I gave here has its own complexity, though, explored perspicaciously in e.g. Radder (1995) and Buchwald (1995b).

¹⁸ Something like the converse is true too: factual judgements make sense only as elements of a discourse governed by certain norms, constitutive of the character and use of such discourse. Cf. Putnam (1983).

actually used, and how a fiction replaced it in modern philosophy. As Dewey pointed out, the ordinary use is still there in the initial division in ancient medicine, between the dogmatists who relied on theories and the empiricists who relied on the accumulated experience of the medical profession. The latter day conception of experience signalled in that ‘source of knowledge’ slogan is a curious philosophical miscreant: something like ‘a psychic event involving a single individual, with a “content” logically independent of what is happening to that individual.’ It is a mystery how anyone could mention this with a straight face as the basis on which scientific knowledge is built—or attribute that view to anyone else. But I was steeped in the textbook history of philosophy which depicts the rationalism–empiricism prelude to Kant in just that way, and taken in by it.

When I started thinking for myself about this, I became aghast at the implications of what this could mean, and at the disconnect in my own thoughts between this ‘official’ view of empiricism and what I really knew about it. I soon realized also that the ‘textbook’ version of the history was far out of date. My critique of that slogan understanding of empiricism began in talks given in the early 1990s, issuing in two articles, ‘Against Naturalized Empiricism’ (incorrectly published as ‘Against Naturalized Epistemology’) and ‘Against Transcendental Empiricism’. There I began to formulate the conception of empiricism as a stance. It is not easy to live down that past mistake, which came with my initial concentration—as Joseph Kockelmans pointed out—on framing an empiricist view of empirical science without focusing on what empiricism is.

3.2 Overcoming the Mistake

In *The Empirical Stance* I gave a rather quick argument to show that the older forms of empiricism, presenting themselves as a doctrine about experience, reduce to absurdity. For me this quick argument came in the context of previous discussion of that purported ‘doctrine’, which I did not supply there. In ‘Against Naturalized Empiricism’ I had argued that such a slogan as ‘Experience is our only source of information’—or anything of that ilk—*makes no literal sense*, and cannot be the core of an empiricist position. The slogan appears recurrently in many guises: in the mixture of philosophy and psychology, as yet not distinguished, of the seventeenth- and eighteenth-century empiricists, in the *Protokolsätze* of the early Logical Positivists, in the metaphysical psychology of Gustav Bergmann and his followers, and in the new mixture of philosophy and cognitive science of late.

If anything could dispel the mistaken apprehension of empiricism as at heart a theory about the source of knowledge, it should be the history of how the tradition continued by reconceiving itself in response to criticism. This I related as best I could in chapter 2 and the appendix of *The Empirical Stance*. As had been forcefully pointed out by later historians, the slogan formulation of empiricism was a construct of nineteenth-century historians, intent on placing narrative dialectical structure on modern philosophy. That formulation ran into difficulties already when trying to fit Comte's positivism into its procrustean bed. But the more significant factor is the way in which twentieth-century empiricism matured in a way that is responsive (even if not very consciously) to the critiques of the British idealists as well as those of Husserl, James, Dewey, Wittgenstein.

In my own proposal for a characterization of empiricism (beginning with *The Empirical Stance*, pp. 36–8, 46–8, 62–3) *experience is not mentioned at all*. If I had succeeded in all I set out to do, the 'sole source of information' slogan about what experience is would not be discussed any more in connection with the formulation of empiricism.

In the appendix I suggested that the new self-understanding of empiricism had emerged most clearly in Reichenbach. Questions framed in the terms of metaphysics and traditional epistemology are *transposed* to the area of methodology. There is something to be said about scientific practice that is reminiscent of that infamous slogan about experience, so unjustly saddled on empiricism by its detractors. Theory choice in the sciences involves a variety of values and criteria, but what can be accepted as data is very strictly circumscribed in what counts as good experimental design. That is where we can find the cash-value of the claim 'scientific knowledge is based on experience.' But that insight is completely lost if we then confuse this claim with a factual statement about individual psychic events and human learning—subjects in the domain of the empirical science of psychology, not to be broached in the armchair.

3.3 Mohler and Jauernig

Empiricism today should not be saddled with the confusions that surrounded the notion of experience. There is a temptation to do that, because as I said above, a tradition is always identified by its opponents in terms of its past mistakes. The temptation continues with an insistence that the tradition should 'make good' on its mistakes—but in this case it can do so only by

wholeheartedly abandoning them. I may have strengthened the temptation unfortunately just because I gave that slogan as an example of what E+, the doctrine of the naive empiricist, could be. This may have given the impression that I took that slogan to make sense, even though it appears there as premise in a *reductio*. That was a tactical mistake, all the more since my '*reductio*' did not hinge on the content of E+, but on its role.

Anja Jauernig follows her focus on the slogan in the early parts of her chapter with an attempt to construct the *sola experientia* rule as a sort of substitute for the thesis that experience is the sole source of information, surprisingly while still noting the precise slippery, quasi-political, dual role that I ascribe to that 'rule'. I tried there to follow Reichenbach's example of transposing traditional issues of metaphysics and epistemology to ones in methodology. Such a transposition is of course motivated by the ideas that philosophical will o'the wisps do have their origin in real problems that have occurred in practice, and that we gain insight by returning to those roots.

Chad Mohler, who notes my argument in 'Against Naturalized Empiricism', nevertheless maintains the conviction that such a 'thesis' concerning experience makes sense, so that it can be assigned a high or low probability. But my critique of such a thesis in 1995 began with the insistence that every noun in the slogan—experience, source, information—harbours a nest of philosophical misconstruals. I will stand by my earlier arguments on this point. When I ask what empiricism can be now, I am asking at the same time how we can conceive of the empiricist tradition from our present vantage point, and the answer should not embroil us once more in the old confusions (and my own!) about experience and knowledge.

I will return to their arguments below.

3.4 Ladyman (and Nagel)

In the latter parts of his contribution Ladyman explores a challenge that Jennifer Nagel (2000) posed the year before I gave the *Empirical Stance* lectures, in her 'The Empiricist Conception of Experience'. The first half of Nagel's brilliant essay continues the deconstruction of eighteenth-century empiricism; the second half addresses my struggles in the 1990s that led up to *The Empirical Stance*.

Although Nagel refers to the concept of stance empiricism introduced in 'Against Naturalized Empiricism', she focuses on the point in 'Empiricism in the Philosophy of Science' where I identified empiricism still as an epistemological

thesis about the ‘source’ of knowledge. She cites my acknowledgement of the need for the subject to distinguish genuine experience, that can play the role of such a source, from surrounding private events such as dreams and hallucinations. Ladyman echoes this when he begins to discuss her paper: ‘Rather than address the question of whether we can know by experience whether it is true that experience is the sole source of information about the world, I want to briefly pursue Jennifer Nagel’s question: how can we tell what experience is?’

The first half of Nagel’s paper, which concentrates on Locke, I see as a valuable extension of Green and Grote’s massive critical introduction to the 1874 edition of Hume’s work (see *The Empirical Stance*, p. 118). I see that part of her essay as, in effect, strongly supportive of the deconstruction of the sort of empiricism-fallen-into-metaphysics of which she still found many traces in my writings—which I hope I managed to shed by the time of *The Empirical Stance*.

What of that challenge to show how genuine experience is to be distinguished by the subject from its imitations in dreams and hallucinations, without recourse to a priori knowledge? Nagel depicted me as facing the problem she could pose for eighteenth-century empiricists:

As with Locke, however, our question for van Fraassen is not about what happens after we are aware of the actual phenomena or in possession of the deliverances of experience, but rather about what (if anything) we must know to gain knowledge from experience in the first place, or how we come to believe in the actual phenomena as opposed to the products of imagination or illusion. (p. 357)¹⁹

It does not seem to me to apply once we let go of the notion of experience as psychic events but rather use ‘experience’ in its common sense of what happens to us that we are aware of. If I really wonder whether I am dreaming, as can indeed sometimes happen, then happily there are other people around to help me.

The *private psychic event* picture of experience came hand in hand with a philosophical fiction about the self or mind disconnected from the ‘external world’—what we could call a *Robinson Crusoe Ego*, an ego situated on an island of images that might or might not resemble anything beyond the horizon, and

¹⁹ Some of what she quotes, to show that I favoured a certain answer here, was actually part of arguments meaning to destroy such a conception, and presumably not sufficiently well signalled as such.

no way of checking whether they do. This is metaphysics, and when coupled with the traditional demand in epistemology for justification, it issues in a problem that is unsolvable by design:

Justify the opinion you have about how things are in the world. You have nothing to draw on except your own opinion (for what you think others have told you, or what has happened to you and your acquaintances, that is also just your opinion). But your justification must not be circular!

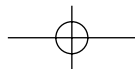
Surely all this was left behind in twentieth-century philosophy. Common sense realism, and a rejection of the demands posed by traditional metaphysics and 'defensive' epistemology, are not themselves part of a special philosophical position, but just a refusal to submit to contrived, unrealistic demands.

Common-sense realism is enough, no metaphysics is needed, to reject sceptical problems. We have the ever constant task of managing our opinion, but always as already at sea, starting from where we are.²⁰ True: when doubts arise, reliance on our own opinion is definitely a decisive attitude, and subject to self-scrutiny. Critical reflection on what we have is in order as well, but for that too we must and do rely on resources we count as reliable though defeasible: our memories, the testimony of others, new experience in settings we seek or construct to test our ideas. Here 'experience' has its ordinary use, as it has in the ancient doctors' insistence that they relied on the accumulated experience of their profession. The nearest we come in realistic philosophy of science to a *real* use of the '*sola experientia*' rule is the deliberative appeal to what has been observed in *reproducible* circumstances, either to reinforce held opinion or to challenge it, as the case may be.

4. Stances

Chad Mohler argues that stance empiricism succumbs to the same *reductio* as naive empiricism. Anja Jauernig at one point endorses Mohler's main argument, but argues that if properly reformulated, even naive empiricism does not succumb. Anjan Chakravartty argued, as we saw above, that stance empiricism lands itself in metaphysics after all; so does Jauernig, though on

²⁰ The question 'but how do we acquire this starting point?' I can only understand as an empirical question, concerning how children learn; that should be addressed by scientific investigation.



different grounds. At a number of points above I have been arguing for a role for values in epistemology, and value judgements and their kin are crucial to the identification of a philosophical stance. Dien Ho and Jauernig argue that this lands the view of philosophy as stance into a debilitating relativism—as Ho writes, rational argument seems to go by the board, for we can only ‘bribe, confuse, seduce, threaten and beg our opponents into changing their value-commitments’.

Together these make for an overwhelming critique, and I might seem to have no options beyond unconditional surrender or a desperate attempt to refute the arguments line by line! Let me see instead if I can unearth some presuppositions of their critiques that I could reject.

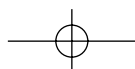
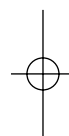
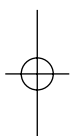
4.1 On Naive Empiricism: Jauernig

One criticism which occurs almost in passing in Jauernig’s contribution is one that I find serious and must endorse. When I added

- (c) ‘as in science so in philosophy’: disagreement with any admissible factual hypothesis is admissible (*The Empirical Stance*, p. 43)

to characterize naive empiricism, I already made it violate Principle Zero (that the position can be identified as a factual belief), and thus made it a stance. Jauernig takes this in stride. She tries to blunt this objection by saying that (c) can ‘derive’ from the dogma E+. I do not see how such a methodological commitment can derive from a factual belief, except in the context of other commitments, for example, to a certain purpose or accepted criterion. The best I can say in defence is that it is surely telling that I could not formulate even a naive empiricist position without straying beyond a factual thesis. Of that factual thesis alone we can say that it is just the sort of metaphysical thesis that the empiricists reject—in the context of typical empiricist attitudes, this thesis self-destructs.

This admission on my part should open the way to attempts to formulate a naive empiricism not vulnerable to the *reductio*. But we may note three things. First, those of Jauernig’s reformulations that might hold some promise all have something like (c)—they are stances. Secondly, with the senselessness of ‘Experience is our only source of information’, I can see no plausible candidate for E+ at all that could make the resurrection of naive empiricism attractive. Finally, if (c) is dropped then it is blatantly obvious that the position has no way to distinguish itself in philosophical practice from the metaphysics to



be criticized. In that case it can only oppose factual statements to factual statements. So the conclusion I would draw from my own failure here is that even at the most naive level, empiricism cannot keep itself from stance as opposed to dogma.

4.2 Dogma Still Present: Mohler

But Mohler argues then that the other ingredients in a stance bring factual beliefs with them, and that although those can vary, some are indispensable to empiricist positions, hence can be targeted by the *reductio*. As Mohler notes, a similar objection had been raised by Ladyman, and I had replied that the beliefs in question are not the basis for the value judgements and commitments but only 'pragmatically brought along' by them. To Mohler's argument this makes no difference:

The required beliefs provide the 'dogma' whose contraries the empiricist must regard as both inadmissible (insofar as they contradict dogma) and admissible (insofar as the contraries are themselves factual hypotheses). Even the stance empiricist is vulnerable to this kind of inconsistency.

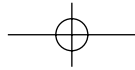
Perhaps I can put my critique of naive empiricism very briefly like this:

If incompatibility with beliefs involved in the empiricist position is the basis for its critique of metaphysics, then this amounts simply to the assertion of a factual statement of the same sort as the metaphysicians' (and with no greater admissibility) so amounts to another bit of metaphysics.

But for the naive empiricist there isn't really anything else to do, is there? Listening to the metaphysician s/he will retort that we are so constructed that and so on, and so on, and that therefore we cannot have the knowledge the metaphysician claims to attain.

Mohler is right that if the stance empiricist continues in the same way, s/he'll be in the same boat—even if simultaneously expressing attitudes that discount metaphysical theorizing or value judgements that rank it as worthless.

Mohler's own response to this problem is to advocate a sort of position that I would call naturalized empiricism. He makes a good case for its tenability and consistency. But I doubt the *stability* of such a position. As an example that instils this doubt, I can point to Quine's vacillations in just this respect. At one point in *Pursuit of Truth* it seems that advice against telepaths and soothsayers is all that remains for Quine of empiricism (p. 19), conceived of as a thesis about



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how we can have factual knowledge. But a little later he indicates that (as a good empiricist?) he can't be certain that this is good advice (p. 21): even the most outrageous factual hypothesis about cognitive processes can't be ruled out on philosophical grounds.

In any case, my own response to the problem Mohler poses is not the same as his. A stance empiricist does not level his critique of metaphysics in the same fashion as the naive empiricist. The basic critique by a stance empiricist is not that the metaphysical theories are incompatible with factual beliefs involved in the empiricist position. To see the elements of the stance empiricist's critique of metaphysics, look rather at these responses to the metaphysician's lecture:

rejection of the demand for explanation at some point, dissatisfaction with explanation-by-postulate, disdain for factual assertions 'from the armchair' invulnerable to empirical test and even more for baseless pretense to a pursuit of truth.²¹

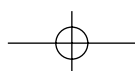
If we now look to science as example, we see there no analogue to (c) in the area of such value judgements relating to practice (cf. *The Empirical Stance*, p. 48).²² The tolerance for diverging factual judgement, which comes for the empiricist along with his lack of any sense of a priori knowledge, or even of what that could mean, does not extend to admitting contrary norms or opposite value judgements in the 'enterprise of knowledge'.

Now, of course, we are going to hear the objection that this form of critique is not one that 'plays' in philosophy, that rejection, dissatisfaction, and disdain cannot play the role that factual contradiction plays.

The conception of philosophy, and perhaps of rationality itself, behind this objection is not at all incontestable. Neither form of critique can stand on its own legs. If I point out that your beliefs are in contradiction with mine, you can retort 'so much the worse for yours!' If I express an attitude or value judgement that pronounces negatively on what you are doing, you can retort 'so much for your perverted sense of values!' These retorts are in perfect parallel, so far. In both cases, I have recourse. In both cases I will bring evidence

²¹ 'Disdain' is provocative, but I intend no offence. It seems to me that an empiricist position must involve disdain for certain practices and values abroad in philosophy today, but that does not imply disdain for one's opponents. It does certainly imply an attitude that will strongly constrain one's own efforts.

²² Admittedly, the distinction can be blurry: to reject a purportedly factual statement on the basis that it does not come with any clear content involves factual disagreement, on a semantic level, but is sure to involve insistence on certain norms for making sense. See further my note in the section replying to Chakravartty.



to support my way of seeing the matter. In both cases, no evidence will have any weight for you unless it links significantly with something you think (i.e. believe; respectively, value).

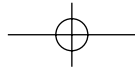
4.3 Values and Objectivity: Jauernig, Ho

So now, as you will have realized, we are at the main point of attack by Jauernig and Ho. If philosophical positions are stances, how shall we reason with each other? What recourse could a metaphysician have in face of disdain for his/her enterprise, for example? What recourse could an empiricist have in the face of great value placed on explanation, even if it involves postulates that are not subject to empirical test? In response I want to make three points.

First, the offered view of this situation seems to me governed by a simplistic dichotomy. Secondly, as I began to outline at the end of my reply to Mohler, there is a parallel between value disagreement and factual disagreement: reason to despair in one case would be reason to despair in the other, but in fact there are precisely similar possibilities for dialogue in both. A debilitating relativism and a shrill shouting dogmatism are the two absurd extremes in either case. Thirdly, I agree that mere pointing to a possibility of dialogue between stances will not serve me sufficiently. We need to articulate a conception of values that allows one simultaneously to maintain one's own while truly understanding the others'. I cannot articulate that conception sufficiently at this point. But this is not a task only for stance empiricism; the need is one we all have.

Let us then first look at how Jauernig and Ho depict the topic of value judgement. Jauernig says quite rightly that a philosophical critique cannot just 'consist in the voicing of a personal sentiment of the kind "I don't like metaphysics"'. But then the two options she outlines are a 'non-cognitivist view of value judgements, that is, a view according to which value judgements . . . are to be understood as expressions of personal preferences or sentiments', and the 'cognitivist' view she depicts as holding that those judgements are factual, either susceptible to empirical investigation or metaphysical. Any of these options, she argues, will lead the stance empiricist who holds them into an untenable position as critic of metaphysics.

There may be sophisticated theories of value that go under those names but these are surely not. Even the logic of value judgement does not allow for equation with personal preference: we can quite consistently express personal preferences at odds with what we admit to be real values, or make negative value judgements about our own preferences, even while expressing



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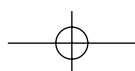
a preference to maintain those very preferences. Nor does it help to let the connotations of such a word as ‘fact’ determine what the alternative is to be. Once again the logic is different: a person may land in an irresolvable moral conflict, where both ‘ought’ and ‘ought not’ are in force, without defect in reason, but not in contradictory factual beliefs that are both true. If the judgement *that explanation is valuable, or to be valued above empirical testability when we form our beliefs*, makes sense at all, then it is not an expression of merely personal or even communal preference. Neither is it of a piece with the factual report that there are explanations, or people who value explanation—not because it is not right, or could not be right, but because the norms that govern value judgement are not the same as those that govern reportage.

Think here also of the difference between stating and expressing. I can state that I have certain values, or that someone else does; but I can also express my value judgements. Only the value judgements that I can express as my own play a role in my acting, judging, planning, practice. In this respect my value orientation is like my opinion and intentions, and this sets the value and opinion discourse apart from talk about facts. We can state facts, but there is no literal sense of ‘express a fact’.²³ The point about expression, and the role that only the values, opinions, intentions, and aims we can express as our own can play a significant role in our practice, may also abet the confusion with preferences, which can equally be either expressed or attributed. But this similarity provides no good argument for equating values, opinions, intentions, aims, and preferences.

I think I may reject the simplistic dichotomy at this point, and thereby the frame of that critique. True, I do not have a position in value theory of my own, to do justice to that subject as a whole, nor do I subscribe to an extant such theory in the literature. (For a bit more, see the last part of my 1993b.) The very clear difficulties that beset someone who holds to that dichotomy are enough for its rejection, to go on for now, it seems to me.

What now of discourse between the empiricist and the metaphysician? Jauernig writes ‘the stance empiricist’s critique will ultimately bottom out in the assertion of a value judgement that the metaphysician doesn’t share, namely that metaphysics is not worth spending any time on.’ Dien Ho’s

²³ In philosophy of language the term ‘express’ has been appropriated for another use which may obscure this point. A statement is said to express a fact or proposition. So we could introduce by definition the phrase ‘X expresses the fact that A’ as meaning that X makes a statement that expresses that fact; but this is contrived and artificial.

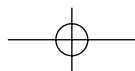


argument for far-reaching, even disconcerting, consequences of the idea of philosophy as stance seems to rest on a similar view of value conflicts. He writes: 'A conflict is in principle resolvable if it concerns matters of facts. Value conflicts, on the other hand, are typically thought of as being potentially unresolvable' and 'If a given disagreement between stances consists of disagreements in values, then what possible good can philosophical dialogues do to help resolve the conflict?'

4.4 The End of the Affair? Ho

Ho continues with Hume's argument that 'ought' cannot follow from 'is'. With the terms taken sufficiently narrowly, I agree: Oughts only from oughts, necessities only from necessities, probabilities only from probabilities, values only from values, empirical predictions only from empirically contentful premises . . . We can't go anywhere except by starting from where we are, and if our starting resources are deficient in a certain respect we 'can't get there from here'. Surely this is not an insuperable obstacle to either controversy or agreement? We do have factual opinion and values already, we mariners already at sea, confronting difficulties to be resolved now. The parallel in this respect is complete. If we have a factual disagreement, we can settle that just by looking, *but only provided* our background beliefs are such that what we see settles it. If we have a value disagreement, we can settle that too by just looking, *mutatis mutandis*; namely, provided our background values are such that what we see settles it.

Does this apply to philosophers, even to empiricists and metaphysicians meeting in the forum? I think so: for any disagreement, we need to look to the facts on the ground. Look, the empiricist says to a given metaphysician, how your basic principles concerning substance, causality, and interaction have led you into fruitless hidden-variable mongering. The metaphysician stares helplessly at the mess, and suddenly recognizes a value that s/he has held all along, about what brings valuable understanding and what does not. Or, look, says the metaphysician to the naive empiricist, how you built everything on a notion of experience while having no theory of experience at all, and while not being able to have either a scientific or metaphysical substitute for such a theory that could serve your purpose. The naive empiricist could stonewall, but may very well see the point of the value judgement, that a position resting on something familiarly named but extrapolated, without sufficient explanation, to a role no familiar notion can play, is not for a philosopher



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worth having. So s/he becomes a stance empiricist. Such dialogues, in which one party becomes convinced by the other, are possible. In the case of factual disagreement they require shared factual opinion, and in the case of value disagreement, shared value.

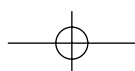
But for Ho as for Jauernig, what looms large is confrontation with others who do not share relevant values. What can we do then but ‘bribe, confuse, seduce, threaten, and beg our opponents into changing their value commitments’? Well, what about showing them possibilities in the human condition they had not already apprehended? What about opening new vistas for them, about what the world is or could be like? Why this scepticism about human communication that would make it inconceivable that we can show for example metaphysicians how attractive empiricism is, just as we can show people who grew up quite differently just how attractive a life of charity and tolerance toward all can be?

Actually I think Ho does not overlook this. Commenting in conclusion he writes ‘Philosophy cannot tell us what to do. We, as a community of philosophers, must *decide* what to do. It is, I think, in this sense that van Fraassen is correct when he says “Welcome to the real world.” In the real world, we do make decisions about a particular practice not guided by the rules of the practice but instead by certain extra-practice considerations (humanism, love, beauty, simplicity, pragmatic reasons, and so on).’ It is precisely in this reflection that, it seems to me, Ho announces the true upshot of the argument, the insight that there is nothing in reason that can take the responsibility from our shoulders, that decision is finally involved in the shaping and management of our value-orientation in the same way as in our factual opinion.

5. Reconciliation on Metaphysics

Jauernig ends her chapter with a conception of philosophy and metaphysics that is certainly challenging, but still not so far from my own—perhaps the core of our views is the same but rendered in a different key.

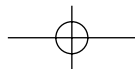
We do not have the sense that we understand things unless we can fit them into a coherent picture, a narrative obeying the dramatic unities, a unified structure, with the ingredients held together with strands of explanation. To all of that I agree, but I qualify it as follows. Our desires for explanation, coherence (in the sense pertinent here), unity, and the like are constituted in



part by norms that determine what counts as success, and have presuppositions about what things can be like. Both those norms and those presuppositions are defeasible, are historically revised, generally under the pressure of cases where we do not find those desires satisfied. To give one example, Reichenbach struggled to show that a world which is not deterministic is still intelligible, that we can have a remaining sense of understanding if we are not able to picture the world as a deterministic system. But he offered a weaker norm, that phenomena should always be fitted into common cause models. Ought implies can, so this norm presupposes that this is always possible. In his own writings on quantum mechanics you can already see doubts concerning this presupposition. Later we came to see that it may fail; at best, it can be maintained only at the cost of violating even deeper criteria of intelligibility for our understanding of nature. So I rank such presuppositions, when they still have a grip on our thinking, with Kant's Illusions of Reason. And I see the pursuit of satisfaction for such desires as to be viewed as a pursuit of interpretation, not a pursuit of truth. But here we are at a point of contact with Jauernig's conception of the matter.

The metaphysics I criticize and reject is specific in character, and does not nearly exhaust all that our history has seen in metaphysics, where we can witness towering achievements, as Whitehead said, views from the mountaintop. So in *Laws and Symmetry* I wrote that I lacked 'sympathy for metaphysics, though not in general: only for pre-Kantian metaphysics—and then only if practised after Kant' (p. viii). What I was referring to I made clearer in *The Empirical Stance*, again with that qualification: 'As I see it, analytic philosophy—which is the strand to which I belong—began with a revolution that was subverted by reactionary forces. I am speaking here of reversion to a seventeenth-century style of metaphysics. I do not reject all metaphysics, but this reversion I see as disastrous' (p. xviii).

My take on the value of metaphysics is thus that its valuable core is not a search for truth about what the world and we are like, but for *interpretation*. In my 2003 reply to McMullin I spelled out how I see this search in the context of philosophy of science. When we think of the theories and models science gives us as representations, all the questions posed in ontology do arise—such as questions about substance, individuation, causality, haecceity—but in a new key. The concepts that appear in the answers given in interpretations of a physical theory such as quantum mechanics belong to a conceptual framework drawn from metaphysics. But on empiricist lips they are answers



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to questions *not about nature, but about our representation of nature*: answers about nature as represented, not about nature. We should not look at this task only within the narrow confines of technical philosophy of science. I find myself coming much closer to Jauernig's conception when I take a wider look:

In every century again we must interpret ourselves to ourselves. We do not come into our century with a *tabula rasa*. We must interpret what we find ourselves to be, with an eye to what we have been *and* to what we could be and can be. That is the perennial, ever recurring task, ever new. What we find includes both science and religion, the secular and the spiritual—and what we transform in our reinterpretation includes contrasts and boundaries between the two. (*The Empirical Stance*, p. xvii)

That is not so far from her 'the project of making the world and ourselves intelligible to ourselves' and 'metaphysical theories . . . not presented as true but as metaphysically adequate, that is, as possible and explanatorily or intellectually satisfying'—'constructive metaphysics'. Her 'Let a thousand flowers bloom!' echoes my insistence that while understanding is the aim, we understand better with every interpretation we find that we can accept as adequate.

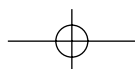
There may be a lacuna just here, however. On the ultimate value of engaging in metaphysics Jauernig writes that

what is at stake is our peace of mind—Spinoza would agree with me here—and what we gain is the satisfaction of our natural desire for a complete description of how things might be, which is integral to a real understanding of the world and the human condition.

Is letting a thousand flowers bloom consonant with this suggestion? Would that peace persist once that natural desire, satisfied by one answer, is confronted with an equally adequate rival answer?

A similar suggestion in defence of such a conception of metaphysics was made by Alicia Finch in her review of *The Empirical Stance*:

it is not the case that whenever the ontologist makes an inference to the best explanation, truth and falsity are the only values that are at stake. Other values come to light when we consider that at its best, analytic ontology draws out the implications of a metaphysical theory until it reveals an implication that is of crucial significance to the way we live our lives. In these cases, what is at stake is our conception of who we are and how we ought to live; and we bet on one theory rather than another because it is the theory that best allows us to cling to that conception. (Finch 2003: 303)



Is that not a search for a best theory that brooks no rivals, if what is found is meant to play that role?

I do not ask this question because I disagree fundamentally, but because I want to add something. I want to emphasize an existentialist theme. Just having such a world-view on the books, even if seen as an adequate or even the most adequate available interpretation upon our existence, cannot suffice. In the end the sense things have for us comes from a decision to accept something as giving them sense.²⁴ The element of decision, how it rests on our own shoulders, the responsibility for the very meaning that things have for us, can't be escaped, and the work of the intellect, sufficient for theoretical decisions, can't suffice.

Interpretations are placed on texts, but equally on actions, practices, behaviour, natural events. Interpretations can be tested: some interpretations are in better accord with their targets than others (no great matter whether we classify such testing as empirical or not). If I agree that to make something intelligible requires an interpretation which comes in propositional form, and Jauernig agrees that the propositions that go beyond all possible experience are not thereby put forward as true, then we are not far apart. Although I won't grant overriding priority to, or the absolute need for, such intelligibility, I won't resist the conclusion that an empiricist stance involves metaphysics as well, if we can see this project as the good way to engage in metaphysics.²⁵

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²⁴ If to accept a world-view as giving sense would involve having metaphysical beliefs, we would be once more in an impasse; but I do not think that is what was meant.

²⁵ The author gratefully acknowledges support by the NSF through SES Award 549002.

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