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# Pragmatism and Philosophy of Science: A Critical Survey

Robert Almeder

*After delineating the distinguishing features of pragmatism, and noting the resources that pragmatists have available to respond effectively as pragmatists to the two major objections to pragmatism, I examine and critically evaluate the various proposals that pragmatists have offered as a solution to the problem of induction, followed by a discussion of the pragmatic positions on the status of theoretical entities. Thereafter I discuss the pragmatic posture toward the nature of explanation in science. I conclude that pragmatism has (a) a generally compelling solution to Hume's problem of induction; (b) no specific position on the status of theoretical entities, although something like the non-realism of the sort developed by van Fraassen seems a defensible candidate for most pragmatists in general, even though there are non-trivial objections to van Fraassen's position; and (c) central to the pragmatic conception of scientific explanation is the abandonment of our common conception of truth as a necessary condition for sentences to provide adequate explanations, and a drift in the direction of a contextualist account of explanation.*

## 1. What Is Pragmatism?

Typically, pragmatists believe that the rational acceptability of any proposed belief is *ultimately* a function of whether the belief in question is likely to be the best available instrument for our successfully informing our behaviour in the interest of adapting to the world in some fundamental way under the principle of *homeostasis*. Either that or they believe that the rational acceptability, or justification, of a belief is ultimately a matter of its being produced by a method or process generally reliable in producing beliefs permitting that sort of successful adaptation. So characterized, sophisticated natural scientists can be, and have been, pragmatists simply for believing that the fruits of standard scientific methodology generally tend to produce beliefs better by way of providing suitable predictions for biological success than any other available

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method. That sort of success, they can say, justifies the methodology of natural science and serves simultaneously to indicate the goal or fundamental purpose of science (Feyerabend 1973).

But some pragmatists, such as Charles S. Peirce, William James, John Dewey, and their respective admirers, go further than simply adopting the inductive and deductive methods of the natural sciences in order to determine the epistemological reliability of beliefs about the physical world. They add that, under certain conditions, there are some proposed beliefs that we can neither establish nor refute under the deductive or inductive methods of testing and confirmation in the natural sciences. Such beliefs are, nevertheless, epistemologically justified and hence rationally acceptable because when *adopted as true* they directly or indirectly produce behavioural or epistemological consequences that provide for suitable adaptation under homeostasis, or natural selection, just as efficiently as beliefs forged under the anvil of the inductive/deductive methodology of natural science. Like the beliefs established in good science, those beliefs also have a right to exist under the same general rubric that justifies the beliefs emerging from good scientific practice.<sup>1</sup>

Generally, then, pragmatists distinctively adopt the position that some beliefs about the physical world are epistemologically justified under certain conditions, even when they do not admit of any explicit or direct justification by conscious appeal to inductive or deductive inference from antecedently known or justified beliefs. Those conditions obtain when, after exhaustive reflection and inquiry, the proposition proposed for belief has no currently discernible empirical or deductive evidence against our accepting it, and when there is some reason for thinking that the belief, if adopted as true, would have a tendency to produce consequences providing either cognitive or moral utilities that would not otherwise obtain if one had not accepted the belief in question. One way to express more succinctly this defining pragmatic principle (PP) is as follows:

- PP     A person will be rationally justified in accepting a proposed proposition *P* as true if
- (a)     After exhaustive research, there is at that time no currently available conscious inference, either inductive or deductive, from other antecedently known or justified beliefs that would either confirm or disconfirm the proposition *P*; and
  - (b)     There is some real possibility that accepting *P* as true, or very likely to be true, will have a tendency to provide behavioural consequences more productive of cognitive or moral utilities than would be the case if one had accepted instead either the denial of *P* or nothing at all.

Stated differently, as long as there is no compelling evidence either for or against accepting *P*, and as long as accepting *P* could provide cognitive or moral consequences that would tend to better the world or believing subject, more than if the subject were to disbelieve *P* or believe nothing at all relative to *P*, then one would be justified in accepting *P*, even if *P* is ostensibly a proposition about the physical world.

As an application of PP, for example, most pragmatists, as we shall see, are sympathetic to accepting the inductive method as a reliable way of providing justified beliefs about the world, simply because, while there is, as David Hume taught us, no deductive nor

inductive justification for the validity of inductive inference, nevertheless, there is no good reason not to accept it either, because it generally tends also to produce beliefs that allow us to predict precisely our sensory experiences and thereby permits the production of other beliefs whose adoption and applications allow us to navigate more successfully under the principle of homeostasis. If we are uncomfortable with calling this a *justification* of induction, we may, if we like, agree with other pragmatists that this is at least a non-circular *vindication* or *validation* of induction as a source of reliable belief.

Similarly, William James, among others, came to believe that there was nothing at all wrong with accepting the proposition that human beings have freedom of choice, even though that proposition could not be established inductively or deductively in natural science. He accepted it because he believed that that particular belief, once accepted, is more likely than its denial to produce the motivation necessary for the strenuous moral pursuit necessary for making the world a better, and hence a happier, place. He assumed that somewhere down deep we all unaccountably feel a strong moral responsibility to ameliorate the human condition, as a necessary condition for attaining deeper human satisfaction or happiness (Almeder 1986). At any rate, those who deny PP for whatever reason will deserve the label non-pragmatist.

Whether we call such latter propositions authorized under PP 'common sense beliefs', 'fundamental inference tickets', 'useful posits', 'working hypotheses', or 'beliefs beyond doubt' is incidental to the basic point that one has every epistemological right to accept such propositions as true in the name of human rationality because such acceptance tends to produce ways of acting more conducive to cognitive and biological adequacy than if one had rejected them or had believed nothing at all instead. Of course, at a later date such beliefs may turn out to be rejected on the occasion of new evidence or new ways to interpret existing evidence. But that is a liability affecting all beliefs about the physical universe.

Invariably, pragmatists have also been fallibilists and verificationists relative to beliefs about physical objects and the physical laws governing them. Of course, there are fallibilists and verificationists who would reject PP and thereby reject the label 'pragmatist', probably because they too often mistakenly think that pragmatists, in virtue of adopting PP, generally hold that beliefs are true, or rationally justified, if it merely pleases them to think they are true, no matter what the facts may be. Fallibilism, at any rate, is the view that no matter how much evidence one has at any given time for one's beliefs there is always some real probability greater than zero that one is in error with regard to some of them. In other words, however well-confirmed one's beliefs about the world may be and however confident one may be in the truth of one's beliefs about the world, they are always subject to truth-value revision, pending their adequacy as predictive and adaptive instruments in the face of new and changing bodies of evidence or rules for interpreting the evidence. Where such beliefs may not be directly verifiable or consciously derivable from other known propositions under the canons of confirmation in natural science, most pragmatists, along with Peirce, James, and Dewey, justify those beliefs as revisable postulates, or inference tickets, when there is a common and ineluctable inclination to accept them and no good reason at the moment to reject them. Although fallible, such basic beliefs are rationally acceptable for their producing

consistent consequences promoting adaptive or applicative success by way of providing for cognitive utilities or even forms of happiness, based on the predictable results of accepting such beliefs as true, even if they were not systematically testable and confirmable under the standard methods of science (Almeder 1986).

With regard to verificationism, although there are stronger and weaker versions of verificationism, beginning with Peirce's pragmatic maxim, pragmatists can be seen to adopt the general verificationist view that one does not know the meaning of a proposition if one has no conception of what one should take as sufficient evidence for confirming the proposition.<sup>2</sup>

Finally, most classical pragmatists along with many other contemporary or recent pragmatists, agree that when it comes to propositional knowledge about the external world, the truth or justification of a belief is less a function of how the belief is caused or originates from experience than it is of whether the belief, however it is caused, leads to what the belief predicts at the sensory level under the assumption of its truth. This particular feature of pragmatism is what James christened 'radical empiricism', in contradistinction to Humean empiricism, when he asserted that it is in the fruits of our beliefs, and not the roots, where the truth resides (James 1907, 1909; Tiercelin 2005). These then are the major common traits of pragmatism that together distinguish pragmatists from non-pragmatists.

Not surprisingly, incidentally, pragmatists continue to spend much time responding to the mysteriously enduring anti-pragmatist objection that knowledge requires truth, just as highly justified belief requires strong truth-conduciveness, and neither is plausibly defined or construed solely in terms of the appropriate instrumental utilities accruing to the acceptance of a belief if the justification of a belief can be achieved independently of its having test conditions of the sort usually required in natural science.<sup>3</sup> There is, the anti-pragmatists say, a world of difference between believing what it is best for us to believe in virtue of the predictive implications (either psychological or social) of our beliefs and believing the truth or what is likely to be true, and the goal of inquiry is to find the truth or what is likely to be true, rather than what it is best for us to believe in the way of survival in this cruel and unforgiving world.<sup>4</sup> The way pragmatists respond to this objection permits us to distinguish briefly between two further types of pragmatist.

The first type of response, advanced by Richard Rorty and others, consists in affirming that the objection in question assumes that truth, as we ordinarily understand it, is certifiably attainable, that we can, at least from time to time, decisively *show* which of our beliefs are true in the platitudinous or common sense of 'true'. But this, says Rorty, we cannot do, and so truth, as we ordinarily understand it, is a myth, no less than any concept of knowledge that would require either truth or the strong likelihood of truth as a necessary condition for knowing (Brandom 2000, 2–4). In short, for Rorty and others, the objection would make sense only if we can effectively determine *which* of our beliefs are true, rather than merely justified by appeal to the standards of rational justification present in contemporary discourse, standards by no means certain to prevail for future audiences in different places. Given this response, all we have is justification relative to changing standards of acceptance. That, says Rorty, is all we could

ever mean by 'knowledge', and it requires abandoning truth as a necessary condition for knowledge. Either that or we abandon the concept of knowledge altogether and settle for justified belief as our highest epistemological calling. If we must label it, call this *radical pragmatism* (Brandom 2000, 4–14). It is sometimes described as cultural relativism in epistemology.

The second type of response one often hears to the above objection consists in affirming that pragmatism is at liberty to emphasize the utility of one's beliefs as the criterion for their *acceptance as true* without abandoning the idea that some of them are in fact true. That one's system of beliefs may well allow us to adapt successfully to the world is perfectly consistent with thinking that the reason they have such consequences is plausibly a function of at least some of those beliefs or beliefs implied by them succeeding in correctly describing the world, even if incompletely. Alternatively put, pragmatists have the option of responding, contra Rorty, that even if we cannot determine at any given time which of our beliefs are true in the ordinary sense of true, we can avoid making a mystery or a miracle of scientific progress by urging that the success we so earnestly seek and often find in our theories and predictive hypotheses is there simply because some of the assertions in them or assumed or implied by them do in fact succeed in correctly describing the world, even if we cannot say, or reliably determine, which assertions are doing the work and thereby allowing for the correct predictions.<sup>5</sup> Again, if we must label it, call this *non-radical pragmatism*.

With this admittedly brief and qualified description of what most pragmatists have in common and how they may respond to the most common objections to pragmatism, we can turn now to an examination of proposed pragmatic solutions to the problem of induction, the problem of theoretical entities in science, and the problem of scientific explanation.

## 2. Pragmatism and Induction

Induction is the method of inference by which we infer that all Xs are Ys because all past-observed Xs were also observed to be Ys. Hume pointed out that such an inference assumes that the future will be like the past, or that the unexamined members of a class will be like the past examined members of the class when the number of observations is large. But how do we know that the unexamined members of the class will have the same properties as the examined members, or how do we know that future will be like the past? If we do not know as much, we do not know that any generalization about the world is true or known. Hume claimed that we have neither an inductive nor a deductive justification for believing that the future will be like the past (Hume 1955, IV, ii, 32). Certainly, an inductive justification of induction based on the observation that past futures were like the past pasts is minimally circular for appealing to the inductive justification offered while overlooking the question of how we know that future futures will be like past futures. Also, sometimes past futures were not like past pasts, as Bertrand Russell and many others have clearly noted (Russell 1948, 1960). Moreover, there can be no *deductive* proof that the future will be like the past; it will always be logically possible that the future will not be like the past. Arguing a priori that there is a

principle of uniformity in the world guaranteeing and explaining past successes in predicting that the future would be like the past in specific ways will not help either.<sup>6</sup> At best, that evidence shows only that there *was* a principle of uniformity in the past, when the question is whether such a principle of uniformity will continue to exist in the future. That question cannot be answered affirmatively deductively without assuming that the future will be like the past. Most pragmatists have generally agreed with Hume that any standard inductive justification of induction is viciously circular because it assumes what needs to be proven, namely, that the future will be like the past; and they have also agreed that no deductive justification of induction is possible, because whatever argument one provides in that vein, it will always be possible to imagine the conclusion false, or that the future will not be like the past. Hume, to be sure, did not suggest that we should not be inductive reasoners. Such reasoning has worked wonders in often providing us with successful beliefs. But it does not follow from that that the success we have garnered in the past under such reasoning amounts to *knowing* anything generally true about the world. For Hume, the success of our inductive reasoning is not evidence of our having any knowledge about the world, rather than of simply having useful beliefs, any of which could at any time be false, although not all of them could be false simultaneously.

In philosophy of science, there are at least three distinct pragmatic responses to the problem of induction. The first is Peirce's. The second is methodological pragmatism, and the third is simply a non-methodological form of pragmatism. Let us examine these approaches.

A) Peirce offered the first pragmatic defence or *vindication* of induction. He argued that while inductive reasoning at its best could mislead us from time to time into accepting as true what is in fact false, nevertheless, in the long run and on the whole, the method of induction will sooner or later lead to the Truth (with a capital *T*) on any answerable question. More specifically, and in response to Hume's challenge to justify the method of induction when any inductive justification would be circular and any deductive justification impossible, Peirce granted that in any particular case inductive inference can lead us to false conclusions; but the *method* of induction finds its *vindication* or *validation* as the only reliable method for fixing belief about the world in the fact that in the long run induction will lead the scientific community inexorably to the one destined and irreversibly true answer for any answerable question about the world (Peirce 1931–58, 5.345, 8.12, 2.769, 2.693, 8.43; see also Almeder 1980, 61ff.). But what was his reasoning for this confident assertion, and what kind of reasoning was it anyway?

Peirce's basic argument for asserting that induction will lead to truth was that all reasoning and inquiry proceeds on the general assumption that there is a correct objective answer to any answerable question, one that does not depend on what anybody wishes, wants, or opines, and that inquiry pursued indefinitely long under inductive reasoning will reach this one True irreversible answer (Peirce 1931–58, 4.547, 7.335, 8.12). He thought that if we did not assume as much, no inquiry would go forth at all. Believing in the *general* reliability of induction to lead sooner or later to the Truth was for Peirce something we had to do if we were to fix or establish reliable belief in the



presence of recalcitrant experience under existing beliefs or no beliefs at all. So, his proposed justification for the method of induction was inspired by what he called his *doubt-belief theory of inquiry*. This theory of inquiry, incidentally, he found in the works of the Scotch psychologist Alexander Bain (Bain 1875, 505–507; Peirce 1931–58, 5.375, 5.12, 7.358; see also Fisch 1954), who argued that the whole purpose of inquiry is to overcome the dissatisfaction and irritation of not knowing what to believe when existing beliefs suddenly failed to provide us with the means of adapting successfully. For Peirce the only reliable method that will fix belief is the scientific or inductive method because only it will give us beliefs devoid of wishful thinking and thus provide objectively reliable instruments for predicting future sensory experience (Peirce 1931–58, 2.757n1, 2.769). Without the capacity and method to predict with some degree of precision our sensory experiences our beliefs would not satisfy the end for which we inquire, which is not, according to Peirce, to find the truth rather than to find those beliefs we sincerely think to be true as a result of applying a method that guarantees objectivity.<sup>7</sup> Peirce's doubt-belief theory of inquiry, then, construes the activity of fixing or establishing belief as a biological activity under the principle of *homeostasis*. Evolutionary forces drive us to the method that best enables us to establish beliefs we will not find wanting as adaptive instruments relieving the stress of not knowing what to believe, and only inductive reasoning can do the trick. Is Peirce's defence of induction persuasive?

Depends on how we read it. What are we to make of the claim that all inquiry proceeds on the *assumption* that there is one objective answer to any answerable question and that inquiry conducted indefinitely long by the scientific community using only the inductive methods will sooner or later come to the one irreversibly true answer? The problem seems to be that Hume could readily accept the assertion that all inquiry proceeds on that assumption, and then promptly urge that the assertion itself is an inductive conclusion based on an examination of all past cases of inquiry. That lands us back in the vicious circle of seeking to justify induction by inductive inference, even if all past inquiries may have worked well on that assumption. Either that or Peirce was seeking to avoid the necessity of an infinite regress of justification by implicitly asserting that all reasoning begins with certain assumptions that cannot be justified except by the practical consequences they have for ultimately providing beliefs we generally find suitable for the ends for which we conduct inquiry. But even then, the ghost of Hume will pleasantly note that assumptions are still unjustified assertions, and however intuitively acceptable they may seem, any conclusion based on them will presuppose that the future will be like the past.

B) To this last Humean objection, however, those contemporary pragmatists following Peirce's lead tend to press the Peircean point that unless we start with assumptions we know not how to justify, except to say there seems no good reason to doubt them either by way of their inductive or deductive implications, not only will we end up with nothing at all by way of justified belief or knowledge, we are laying down demands for certainty and implicitly faulting inductive inference for not being deductive inference.

Take, for example, the defence of induction offered by Nicholas Rescher (1977, 2003). Rescher affirms, along with Aristotle, Carnap (1950), Quine (1953), Sellars (1963), and many others, that the first principles and the methodology of natural



science cannot be justified in any non-circular way by explicit and direct appeal to those very principles in question. So much is clear. Even so, Rescher elsewhere asserts that there must be a solution to the problem of induction because, 'contrary to what the skeptic suggests, we do have knowledge of the physical world, thanks to the use of inductive methods. We know, for example, that atoms exist, and this latter bit of knowledge depends on a good deal of inductive inference' (Rescher 1982, 25ff; see also 1984, 12ff). Nor can we establish the validity of inductive methods a priori, as some have argued (e.g., Bonjour 1998). Rather the validation or vindication of induction can, and should be, established *pragmatically*, that is, by directly seeing whether, when an induction is simply adopted, its fruits lead to the observable satisfaction of the primary goals of science in accurate predictions and applicative success of beliefs so formed. If they do, then, although this pragmatic form of justification may indeed be empirical, it is not *scientific* in the sense of proceeding directly from standard testing and confirming whether the primary cognitive goal has been satisfied (Rescher 1984, 12ff). If the skeptic demands more than this for a justification or vindication of inductive reasoning and first principles, Rescher, like Peirce before him, locates the demand, for various reasons, in a sterile Cartesianism feeding on a faulty argument whose conclusion is that all beliefs about the world are truly doubtful, and therefore in need of justification (Rescher 1977, 175–184).<sup>8</sup> This would be to say that induction fails simply because it is not deduction. What can one say about Rescher's proposal and generalized critique of the problem of induction?

Well, those impressed with Hume's argument may well find themselves reluctant to accept the charge that they are advancing a sterile Cartesianism. They will want to know why they should accept that particular criticism. After all, they may say, if there is no non-circular inductive justification for induction, and no deductive justification for it either, what other conclusion can we draw except that the belief that the future will be like the past is more like a touching act of faith or a grand assumption which, however useful it has been in the past, does not provide us with any real justification for accepting induction as a source of knowledge, or even of justified belief (which makes no sense without reference to certifiably true beliefs about the world)? But then again, as we saw, Rescher and several other pragmatists will want to insist that there is something disturbingly inaccurate about the claim that the pragmatic story is going to be simply another inductive, and hence circular, justification for induction. They may even complain that there is not much difference here between the pragmatic story and what Aristotle in fact came to believe about how one would be justified in believing first principles. Doubtless, Hume would respond in kind to Aristotle's proposals and urge that such a defence of first principles amounts to nothing more than an assumption offered arbitrarily to end the infinite regress. At that point, one may well expect the pragmatist to forge forward with a defence of the pragmatic proposal by appealing to some basic form of the PP as explicated above. It is difficult to see how the Humean could reply satisfactorily to such a defence.

C) There is a third pragmatic proposal to solve the Humean problem of induction. It was initially offered by Hans Reichenbach (1938) and Richard B. Braithwaite (1955), and more recently defended by Brian Skyrms (1975), Wesley Salmon (1967) has also

defended it, along with Isaac Levi (1967) and D. H. Mellor (1991). In explicating this pragmatic solution, Skyrms, for example, asks us to imagine that

you were forcefully taken into a locked room and told that whether or not you will be allowed to live depends on whether you win or lose a wager. The object of the wager is a box with red, blue, yellow, and orange lights on it. You know nothing about the construction of the box but are told that either all of the lights, or some of them, or none of them will come on. If the colored light you chose comes on, you live; if not you die. But before you make your choice, you are also told that neither the blue, nor the yellow, nor the orange can come on without the red light also coming on. If this is the only information you have, then you will surely bet on red. (Skyrms 1975, 43)

In other words, if any light will come on, the red one will; but of course it is possible that no light will come on. So, if any bet is successful red will be successful. Skyrms urges then that something like this is true of induction. That is to say, if any method will work for forming reliable (that is, more often than not true) accurate general beliefs about the world, the inductive method will work. As others have noted, the reason why we should accept the view that the claim if anything will work to form reliably accurate beliefs about unobservable things, then induction will do so is simply the frequently observed self-correcting nature of induction. If any method other than induction is found to be successful by way of producing true beliefs about observables and unobservables alike, then induction will sanction it. So, if any method will work, induction will work.<sup>9</sup> What can we say about this self-styled pragmatic proposal?

Hume might well reply that *if* any method succeeds in providing us with true generalizations about the world, the inductive method will succeed. But then Hume might also wonder aloud why anybody would think that such a claim justifies, vindicates, or validates the belief that some method in fact so succeeds. After all, to show that any method in fact reliably provides us with true generalizations about the world will presuppose, and not show, that the future will be like the past. Hume's argument is ostensibly an argument to the effect that we have no way of knowing whether *any method* will in fact provide us reliably with true generalizations, because such a proof would require that we know the future will be like the past. That, of course, cannot be shown either deductively or inductively. Hume's nettlesome *tour de force* was to note that we in fact infer that all *Xs* are *Ys* simply because in the past we have never seen an *X* that was not a *Y* and that the generalization 'All *Xs* are *Ys*' extends to the unobserved members of the class on the *assumption* that the future will be like the past. So, how exactly do Reichenbach, Salmon, Skyrms, and all the other pragmatists who adopt this argument show that the future will be like the past? If inductively, then it will be circular; and it cannot be done deductively because it is always logically possible that the future will not be like the past. In short, for the Humean, it may well appear we could all accept the claim that if any method will work, induction will work, and then affirm that no method will work because that would presuppose a non-circular, non-deductive demonstration that the future will be like the past, when in fact we do not know that the future will be like the past, even if it were true that in the past all futures were like the past.

To this Humean rejoinder, a predictable rejoinder on the part of the pragmatist will be that, as Rescher and most of the others pragmatists listed above have noted, all such Humean criticisms amount to blaming induction for not being deduction; and that is to advance by implication and without justification a sterile Cartesianism. (Rescher 2000, 2001). More can be said about this pragmatist reply to the charge that this third pragmatic proposal begs the question against Hume's argument. But the attractiveness of the pragmatic reply that the Humean problem is rooted in a sterile and arbitrary Cartesianism and its thus freeing the pragmatist from the charge of begging the question against the Humean argument certainly seems to be its way of furthering the vindication or validation of induction in the face of Hume's argument.<sup>10</sup>

### 3. Pragmatism and Scientific Realism

Scientific realists believe that

- (a) There is a world of physical objects whose existence and some of whose properties depend neither causally nor logically on the existence of any number of human minds;
- (b) Some of our beliefs about that world are, even if somewhat incomplete at any time, correct descriptions of that world; and
- (c) We can reliably or justifiably determine and say *which* of those beliefs, including our theoretical beliefs, about this world are in fact the correct descriptions.

Scientific realism shares with classical realism conditions (a) through (c). What distinguishes scientific realism from classical realism is simply that scientific realists extend classical realism to include the existence of theoretical entities postulated to exist by empirically adequate or successful scientific theories, even though such theoretical entities may not be directly observable.

On the question of whether science provides us with correct descriptions of an external world, and in contradistinction to classical realists and scientific realists, there will be positions that fall into the categories of scientific non-realist, or scientific anti-realist.

Scientific non-realists typically assert that while the world may, or may not, be as the scientific realist claims in asserting above conditions (a) through (c), we have no generally non-controversial way of showing that *all* those conditions are satisfied; moreover, for the purpose of satisfying the goal of natural science, we need only generate theories or hypotheses that allow us, through the prevalent canons of testing and confirmation, to predict reasonably precisely our sensory experiences, thereby allowing for maximal prediction and control. In other words, for the scientific non-realist, there is nothing about the success of scientific theories that requires us to accept *all* the conditions (a) through (c). As a result, the scientific non-realist pleads agnosticism on questions such as 'Do successful scientific theories succeed in correctly describing an external world, even when such theories assert the existence of theoretical entities?' or 'Does science provide us with knowledge of an external world, including knowledge of the existence and nature of theoretical entities?' The only interesting question, at least for the

scientist, is whether the theory works by way of providing explanations that allow us to predict the sensory phenomena implied by the presumed truth of the theory. The rest may be grist for the philosopher's mill, however it turns out.

Scientific anti-realists assert that it makes no sense to adopt conditions (a) through (c). Better yet, they assert, for example, that there is every reason to believe that all properties are linguistic in nature, and so the first tenet of realism, that is condition (a), is philosophically indefensible; and if it were defensible, for some reason, neither condition (b) nor (c) would be defensible. Scientific non-realists are willing to think that scientific theories *may*, at their best, provide us with knowledge of an external world, even though they also may not do so, there is no way of knowing, whereas scientific anti-realists assert the stronger position that in fact there are no defensible arguments showing that scientific theories may, at their best, successfully describe an external world. Where, if at all, do pragmatists fit into this picture?

From a purely historical perspective, it seems doubtful that there is a distinctively pragmatic position on the question of scientific realism, or on the ontological status of theoretical entities. Well-known pragmatists have defended different versions of scientific realism, while others have defended different species of scientific non-realism, and others have defended scientific anti-realism. All either claim to be pragmatists or are widely regarded as pragmatists.

Peirce, for example, argued strenuously that there is an external world and that the scientific community *will* come sooner or later to answer correctly every answerable question about that world. This destined irreversible opinion of the scientific community will be the Truth about the world. He defined Truth in terms of correspondence, and he reckoned such truth as the property of the destined final and irreversible opinion of the scientific community. But, for Peirce, some of our current beliefs will continue to be asserted in the final opinion of the scientific community. Indeed, he believed that when we arrive at this destined point in the indefinite future (whether we know we have arrived there or not) the opinion asserted as true in the scientific community will be the final demonstrable Truth about the world. Bypassing problems in Peirce scholarship on this issue, (because some Peirce scholars urge that Peirce did not really assert that in fact the final opinion on any answerable question about the world *will* emerge in the course of time, rather than that it *would* emerge if only we could continue successful inquiry for the indefinite future), it seems that this position offered by Peirce certainly satisfies the above conditions (a) and (b), but does not clearly satisfy condition (c), thereby qualifying him as a scientific realist; but because of its failure to satisfy condition (c), not a classical one.<sup>11</sup>

In the pragmatic tradition we can find classical scientific realists who have argued for conditions (a) through (c). Unlike Peirce, however, some of them think we should *postulate* or *posit*, rather than profess to prove, the existence of the external world: the implications of such a postulate or posit at the level of applicative success more than vindicates condition (a), as there is no good reason against believing that there is an external world and a very strong common sense impulse in favour of it, whereas its denial leads to serious troubles, not the least of which is that everything we take our world to be is some basic mental or human construct. That conclusion, for them, is

horrendously counter-intuitive. More importantly, however, we have no reason against believing in physical objects, and the belief that there are such objects has been and remains maximally productive. Ditto for conditions (b) and (c). Denying that we have any beliefs about and external world that are correct descriptions of an it flies in the face of common sense, just as does the claim that we cannot ever justifiably say which of our beliefs (including the theoretical) are in fact correct descriptions. This latter view is advanced by Rescher (2003), among others. But Rescher does not accept Peirce's proof for the existence of the external world, rather than justifying belief in the existence of an external world as a posit or postulate that we have no plausible reason to reject and that the belief has never disappointed us in its applicative consequences. Nor does Rescher accept the view that the Truth of things will be seen only in some final irreversible theory of the world. On the contrary, Rescher thinks we now have many irrefutable general truths about the world, and that there will never be any final irreversible theory about the world (Rescher 2000; 2001; 2003). But not all pragmatists are species of scientific realist.

Indeed, if we take fallibilism seriously, and then combine it with certain lessons from the history of science, we might well end up as pragmatists endorsing a scientific anti-realism of the sort adopted famously by Richard Rorty, who, among other self-proclaimed pragmatists, urged at one time that truth, at least as we ordinarily understand it, does not exist, because nobody has ever been more than justified in any belief adopted, and even then justification would have been relative to changing social standards of acceptance. Insofar as classical realism and classical scientific realism both require belief in the commonly accepted concept of truth (reflected trivially in Tarski's biconditionals) and in our ability to determine reliably which sentences are true in that sense, Richard Rorty, again among others, made a strong case for being an anti-realist, although claiming to be a pragmatist for the reason of adopting what he took to be the soul of pragmatism, namely, a verificationist or warranted assertibility theory of justification *sans vérité*. He also rejected strenuously the claim that some of our beliefs are correct descriptions of an external world, because, he claimed, we have no reliable method for deciding *which* propositions would be correct descriptions rather than simply justified beliefs that they are correct descriptions (Brandom 2000, 2–14). Rorty thought that the true pragmatist should have been a cultural relativist rather than a classical realist or a scientific realist. And even Quine was often tempted to move into the scientific anti-realist position when arguing for hyper-Pythagoreanism, or ontological relativism, while arguing (along with Carnap 1950; 1956; Sellars 1963) that the choice of a physical-object language was always and only a matter of the superior empirical adequacy of the theory chosen to systematize our surface irritations (Quine 1953; 1976a; 1976b). Of course, we must not forget that Goodman also, while hastening to declare himself an *irrealist*, in fact adopted an anti-realist position, for affirming unhesitatingly that all properties of the world were in fact linguistic in nature (Goodman 1978), and Rorty agreed with him (Rorty 1995; 1997; 1998).

Finally, there is a strong tendency for most philosophers simply to assume that the standard pragmatic solution to the problem of realism is simply instrumentalism and that, for that reason, most pragmatists are basically scientific non-realists, rather than

anti-realists or realists. This is because, as we saw a few pages back, the first core epistemological principle of pragmatism is that beliefs and belief systems are created by us in response to our perceived needs to have beliefs that will ultimately facilitate our dealings with sensory experience. Such beliefs are more or less adequate, pending their efficacy as instruments for successfully forecasting our phenomenal experience and thereby advancing biological adequacy, under the principle of homeostasis. Stated differently, for some pragmatists it is not the goal of scientific inquiry to find the truth of things or to determine whether our current scientific beliefs are providing us with knowledge of an external world. Whether our beliefs in fact do as much is not a scientific question, and certainly if they are philosophical questions, scientists *qua* scientists will not be helpful in answering it. We cannot be sure, moreover, that simply because some scientific theory provides us with remarkable instruments for predicting precisely the sensory experiences implied by the theory that therefore it is true, in the sense that the sentences asserted to be true in an empirically adequate theory are true. Ordinary appeals to the truth of a theory do not explain the predictive adequacy of theories because we can find past theories that were empirically adequate but were not true because they came subsequently to be replaced (Cartwright 1983). Instrumentalists will note that history has taught us that yesterday's absolute truth is today's outmoded theory but yesterday's empirically adequate theory. How else could we determine that empirically adequate theories are, for all that, a set of true sentences providing us with knowledge of the external world? Better by far, they say, that we rest content with the more modest view that the purpose of science is simply to create empirically adequate systems of beliefs that will allow us to predict the relevant sensory phenomena implied by the presumed truth of the theory and then thereby adopt it. Again, this is not to deny that successful scientific theories sometimes, in their central theoretical claims, provide true pictures of an external world. It's just that we have no way of demonstrating as much from the mere empirical adequacy of the theory. Nor does there seem to be any other way to do it. We are much better off, one might say, if we take our successful theories merely as instruments for predictive adequacy, affirm that as the major goal of all scientific reasoning, and leave aside the issue whether science ever gets to the truth of an external world for the simple reason that there is no non-arbitrary way to answer it. This general story has been demonstrably adopted by various pragmatists, such as John Dewey (1929), Wilfrid Sellars (1963), Rudolf Carnap (1966), Bas van Fraassen (1980), Nancy Cartwright (1983), and Nicholas Jardine (1986). What might we say about these various pragmatic responses to the question of whether scientific theories, at their best, tell us anything about an external world or are merely successful instruments for prediction and control, carrying no ontological implication about the real external world?

Given these considerations, it is something of a stretch to determine the value of the pragmatic contribution to the discussion on scientific realism and its mutually exclusive options in anti-realism and non-realism. Indeed, there is arguably no distinctively pragmatic position and, although some of these positions may seem considerably more defensible than others, all of them seem to satisfy the core features of pragmatism, noted above. Take, for example, the first three tenets of scientific realism, namely,



conditions (a) through (c) above. Separate arguments are required for each of these three assertions. Peirce actually offers no fewer than three distinct arguments for (a). The first one we mentioned above when referring to Peirce's argument to the effect that all inquiry proceeds on the assumption that there is an external world and that something or other is so about it, something that does not depend on our thinking or wishing it to be so, and that if we continue to conduct inquiry with the scientific method about the correct answer to any answerable question, we shall find the correct answer sooner or later and that will be the Truth. His second argument was simply an assertion that we are all aware of something not of our creation in the experienced duality and brute compulsiveness of our experience, a compulsiveness that speaks to the existence of something not caused by our minds. The third argument, which he classified as an experimental proof of the existence of the external world, and which has become known as 'Peirce's Harvard experiment' (because he conducted the experiment during a lecture he gave at Harvard) began with his distinguishing between mental and physical properties, the latter of which would be, unlike mental properties, what they are quite independently of what we think. Mental properties change or can change with changes in the way we think. So, he thought that if we could experience something that could not be altered, changed or destroyed simply by a change in our collective thinking, we would have proof of physical properties not dependent on the existence of any number of minds. This he claimed to find in simply dropping a stone and noting that no matter how much we would want it otherwise, the stone will drop every time when released. Not only did he find that sufficient evidence for the existence of an external world, the observed regularity with which it occurs is also evidence that the stone is governed by law-like forces equally non-mental for not being alterable by our wishing, willing, or wanting otherwise. Whatever else we might think about Peirce's arguments for the existence of an external world, there does not seem to be anything particularly pragmatic about this last argument or about the one before it. The first one, however, the one to the effect that all inquiry goes on the assumption of an external world and without such an assumption we would not have any knowledge or even desire to conduct inquiries, seems particularly vulnerable. As we saw above, the skeptic will urge so much the worse for that assumption because the skeptic has argued in various places that indeed there is no knowledge and that in effect the assumption in question is unjustifiable. Besides, is it really true that all inquiry proceeds on the assumption that if we continue indefinitely and progressively in searching for the truth that sooner or later we will find it? Is that meant to be obvious? This is not the place to launch into Peirce's other arguments. Suffice it to say that while they may be acceptable, there is nothing particularly pragmatic about them, although, to be sure, Peirce was a fallibilist and a verificationist of sorts and a radical empiricist.<sup>12</sup>

Does construing the existence of an external world, or the use of a physicalist language, as a useful posit or postulate fare any better? Recall that pragmatists such as Quine, Rescher, and others have offered just such a reason for believing the first tenet of scientific realism. Well, of course, if we have a strong inclination to believe in an external world, and if, after careful reflection, we have no reason to disbelieve it, certainly one would be justified in accepting it. But, as Goodman once noted, science

does not decide as between realism and phenomenalism (Goodman 1965; 1978). We can do science equally well, he claimed, whether we construe our sensory experience phenomenally or realistically. We can always convert physical object statements into phenomenal statements about how the world will appear under certain conditions and do science just as well. If that is so, and there are some who might object to this claim, there is no reason to think that the language of physicalism, and its commitments, is more privileged in principle than the language of phenomenalism, with its having no realistic commitments. Certainly, philosophers such as Quine (1953) and Sellars (1963) and many scientists accept the language of physical objects because it seems to work better for describing the universe. The scientific realist who argues that we should accept the belief in an external world because as posits or postulates go, it is not refuted in the implications of accepting it will need to deal with the reply that if we construe physical objects in purely phenomenal terms, we would get the same results, thereby showing that the posit argument is no better than the argument positing only phenomenal objects. But this is a longer story than we have space here to discuss.

Turning to the above arguments for scientific non-realism and scientific anti-realism, it seems worth noting that the scientific anti-realist position Rorty has defended for many years he has also recently abandoned after coming to agree with Donald Davidson that if we are to explain our success in communicating with each other we must assume that the vast majority of our beliefs must be true in the ordinary sense of 'true' (Brandom 2000, 77, 370–378). Whether he should have agreed with Davidson on that point, for the reason given, is debatable.<sup>13</sup> Even so, Rorty does not see his reversal as much of a problem. But others certainly will. It makes him not an anti-realist, but more of a 'blind realist' or perhaps even a structural realist because he will continue to assert that even if most of our beliefs must be true, we still cannot justifiably distinguish between true beliefs and those that we are justified in accepting as true relative to current and revisable societal standards of evidence. After all, it was Rorty's earlier view that there is no truth because nobody would know it even if they had found it, and he has not yet abandoned that point of view.

In the end, scientific non-realism, at least as it is developed and defended by van Fraassen, seems the most defensible of pragmatic positions on the question of scientific realism, or the status of theoretical entities in science. Van Fraassen, along with many others, argues that while we may seek the truth in science, our basic theoretical goal is empirical adequacy by way of explaining all past, present, and future phenomena in belief systems that allow us thereby to predict in detail our sensory data.<sup>14</sup> The latter occurs under robust but fallible confirmation subject to future revision. Whether the empirically adequate theory at any given time is telling us the truth (understood alethically) about the external world, rather than merely about how to predict precisely our sensory experience, is not a scientific question. So the main goal of science will need to be construed solely in terms of the ability of our theoretical models to make suitable predictions in the light of our empirically adequate explanations. Van Fraassen calls all this *constructive empiricism*. Given that, rational acceptability is simply a matter of the capacity of the theory to provide that predictive power so necessary for adaptation under homeostasis. When we have such a theory, we can, if we so choose, call it true

and embrace it as true. But we do so at our peril especially because we have seen enough empirical adequacy in the presence of what we subsequently called a false theory. So, it may well be that the instrumentalism of a van Fraassen, or a Dewey, or a Carnap, or a Sellars turns out to be the ultimately pragmatic line on this issue, and that story is by no means a tale told by an idiot signifying nothing.<sup>15</sup>

All this is not to say, of course, that van Fraassen's instrumentalism is without problems. Several critics have pointed to different problems. Brian Ellis, for example, has argued that van Fraassen has based his agnosticism about theoretical entities in theoretical science solely in terms of model-theoretic science and has also fundamentally justified his instrumentalism on the underdetermination of theories by data, when in fact some scientific theories are plausibly not underdetermined by the data (Ellis 1985). Stathis Psillos has argued that there are serious perils associated with van Fraassen's selective scepticism relative to his rejection of truth as a goal of theoretical science while asserting a form of common sense realism and truth for observational claims (Psillos 1996). Finally, others have observed that van Fraassen's effort to provide for a more modest set of goals for theoretic science than what the realist offers does not liberate van Fraassen from the difficulties the scientific realist must face. On this note, for example, one of the idiosyncracies of his instrumentalism is that it asserts that the classical realist cannot determine when the truth has been reached, and yet van Fraassen never provides us with any insight on how to determine when a theory has attained to 'empirical adequacy' in the strong sense of the phrase he insists on as the goal of theoretical science.<sup>16</sup>

People will differ, of course, on how seriously we should take all these criticisms. Without caring to enter into this particular debate in any great detail, I would urge that the position adopted by van Fraassen would seem to follow fairly straightforwardly from any commitment to fallibilism and the open-ended nature of evidence, even if some theories are not underdetermined by the data. Nor would we seem to have good grounds for re-classifying van Fraassen's instrumentalism as a form of anti-realism. After all, he can still urge along with Putnam and Boyd that, if we are not to make a mystery of scientific success, at least some of our theoretical beliefs must be true alethically at any given time, even if we cannot say which of them are true.

The last item where pragmatic thought has entered prominently into the discussion of the philosophy of science is in the area of scientific explanation. Let us turn to that topic.

#### 4. Pragmatism and Explanation

The extensive literature on the concept of explanation in the natural sciences generally affirms that if we want to explain why a particular event occurs at some time or other we must appeal to some law-like generalization in such a way as to make the event to be explained (the explanandum) predictable prior to its occurrence, and such law-like generalizations, whether nomic or statistical, must be true. In short, whatever other differences there are among those who adopt this general rubric, they generally agree that when we ask the question 'Why?' about the occurrence of some event in the

observable world, we do so to understand what causes things to occur in one way, rather than in another; and because their being one way rather than another seems obviously independent of our wishes and wants, the causes we adduce in giving an explanation should be understood to capture objective forces in the world and account for the state of affairs we wish to understand. Alternatively expressed, the point of natural science is generally to understand why things happen in the way they do, and that requires an understanding of why things are the way they are, or why events occur in the way they do. In all of this there is then an implicit assumption that there is a way the world really is, and its being that way is causally and logically independent of the existence and cognitive activities of any number of human minds, and that if the knowledge we acquire via our proffered explanations is to provide any real understanding of this independent world, it is the truth of why things are the way they are that is the primary goal of our explanations. On this proposed view, explanations are truth-seeking instruments, or attempts to provide an understanding how things really are, and why they are what they are. Hence explanations, or the statements that constitute the explanations, must themselves be true if they are to provide an understanding or knowledge of what it is they seek to explain. In advancing the classical deductive-nomological (D-N) model, or covering-law model, of explanation, Carl Hempel and Robert Oppenheim, for example, clearly argued that to explain why something occurs in the way it does we must appeal to some true law-like-generalization, followed by a true statement of the current initial conditions under which the law designated by the statement of law applies. The event to be explained is then explained as the deductive conclusion of the statement of law and the current conditions under which it applies.

For example, to answer the question, 'Why does this gas pressure gauge read 40 lbs psi (per square inch) after the amount of the gas within the container was increased by 100%?', we must provide an argument consisting some true lawlike-statements, followed by a true statement of the current conditions under which the aforementioned laws designated by the statements of law apply, and the event to be explained should follow deductively from the statements of law and the particular conditions under which the laws apply. That is to say, we must provide a deductive argument of the following sort:

P<sub>1</sub>. All else being equal, assuming constancy of temperature, the pressure and volume of any gas in a closed container vary inversely.

P<sub>2</sub>. This is a closed container with a pressure gauge reading 20 lbs psi five minutes ago, and we just now finished pumping into the same container an amount of gas equal to the original amount.

Therefore, this gas pressure gauge reads 40 lbs psi after the amount of gas was increased by 100%.

Accordingly, the explanandum follows from the explanans in the same way a conclusion follows from the premises in any sound deductive argument. This model, according to Hempel and Oppenheim, provides a confirmable answer to the original why question because the state of affairs to be explained follows deductively from the true statement of law, combined with the true statement of specific relevant conditions

under which the law applies. When the event to be explained follows from a statement of law and specific conditions under which the law applies, one has explained successfully why the event occurred and the why-question is answered. Hempel maintained that this model is in fact a generalization of what practicing scientists actually employ in their search for sound explanations, and so it is simply a description of the logic of good scientific practice, rather than a philosophical prescription. Notice too, of course, how it is a feature of this generalized D-N model that a good explanation offered under it also furnishes reliable grounds to have predicted the event before the event actually occurred. A good explanation is one we would have been able to use to reliably predict the event to be explained prior to its occurrence. If a proposed explanation does not do as much then it will fail for not offering an explanatorily relevant explanation. In fact, as is well-known, under the D-N model the only difference between an explanation and a prediction is pragmatic; if the event has not yet occurred, the D-N model predicts it, and if the event has already occurred, the D-N model explains the event by furnishing grounds to have expected it before it occurred (Hempel 1966, chapter 5).

There are well-known criticisms by way of seeking emendations to the D-N model for reasons of scope or relevance.<sup>17</sup> Invariably, however, those suggested revisions do not question that the goal of an explanation is to find the truth, and that explanations are only adequate if they provide us with a true understanding of the causes of the phenomena to be explained. But there are pragmatists who, as instrumentalists, have challenged the received view that the goal of an explanation is the attainment of truth in understanding the causes of observed phenomena or events.

For example, and as we saw above, van Fraassen, in advancing what he has called constructive empiricism, has argued, along with others, that the goal of theoretical science, and hence of scientific explanation, is not truth but empirical adequacy, meaning that theoretical science in constructing systems and models is not necessarily concerned with seeking and finding the truth in general or with anyone's theories as much as with corroborating or confirming proposed hypotheses, adequate by way of predicting our sensory experience in relevant ways. As soon as we attain to the latter, we may, if we so choose, accept the proposed hypothesis as true but, of course, it may not be (van Fraassen 1980, 151–152).

While for pragmatists such as van Fraassen, theoretical scientific explanation is arguably less a matter of seeking truth than of satisfying certain cognitive needs for adaptation by predicting precisely our sensory experience, explanation is also described as a sufficiently context-sensitive activity to warrant the view that, depending on what one's purposes or goals may be, different explanations of the same event may be adequate and that the adequacy or completeness of an explanation should be judged relative to different goals and purposes.<sup>18</sup> For some purposes or goals certain explanations will be perfectly adequate, but the same explanations would not be adequate for others.

As an example of how contextual factors can determine different but adequate explanations of the same event, van Fraassen refers to a well-known passage written by Norwood R. Hanson:

There are as many causes of  $x$  as there are explanations of  $x$ . Consider how the cause of death might have been set out by a physician as 'multiple hemorrhage', by a barrister as 'negligence on the part of the driver', by a carriage builder as 'a defect in the brakeblock construction', by a civic planner as 'the presence of tall shrubbery at that turning'. (Hanson 1958, 54, cited by van Fraassen 1980, 125)<sup>19</sup>

While philosophers such as Russell and van Fraassen will differ in relevant respects in their understanding of the requirements of a good explanation, a striking similarity will be the belief that, depending on one's purposes, explanations can be more or less adequate, more or less complete. Van Fraassen's view is that science does not in fact seek truth, that science is perfectly happy to accept an explanation as soon as it satisfies our collective need to predict precisely our sensory experiences under standard methods of confirmation, and this we can do without having to provide theories that are true in their theoretical claims, rather than empirically adequate for predicting relevant sensory experience (van Fraassen 1980, 120ff; for a very similar argument, see Cartwright 1983). As described, Russell's position would appear to agree with van Fraassen's instrumentalism, affirming that the goal of a good explanation is simply its confirmed power to predict reliably our sensory phenomena. But, of course, Russell would not have thereby abandoned truth as the primary goal of scientific explanation, rather than seeing it realized in propositions fully confirmed under induction.

Van Fraassen, as we also saw, pleads agnosticism on whether any empirically adequate theory is in fact true in the theoretical claims it makes. For him, empirically adequate theories may, or may not, be true in this way. In fact it may even be the case that they are either true or false in their fundamental claims. But truth, as we ordinarily understand it, is not the goal of theoretical science, hence cannot be a requirement for any explanation seeking to satisfy the goal of theoretical science. The difference between what Salmon and van Fraassen regard as the goal of an explanation is, as Salmon himself acknowledged, rooted in what each regards as the purpose of an explanation in science. It is van Fraassen's view that if we ask practicing scientists what they seek, the answer will be 'empirical adequacy' first and foremost. Whatever else that implies is grist for the philosopher's mill (van Fraassen 1980).<sup>20</sup>

Other radical pragmatists, such as Ellis or even a Rorty, will take issue with van Fraassen's pragmatic instrumentalism for its countenancing even the possibility that one's theories and explanations are true in the usual sense of 'true', or for van Fraassen's claim that one's constructed theories or explanations will of necessity require that we have knowledge or true beliefs about the world of observed phenomena, if we are to provide anything by way of confirmation of proposed theories or explanations (Ellis 1985; Rorty 1997). In the hands of someone like van Fraassen, a pragmatic construal of scientific explanation does not require truth as we ordinarily understand it; but the more radical pragmatists, those who abandon truth completely as an unattainable goal in any way imaginable, will object to the idea of pleading agnosticism on the question of truth, rather than their 'veritistic' atheism, which offers the rest of us nothing by way of an explanation for the long-term success of some scientific theories. As a matter of fact, van Fraassen's agnosticism, as we noted earlier, does not provide us anything



better by way of accounting for the long-term predictive success of some scientific theories.

## 5. Conclusion

By way of summary, if there is a defensible and distinctive proposal made by pragmatists on the problem of induction, it would seem to be the one advancing the view that accepting induction is validated because it leads to beliefs that allow us to adapt successfully, even though there is strictly no inductive or deductive proof of the validity of induction as a source of human knowledge. But that requires defending the view that the primary purpose of inquiry is to establish beliefs that allow us to adapt successfully under homeostasis. For reasons indicated earlier, that goal seems pre-eminently more defensible than having the goal of attaining the truth as either a primary or secondary end of belief formation. Moreover, pragmatists such as Rescher and many others think that denying that induction leads to knowledge is simply to condemn induction for failing to be deduction. Pragmatists usually say that at the root of every rejection of induction as a source of knowledge is a sterile and indefensible Cartesianism that poor Hume never saw as motivating his own position. Although Peirce's defence of induction seems clearly to have begged the question against Hume's argument, Peirce elsewhere argued strongly against the sort of Cartesianism at the root of Hume's rejection of induction as a source of knowledge. The proposal made by Skyrms, Salmon, Reichenbach, and others to the effect that if any method will work, induction will work appears to beg the question against Hume's argument but fails in the end as a good reply because it rests on blaming induction for not being deduction.

When we turn to the question of realism, non-realism and anti-realism in science, it appears that, for all the reasons indicated, there is no distinctively pragmatic proposal on the table, although, to be sure, many pragmatists have delivered themselves of differing views on the matter. Despite that, there is good reason to think that the purest and most defensible pragmatic proposal is the non-realist sort of instrumentalism adopted by van Fraassen and a few others on the question of the external world and the existence of theoretical entities. Although van Fraassen's pragmatic proposal in question is idiosyncratic and subject to various criticisms, none would appear to be insurmountable without too much fundamental alteration of the original position. But this is a long story for another time.

Finally, when we turn, to the question or the problem of scientific explanation, there does seem to be a distinctive pragmatic proposal countering all variations on, and emendations of, the D-N model. To the extent that all pragmatists can be seen to advance a warranted assertibility theory of truth, combined with a deep fallibilism, we can view them as abandoning truth (as we ordinarily understand it) as a necessary condition for adequate statements of law. The premises of an adequate explanation need not be true but only warrantably assertible as true. Truth, as we ordinarily understand it, would not be a necessary condition for any valid explanation. A valid explanation need only predict the sensory phenomena we should expect if the hypotheses were true. This last may well turn out to be the enduring contribution of pragmatism along,

with a van Fraassen-like instrumentalism with regard to the existence and nature of an external world and theoretical entities.

## Notes

- [1] However unfortunate, there is an enduring tradition that consists in characterizing the epistemology of James as endorsing the view that the truth of a belief is fundamentally a matter of whether one attains psychological satisfaction in wilfully accepting any proposed belief as true, as if the requirements of scientific methodology could be generally ignored in determining whether propositions about the world are more or less worthy of rational acceptance in virtue their robustly confirmed predictive power. This tendency continues in Blackburn's recent defence of Craig's assault on irrational conviction, taking as a paradigm example of such irrational behaviour the position adopted by James (Blackburn 2005, 3–20). For the better interpretation of James, see Tiercelin (2005) and Almeder (1986; 1990). Doubtless, James is partly responsible for this traditional misinterpretation. But, as we shall soon see, James's position was less a matter of endorsing wilful belief *carte blanche* than it was a matter of granting provisional permission to accept certain beliefs for which natural science could provide no systemically compelling evidence, either for or against, under induction, when and only when the effects of so believing tend to produce consequences that provide more good or even happiness than would otherwise occur if one had believed the denial of the proposition in question or if one had chosen to believe nothing at all. Why he adopted that position is partly explained, and defended, in Almeder (1986).
- [2] When it comes to the epistemic justification of one's beliefs, the evidence for determining whether the beliefs are successful *because* the beliefs in question in some way either assert, assume, or imply assertions that are more or less correct descriptions of what they purport to describe, is problematic. Some contemporary pragmatists will find good reasons to assert that the best available explanation for the long term success of any hypothesis or theory is that such hypotheses or theories have within them sentences or affirmations that succeed in correctly describing the external world, even if we have no way to determine which assertions in current theory are in fact the correct descriptions. Such pragmatists may characterize themselves as either structural realists or blind realists (Almeder 1990; Worrall 1989). Other pragmatists, as we will see, leave it an open question of whether successful theories are empirically adequate theories *because* they, in some important measure, successfully describe an external independently given world. There are many pragmatists who will, for example, simply urge that empirical adequacy or applicative success of our theories or beliefs will be enough to satisfy the fundamental goal of inquiry. For them, while a goal of science may be to search for the truth, and to accept only what we take to be true in the ordinary sense of 'true', we are perfectly content to declare our theories confirmed and acceptable when they satisfy the primary goal of allowing for precise prediction and control, and there is no fundamental need to assert that the utility of the theory is a function of its truth rather than say that the long-term success of hypotheses and theories is simply a matter of natural selection. For them the goal of science is not so much a matter of providing true sentences about an external world as it is to provide systems of belief that shall not disappoint us by way of providing successful instruments for prediction and control, hence for biological adaptation. For examples of pragmatists who affirm as much, those whose works come to mind immediately are Reichenbach (1938), Carnap (1950, 1966), Quine (1953), Sellars (1963), Putnam (1978, 1981, 1982), van Fraassen (1980), Cartwright (1983), and Rescher (2001; 2003).
- [3] The second most frequent objection, one also raised against verificationism, is that pragmatists, in defining truth in terms of confirmation conditions confuse truth with confirmation, or the concept of truth with the criterion for truth. Anti-pragmatists and anti-verificationists have not understood, however, that pragmatists are not trying to define truth as we ordinarily

employ the concept. They are rather best viewed as offering a substitute notion on the grounds that truth as we ordinarily understand it is an empty concept relative to the attainable goal of inquiry because we have no reliable decision procedures for reliably determining *which* sentences in our language satisfy that alethic concept owing to the fact that we can never attain to anything more than a high probability of truth which is logically distinct from attaining the truth itself. It would have been nice, of course, if they had all made this a bit more explicit.

- [4] For a restatement of this objection, see, for example, Blackburn (2005), 7–13.
- [5] For a similar view see Worrall (1989, 99–124), Almeder (1992, chapter 4), Putnam (1978).
- [6] For this sort of mistake see, for example, Bonjour (1998), chapter 5.
- [7] Peirce is often conflicted on whether we seek the truth, with capital T, or just what we deem or sincerely think to be true, even if it is not true (Almeder 1980, chapter 1).
- [8] For a treatment of the Cartesianism at the root of demanding a justification of induction and why the demand should be rejected, see Tiercelin (2005, section 1).
- [9] See the interesting discussion of this argument by Feldman (2003, 136–137).
- [10] For similarly expressed sentiments among pragmatists, see Dewey (1929), Sellars (1963), Carnap (1966), van Fraassen (1980), Cartwright (1983), and Mellor (1991). Mellor offers an especially spirited defence of this pragmatic vindication of induction, as did Reichenbach (1938) and Levi (1967).
- [11] Elsewhere, incidentally, I have argued that Peirce finally came to believe that the final irreversible opinion on any answerable question will come, that he argued as much but that the arguments are not terribly persuasive, and finally that we can offer him compelling arguments for that same thesis (Almeder 1986, 1990, chapters 4 and 5). I characterized his position by calling it ‘blind utopian realism’. But a majority of pragmatists adopt the view that Truth, as Peirce understood it, was an ideal regulative concept approached by the scientific community asymptotically in the ideal limit of inquiry under the assumption that scientific inquiry will continue forever and progressively into the future. This same position is evident, for example, in the works of Sellars (1963), Putnam (1981), Ellis (1985), Jardine (1986), Rescher (2003), and Tiercelin (2006). For them, knowledge will be a matter of warranted assertibility relative to available evidence without truth as a necessary condition, since the latter cannot be attained rather than asymptotically approached. For them, as well as for those who, as Rorty once did, simply abandon truth as a meaningless concept, knowledge will be a matter of warranted assertibility relative to contemporary social standards of evidence. Neither position seem to avoid the pitfalls of cultural relativism and classical idealism. But for those who would adopt the Peircean position that truth, in its ordinary sense will be attained if inquiry will continue indefinitely long, such idealism and cultural relativism would not be such a problem. But it does have the serious problem of proving that scientific inquiry will continue indefinitely long answering more and more non-trivial questions, and that the number of such questions is finitely many. Partly because of the difficulty involved in solving this latter problem it seems easier to most of these Peirceans just to say that truth can only function as a regulative concept. But for the cultural relativism or classical idealism implied in this view of Peirce, it might seem easier and attractive. The more disconcerting point, however, is that there are texts in Peirce’s writings that support both positions although I would argue that the latest opinion he had on the matter was that the final irreversible opinion will come.
- [12] For reasons of space, I pass over without comment what has recently become known as Peirce’s abductive argument for realism. This is what Tiercelin (2005, section 6) has termed ‘abductive realism’, which she discusses and defends as Peirce’s best argument for the existence of the external world.
- [13] On this item, it is again interesting to observe that Peirce argued strenuously that induction will lead to the truth more often than not because it is absurd to think that induction should fail as often as it succeeds in giving us the truth in the long run (Peirce 1931–58, 2.757n1, 2.758, 2.769). Whether this argument would support the position adopted by Davidson and

Rorty to the effect that the vast majority of our beliefs must at any given time be true, is doubtful for the reasons that we have mentioned above.

- [14] For Van Fraassen, the 'empirical adequacy' of a theory can be either weak or strong depending on whether or not the theory fits or explains all available present and past observable phenomena, or whether it fits all the present, past and future observable phenomenal facts. This distinction is important because he claims the goal of scientific theory is empirical adequacy in the strong sense (van Fraassen 1980), and a ticklish subject is whether anybody can show that strong empirical adequacy can be shown to obtain in any given case, the implication being that the condition is too strong and so only the weaker sense of 'empirical adequacy' can stand as a legitimate goal. This objection would sustain, without further comment, the view that van Fraassen's instrumentalism in theoretical science is really not much better than the sort of anti-realism offered by Rorty and other cultural relativists.
- [15] This author has defended a weaker form of instrumentalism affirming the view that some of our beliefs about an external world, including the theoretical beliefs, must in fact be correct descriptions (even if incomplete) of an external world, although we have no reliable method for determining which of our beliefs are correct descriptions rather than very probably correct descriptions (Almeder 1992).
- [16] See note 14 above for the distinction between strong and weak 'empirical adequacy' as it relates to this criticism.
- [17] For a critical discussion, evaluation, and emendation of the D-N model of explanation see, for example, Salmon (1984).
- [18] Van Fraassen (1980, 125), reprinted in Papineau (1996, 82–92). See also Salmon's discussion of the differences between his and Van Fraassen's concept of explanation and why Salmon thought that in the end the pragmatic emphasis on context-sensitivity of explanation is indefensible owing to the fact that complete theoretical explanations will always be required in science, and that in itself requires an understanding of all the causes and causal mechanisms involved in producing a certain event (Salmon 1984, 127ff).
- [19] An historical example, incidentally, of what van Fraassen here advances relative to the context-sensitivity of an explanation, and how its adequacy is to be determined by the goals or purposes of the inquirer, is one offered earlier by Russell in his famous BBC debate with Frederick Copleston on the existence of God. In fact, we may view Russell's position as an earlier instance of what some contemporary pragmatists advance as an adequate explanation. Copleston had argued that we do not have a causal explanation for anything unless we admit to the existence of God. Russell replied that if he wanted to know why the tides, for example were higher or lower at different times, or why the one succeeded the other in a regular way, he only needed to know something about the law of gravity, and varying gravitational effects on different locations on the earth under various phases of the moon, in order to predict precisely the times of the high and low tides. Russell, insisted that the demands of the question required for an explanation nothing like a belief in the existence of a God. For the question at hand, an explanation is perfectly adequate as long as the events to be explained could have been predicted rather precisely by appeal to certain laws and conditions under which they apply. Copleston replied that for some purposes, some explanations will be more or less adequate or complete depending on one's purposes, but one cannot presume to have explained completely why the tides rise in the way they do in different locations, without having an answer as to why the law of gravity exists or works the way it works, or why there is anything at all. The same position Copleston advanced is also offered by Swinburne (1979) in his defence of the existence of God and also in Burr and Goldinger (2004).
- [20] Van Fraassen, incidentally, accepts as true some observational beliefs when there is no good reason to think anything else, because otherwise we could not confirm any proposed theory in terms of its deductive implications at the sensory level (van Fraassen 1980). He sees nothing particularly problematic in affirming the truth of some of our common sense beliefs while denying determinable truth associated with theoretical beliefs. More radical pragmatists, such

as Sellars, who saw natural science as a matter of common sense gone systematic, would tend to see the claims of common sense and natural science as no different in kind, the alleged distinction between the theoretical and the observational a distinction without a real difference, and hence regard all claims about the external world, whether theoretical or observational, as revisable in the light of future changes in evidence or rules for interpreting the evidence.

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