Lessons from Pragmatism for Philosophers of Science: Nine Teachings and a Cautionary Tale

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Introduction

I am suggesting a stance for philosophers of science based on what I take to be the core elements of classical pragmatism. These elements emphasize epistemology, so they are not a complete presentation of all the elements of pragmatism, the listing of which may be an impossible task. I am not as pessimistic as most, however, who say that pragmatism is hardly a cohesive movement, going all the way back to Arthur Lovejoy's famous articles of 1908, "The Thirteen Pragmatisms." I believe that it is possible to find clear common elements in writings of Peirce, James and Dewey, and I believe that their views are best interpreted as a stance. Calling a philosophical position a stance has become rather popular recently among philosophers of science, with Bas van Fraassen's presentation of empiricism as a stance, Anjan Chakravartty's presentation of realism as a stance, Hasok Chang's pragmatist revival of everyday realism as a stance, and even back to Arthur Fine's advocacy for NOA, the natural ontological attitude (van Fraassen 2002, 2011; Chakravartty 2017; Chang 2022; Fine 1983). There have also been two special issues of journals on stances (Rowbottom and Bueno 2011; Chakravartty 2015). Some realists may still hold that their view is known to be correct, rather than a stance, but I will follow my contemporaries in taking general philosophical positions to be stances. Stances are attitudes or dispositions, rather than doctrine or knowledge. While I will not attempt to give a general justification here, I do have some comments under specific headings below that show

the value of the particular pragmatic stance discussed. Despite being strongly indebted to Peirce, James and Dewey, it does not matter much for my purposes if I am being completely fidel to classic pragmatism. I would be just as happy to call my stance a version of what Cheryl Misak has called new pragmatism (Misak 2007). In any case, I will present what I take to be the important lessons from pragmatism for philosophers of science.

1. Fallibilism

We start with a universally critical stance, which is equivalent to fallibilism. Anything is open to inquiry, because any of our beliefs could wrong.[1] There are no absolutes, no foundations, nothing is certain. Notice that this is different from saying that everything can be questioned at once, which is an incoherent position, given that we must always start inquiry with some beliefs, more on this in a moment. Fallibilism is also different from saying that we have some specific doubt about a given claim. If we have some specific reason for doubt, we would not say that we know something, rather we would say that while there is some good evidence for the claim in question, there are also some doubts. When the fallibilists say that they know something, they are simply leaving open the possibility of further inquiry into the subject. We can see this as equivalent to Peirce's famous dictum "do not block the path of

[1] For Dewey, inquiry begins with a problem, for Peirce we aim at overcoming all (legitimate) doubt, but perhaps we should always inquire, even if we are complaisant; indeed, especially if we are complaisant! I would say that we inquire because we have a critical attitude, not because we have a problem or a specific doubt.

inquiry" (Peirce 1899, in 1931-35, CP 1.135). Claiming certainty would close off further inquiry, while the fallibilist maintains that inquiry is always open. I may not see any way to question a belief, but that does not mean that someone else, now or in the future, will not see a way to further inquiry.

Fallibilism is the proper attitude to have as we inquire, because we cannot know what future inquiry will bring. Fallibilism especially is a stance or attitude, not a belief or something known to be true. One cannot say that I am certain that there is no certainty, that would be a performative contradiction, but one can have a critical attitude towards beliefs, rather than knowledge than nothing is certain. Given that those claiming certainty are making positive claims, the burden of proof is on them to present and defend them. Furthermore, many purported certainties from the past have turned out to be false and for many of the rest there are clear alternatives, so adopting fallibilism is justified. Calling fallibilism a stance makes the position similar to that of the Pyrrhonian sceptic (Frede 1984), but a fallibilist can claim that we have knowledge, just that we do not have certainty. Fallibilism, or a critical stance, is key to everything that follows and is really what I take to be the fundamental lesson from classical pragmatism, stemming from Peirce. Fallibilism has a broad popular appeal, fits well with scientific practice and is generally thought of as being harmless. In fact, I think when philosophers start digging deeper into the view and they realize what they have to give up, we might see some push-back. Peirce predicted this years ago: "Indeed, most everybody will admit [to fallibilism] until he begins to see what is involved in the admission—and then most people will draw back" (1931-35 CP 1, 60 (#148)). I will argue later that even Peirce himself is not immune to the dangers of not taking fallibilism seriously enough. We can see some examples of

Peirce wavering on fallibilism in two important papers by Susan Haack (Haack and Kolenda 1977; Haack 1979).

2. Inquiry is practical/science is a practice

When William James introduced pragmatism in his 1898 address to the Philosophical Union of the University of California, he emphasized Peirce's principle that "in order to attain perfect clearness in our thoughts of an object, we need only consider what effects of a conceivably practical kind the object may involve" (James 1904, 673). James rightly takes Peirce's principle as a way to settle metaphysical disputes, but there are further implications. If all inquiry is practical, then surely scientific inquiry is practical. It therefore follows that in order to understand science, philosophers of science should focus on what scientists do in a practical sense, that is, philosophers of science should examine the material and social elements of science as well as the conceptual when they inquire about science. With a nod to Joe Rouse, who has long advocated the view, philosophers of science should consider science as a practice. In *The Quest for Certainty*, Dewey argues against the Greek separation of knowledge from practice (1929/1984, 15), showing that he too sees science as a practice.

3. The starting point of inquiry is wherever you are now.

In criticizing Descartes method of inquiry, Peirce says that there is "but one state of mind from which you can 'set out', namely, the very state of mind in which you actually find yourself at the time you do 'set out' "(1931-35 *CP* 5, 416). Just as we can inquire into anything because of fallibilism, we can inquire at any time or place. As a corollary, when we inquire, we

will necessarily bring along various assumptions and prejudices, as well as previously confirmed beliefs, given that we always start from somewhere, in a particular state of mind as Peirce puts it. All inquiry involves some taken-for-granted assumptions which are interpreted as the pragmatic (or relative) a priori. These assumptions play the role that a priori knowledge used to play in our knowledge and practice, but it would be better to drop the idea of the a priori altogether as misleading (Stump 2015). Under the pragmatic theory of the a priori there is no a priori knowledge in the traditional sense, rather there are taken-for-granted assumptions that are required to begin inquiry. Therefore, I prefer to speak of these as the constitutive elements of scientific practice, rather than the pragmatic a priori. However, given that there is a large literature on the pragmatic and other related relative a priori, I will sometimes maintain the term.

4. Anti-foundationalism, holism, and the theory-ladenness of perception/practice.

Some beliefs are better justified than others, but they could not act as a foundation in a tradition philosophical sense of the term. Nothing can act as a foundation because everything is in turn supported by various assumptions and presuppositions, as well as previously confirmed theories. Likewise, experience and observation always take place from a point of view, and is interpreted by an agent with beliefs, whether previously confirmed or presupposed. The thesis of the theory-ladenness of perception can be extended to a thesis of the theory-ladenness of practice. All of our practices involve presuppositions, taken-for-granted assumptions, etc. and without these, we could never begin inquiry. The theory-ladenness of practice is thus a version of holism, which Dewey introduces with his idea that inquiry takes place in what he calls a

situation: “For we never experience nor form judgments about objects and events in isolation, but only in connection with a contextual whole. This latter is what is called a situation” (Dewey 1938/1986, 72). All of our beliefs and even our practices are connected to a background of previously confirmed theories, assumptions, presuppositions and other practices such that if there is a problem, we cannot immediately know what the source is. The picture of inquiry here is that we start wherever we are, that is, with the tools, assumptions, and previously verified beliefs that we have now. All of these are fallible, but we start by taking them to be true, and build on these to inquire into some area or other.

5. Reject metaphysical realism as it requires dualism, but maintain an everyday kind of realism instead

Traditional realists reaching for the so-called external world have an untenable ideal of what knowledge is and this overreach has consequences, opening the door to all kinds of misunderstandings and to skepticism, because we can never directly compare our experience to the postulated external world, we can only have more experiences. Peirce is well known for being the most realist of the classical pragmatists, but Dewey also defends an everyday kind of realism in his *Logic* (1938/1984, 513 ff.) and James in his many essays on truth. For example, in an address to the American Philosophical Association in 1907, William James begins: “My account of truth is realistic, and follows the epistemological dualism of common sense”. He continues in the next paragraph: “This notion of a reality independent of either of us, taken from ordinary social experience, lies at the base of the pragmatist definition of truth. With some such reality any statement, in order to be counted true, must agree” (James 1909/2002, 217).

All that James says here is compatible with any form of realism, even metaphysical or correspondence realism, but note that he bases his dualism on common sense and calls it epistemological rather than metaphysical. In a recent book Hasok Chang also wishes to reclaim the term 'realism' in an everyday sense and he is strongly influenced by the pragmatism of James and Dewey (Chang 2022). Chang does an excellent job of rehabilitating and clarifying James's views, dismissing some of James's misstatements and providing sympathetic quotes different from the one I just gave. Andersen (2023) also presents a strong argument in defense of what James says about truth. Whether or not the term 'scientific realism' is worth salvaging is part of the argument of Chang's book, and his point is to reclaim realism and truth, given that they and their cognates continue to be used in everyday speech and in science. Chang rejects old-fashioned correspondence scientific realism, but he thinks that there is an everyday insight parallel to scientific realism that is important, the idea that our theories are not simply made-up, but rather have to be consistent with experience in a way that can show us that we are sometimes wrong. Chang makes the point that while we frame our experience, we do not control it (2022, 124). James makes the same point by saying that "experience has ways of boiling over" and exceeding our expectations (1907, 106) and Matthew Brown makes the point about need to interact with the world in his presentation of Dewey's philosophy of science: "Despite the fact that situations are agent relative and practice relative, they are nevertheless objective aspects of the agent-environment interactions. Situations are not subjective/mental entities; they are concrete elements of the natural world" (Brown 2012, 274).

It is not only pragmatists who have used this argument. Henri Poincaré also made this point when rejecting a generalization of his conventionalism, saying that while we create a language to express facts, we do not create the facts themselves (Poincaré 1905, 162; 1982 [1913], 332). Ian Hacking makes the same point in relation to his styles of reasoning, which is his term for epistemic systems and practices, by emphasizing that in empirical science, setting out the basic laws and definitions does not determine what is true. You have to experiment, build things and intervene in nature in order to find out what is true (Hacking 2000, S69).

6. Objectivity is still possible without foundations

Related the everyday realism of the pragmatists, they hold that universal and fixed principles are not necessary for objective knowledge. We must use some set of practices to study the world and those practices are necessarily contingent and limited to a specific place and time, but we can still say that some of our beliefs and methods are justified and that some beliefs are better justified than others. Indeed, with probability theory we can sometimes give a quantitative analysis of how justified our theories are, either comparatively or absolutely. I have recently published on these issues (Stump 2022, 2023), so I will give a brief account here.

Since there are no absolutes and inquiry always starts with presuppositions, in some sense everything is relative to those presuppositions, but that is not the same as saying either that every opinion is as good as any other, which is a required element of relativism in how Paul Boghossian defines it (2006) or that science is socially constructed which is what David Bloor and his defender Martin Kusch think. To see how science can be objective, the main issue to focus on is what Kusch has called dependence (Kusch 2016), the claim that a belief is only

justified (or not) relative to an epistemic system or practice. There is no neutral or absolute way to justify belief, only ways that will seem correct to those working from a particular viewpoint or practice. Foundationalists tend to respond to this situation by saying that there is something that is absolute and "self-justifying", either a set of criteria or empirical observations. While this move does break the circular reasoning in which we seem to be stuck, it is, of course, not open to the pragmatic fallibilist who thinks that nothing is absolute. The response to the issue of the dependence of a belief to an epistemic system or practice should rather be to point out that not everything is built into such a system. The epistemic system or practice gives us tools to accomplish our inquiry, but there is no guarantee that the tools will work. We can find things that are unexpected when experimenting, new phenomena that need to be incorporated into our epistemic system or practice. These are the points that I mentioned above, coming from Chang, James, Brown, Poincaré and Hacking.

Am I relying on some form of empirical "given" that is an absolute? And if so, does it not fall prey to the arguments given by Wilfrid Sellars concerning the *Myth of the Given* (Sellars 1956)? The answer to both questions is no, but it would take some argument to show why. Fortunately, Steven Levine recently published a book on this issue, and Jim Garrison an article, so I refer to their work concerning the details relating to classical and to neo-pragmatism rather than rehearse all of those arguments (Levine 2019; Garrison 2019). In brief, Levine and Garrison both argue that Rorty and Brandom were wrong to take Sellar's argument as undermining the concept of experience that we get in James and Dewey. Rorty claimed that empiricism is a dead end and that we have no choice but to take the linguistic turn to mean that truth depends on agreement. Most importantly, Levine argues compellingly that we can recover a notion of

experience that does not leave us in the linguistic realm, so we can get away from the idea that everything ultimately depends on agreement. Experience can settle issues without any claim that it is absolute. Garrison makes a parallel argument, focusing on Dewey.

My own argument is, first, the negative argument already given: The results are not built into a system of practices. The relativist cannot use the theory ladenness of practice to claim that scientific results are always invalid because the justification of scientific results is circular. One must consider individual specific cases to see whether scientific results are justified or not. There is no valid a priori argument for general relativism, which blocks Bloor's claim that social factors must always be brought in to explain why scientists adopt the theories that they do, given that theories are always underdetermined by the evidence and there are always alternatives. Second, I claim, the history of science shows a different image of science than that given by relativism, especially in the long term. Of course, there may be periods of time in which a particular area of inquiry is split between two or more teams of researchers, although only temporarily. For example, in his 1987 study Peter Galison showed how a major dispute in particle physics was settled when one side admitted that they could no longer keep reinterpreting the data, that the negative results would not go away (Galison 1987). My point from this example is that while it is logically possible to keep a dispute open (fallibilism again, and underdetermination) in fact, if one looks at the history of science, disputes are resolved. There are actually three possibilities. First, one side of the dispute may give in and accept the other side, as in Galison's example. Second, those resisting might become marginalized and unable to sustain a viable position. We might consider, for example, Herbert Dingle's resistance to Einstein's theory of relativity and what it did to his career (Chang 1993). Third, the dispute

could remain open, with active research programs on both sides of the dispute. Only in the third case do we have an argument for the description of science advocated by the relativist. As I noted before, there is no valid general argument for relativism, so we have to look case by case to see how disputes are resolved. My claim is simply that in many cases, experimental results determined what choices scientists make. Social factors play a decisive role sometimes, but not always. Relativists are making a mistake in denying the role of experience in the fixation of belief, and in ignoring the material culture of the sciences. Science is not just theory and cannot be reduced to ideas and beliefs.

7. The Continuity of Inquiry

Dewey strongly emphasized what he called the principle of the continuity of inquiry. In his *Logic: The Theory of Inquiry* it is mentioned in the first page of the preface and linked to Peirce (1938/1984, 3). Most notably, Dewey advocated for the continuity of inquiry into facts and into values, and in his contribution to the *Encyclopedia of Unified Science*, it is clear that the role of values in science and how we study values are the major differences that he had with the Logical Empiricists, even when he was publishing in their venue (Dewey 1938). Although inquiry into values is a very important example of the continuity of inquiry, the thesis itself is completely general as all types of inquiry are linked. For example, Dewey sees no essential difference between the social and the physical sciences (1938/1984, 481 ff.). Also, given that physical science and technology both affect society, Dewey rejects the distinction between pure and applied science (1938/1984, 483). Despite Dewey's advocacy of the continuity of inquiry in the 1930s, especially underlining the inquiry in facts and values, there was perhaps once a

consensus in the 1950s that science is (or at least should be) value neutral. However, the value-neutral thesis came under attack from feminist philosophers of science, those studying science policy, and many others including, prominently, Heather Douglas (2009), Matthew Brown (2020) and Philip Kitcher (2024). Philosophers of science now generally side with the pragmatists in arguing against the attempt to separate values from scientific inquiry. Brown recently published an overview and critique of a few attempts to revive value neutrality (2024).

8. Overcoming the fact/value distinction and strict dichotomies in general

Overcoming the fact/value distinction can be thought of as another way of furthering the continuity of inquiry. Facts and values are never pure, given that all facts have some trace of values, at least in the sense that they are important (or not) to us and all values have some kind of factual basis. We find that our dichotomy between fact and value is actually a continuum, pure fact being an ideal limit and pure value as its opposite, with everything else being some form of a mixture of fact and value. Traditional philosophy held many strict dichotomies that the pragmatists reject. For example, in the *Logic* Dewey says that we cannot separate deductive and inductive reasoning in practice, instead, we find mixed inferences (1938/1984, 423; also see 478-479). Similarly, Dewey argues that the classical distinction between nominalism and realism disappears when we consider how concepts actually function in scientific practice (1938/1984, 462). Dewey holds that in general, the dualisms found in traditional philosophy can all be overcome by gradualism (Dewey 1925/2008, p. 268–9). So, overcoming the fact/value distinction is only the beginning. Generalizing, there are no strict dualisms among concepts, only continua of possibilities. For another example, I agree with

Holly Andersen that there is no way to completely separate epistemology and metaphysics (Andersen 2023, 82).

9. Naturalism

Taking anti-foundationalism and the continuity of inquiry as methods and turning towards concrete practical consequences and away from metaphysics and other transcendental modes of inquiry, pragmatists can be said to hold some form of naturalism. There is no first philosophy, given that we use the same inquiry to question our methods, as well as the content of our theories, and all inquiry is the fundamentally the same, or at least different types of inquiry are on a continuum. The pragmatists' naturalism is not scientistic because it does not assume physicalism, nor necessarily privilege science as form of inquiry. For example, James famously advocated for a neutral monism. The point is that there is a continuum of methods, not a requirement that all questions ultimately be answered by science.[2]

10. Addendum, a cautionary tale

This is not a lesson the pragmatists taught, but rather a lesson to be learned from their contact with other philosophers. Most critics have taken the pragmatic theory of truth to be the core doctrine of the pragmatists (e. g. Russell 1910/1966, 116). It has also been noted by many that the pragmatists' treatment of truth has come in for more criticism from other philosophers

[2] For a recent discussion of scientism in relation to the classical pragmatists, see Mostajir (2023, 2025).

than any other of their views (e. g. Chang 2022, 197). Furthermore, pragmatists vary on their statements about truth and tend to say too much. My view is that contemporary deflationary accounts of truth are correct when they warn against defining truth, that is, they reject the question, what makes a sentence true? Instead, they give an account of how the word 'true' is used in ordinary language. Different philosophers have presented slightly different accounts of deflationary views, but here is a good summary from the *Stanford Encyclopedia of Philosophy*:

> "The main idea of the deflationary approach is (a) that all that can be significantly said about truth is exhausted by an account of the role of the expression 'true' or of the concept of truth in our talk and thought, and (b) that, by contrast with what traditional views assume, this role is neither metaphysically substantive nor explanatory" (Armour-Garb et al 2023).

What deflationism offers is a way to understand the disconnect between what the pragmatists say about truth and what their traditionalist critics charge. Pragmatists are talking about the way truth is used in discourse, while the traditionalists are looking for a definition that tells us what truth is—an essence that all true propositions share.

Some deflationists say that there is nothing more to the notion of truth than the disquotation schema ('p' is true iff p), but it is possible for a deflationist to accept a more robust account of the role of the expression 'true' or of the concept of truth in our talk and thought, as long as there is no definition of truth. Pragmatists were assumed to be giving a traditional definition of truth, an alternative to the correspondence theory. They seemed to be saying that a belief is true when agrees with what would be found at the end of inquiry, or that a belief is true if it is useful, etc. I suggest that it would be better for pragmatists to join deflationists by

clearly and emphatically rejecting such definitions and explanations. Indeed, it is possible to interpret the classical pragmatists as having done exactly that. By refusing to define truth they would not only then be able to forcefully address philosophical objections to their theories of truth, they could also then present a unified front on the other nine issues that I mention above. Of course, deflationary accounts of truth have also been criticized by philosophers holding traditional views. I will not attempt to respond to those critiques here, rather, I will concentrate my remarks on disputes between the pragmatists and the deflationists, which is an argument among friends given that pragmatism and deflation share many viewpoints. I will focus on Cheryl Misak's defense of Peirce in comparison with some deflationary accounts and Hasok Chang's defense of James and Dewey over a deflationary account.

Misak criticized deflationary accounts soon after they appeared, and followed up several times (Misak, 1998, 2004, 2007, 2015, 2017). She also has pushed back against the idea that Peirce gave a definition of truth. Indeed, in her later papers, she quite emphatically agrees with the central point of deflationary accounts and places Peirce, Ramsey and others in the deflationist camp (Misak 2015, 2017). Nevertheless, Misak argues that we need a full-bodied account of truth as it is actually used and she is critical of deflationists who do not give such an account. However, there is no reason why a deflationist could not add a naturalized account of the use of the concept of truth while still denying that there is anything more to the definition of truth than the disquotation scheme. Dorthy Grover makes this point (Grover 2001, 510) and Misak acknowledges it, but she argues that "the pragmatist will not agree with Grover that we merely have the option, to take up if we wish, of speaking to theories of theories of what-is-true" (Misak 2007, 78). Perhaps we need such an account, but I think that it is very important to

be careful about what we say about truth. Indeed, the key issue is to say exactly what an account of the role of the expression 'true' or of the concept of truth in our talk and thought amounts to, and how it is different from a definition. After all, it may be thought that we have made the solution to the problem of truth too easy, that we are simply suggesting that we call what the pragmatists say about truth an account, rather than a definition, and thereby avoid the standard criticisms that exist in the philosophical literature. I cannot claim here to provide a complete picture of what such an account of our uses of the concept of truth would be, but I think it is possible to make a few points that set out parameters of what such an account should look like.

First, it would be helpful to clarify some terminology and recall how the debate over truth has taken place. Those looking for a definition of truth assume that there is an essence of truth—one thing that all true sentences (or beliefs) share. A definition of truth is supposed to be explanatory; it will tell us why a sentence (or a belief) is true. Accounts are not taken to be explanatory in this way. Definitions also should allow for substitutions, that is the word and its definition should be synonymous. In his critique of James, G. E. Moore argues that 'is true' and 'works' are not the same (Moore 1908).[3] This is the main critique of all pragmatist accounts of

[3] Moore quite carefully argues that things that James says imply that all and only things that work are true, in other words, he tries to show that James has given a definition of truth, even if James does not explicitly say so (Moore 1908).

truth, that truth and what the pragmatists propose are not interchangeable.[4] In an article dated 1906, Peirce criticizes those giving a definition of truth as satisfaction (Schiller, specifically) and makes the same complaint as Moore, that satisfaction and truth are not synonymous (Peirce 1931-35 CP 5.555-558, 6.485). The second of the standard critiques, that pragmatism mixes the definition of truth with a criterion of truth, started with Bertrand Russell (1910/1966, 116) and was followed by many others who tend to say that the pragmatists give epistemic accounts of truth.[5] Although Russell was hardly trying to be helpful, his critique is a useful diagnosis of what goes wrong in general with definitions of truth that are presented as alternatives to the correspondence theory, such as coherence, agreement by experts, satisfaction, etc. We should avoid treating criteria like definitions. We also need to be very careful about general statements about truth, if we are giving an account that lives up to Peirce's maxim that we need only consider what effects of a conceivably practical kind the object may involve.

The central point that Misak makes is that Peirce should be read as giving a naturalized account of truth and related notions. Rather than read him as giving a traditional definition of the essence of truth, she reads him as aiming for an account of the concept of truth as it is put to use in science and everyday discourse. Peirce sometimes seems to have defined truth as

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[4] See Capps (2023) for five standard critiques of the pragmatic account of truth. Chang responds to each of them (Chang 2022, 202).

[5] Fennell (2024) takes on the task of reconstructing a pragmatic account of truth that avoids Russell's criticism without mentioning deflationary accounts. The issue as he sees it is very much a correct understanding of fallibilism.

whatever is fated to be believed at the end of inquiry, however, there is no end of inquiry, there cannot be, because we are always capable of questioning anything. We can reach a point where we as individuals have no doubts and cannot see how to inquire further, but that does not preclude others from inquiring, now or in the future. It also has been noticed by several critics that the truth at the end of inquiry is no more accessible to us than the truth that corresponds to reality in a metaphysical sense (e. g. Chang 2022, 198). We need, as Peirce and Misak both argue, a naturalized, down to earth account of truth that is accessible. Misak says that she does not like the formulation of truth as the end of inquiry, so she proposes to replace it with indefeasibility.

> "C. S. Peirce, the founder of pragmatism, argued that a belief is true if it would be 'indefeasible', or would not be improved upon, or would never lead to disappointment, no matter how far we were to pursue our inquiries" (Misak 2007, 68; citing Peirce 1931-35 *CP* 6. 485, 5. 569).

And:

> "He argues that there is nothing higher or better we can ask of a belief than that it would forever be assertible, by the standards which govern our practices of proper assertion. He insists that the truth theorist start with our current standards of inquiry and state of information, and characterize true belief as that which would be warranted, no matter how far those standards and that information were to be improved" (Misak 2007, 83).

These are accurate renderings of some of what Peirce said. However, when we have satisfied all of our doubts, in the best of circumstances, we only know that we have found a result that is

consistent with all known experience and that has withstood the criticism than has been leveled at it so far. Even with Misak saying that the truth will be judged by current rather than idealized standards, we surely cannot predict the future and know, apparently with certainty, that further inquiry will not overturn our most well-established theory. I do not see how Peirce's statements on truth as presented here, even when forcefully defended by Misak, can be made consistent with his fallibilism. Furthermore, indefeasibility is no more attainable than correspondence or the end of inquiry, if we take fallibilism seriously. Misak tones down her language quite a bit in her 2015 paper, while making the same basic point:

> "Peirce and the deflationist both think that what we do when we assert that a belief is true is nothing more than assert the belief. The classical disquotationalist stops there and will not go further than ' "p" is true if and only if p'. But Peirce would take another step. He would argue that when we assert p, part of what we are doing is asserting that it stands up to experience and reasons here and now, and we bet that it would continue to do so, although we of course might well be mistaken about that". (Misak 2015, 266).

She here acknowledges fallibilism and also makes the statement a little less general by saying the prediction is only part of what we are doing. These modifiers go a long way towards making a pragmatic account of truth that is acceptable, but why do we need to say that we think that a given statement will continue to be justified? If we present all the evidence that we know and we have no (reasonable) doubts about it, we do not add anything by making the prediction. It would seem that the prediction is simply denying the fallibility of the belief in question.

Besides the importance of remaining committed to fallibilism, as I mention above, we also have to be careful making general claims about truth. All accounts of the usage of truth need to be tied clearly to a specific local context and proceed from the ground up. Misak also makes this point:

> The dispute between deflationism and pragmatism looks more and more like a non-dispute. As soon as it is seen that the pragmatist's account of the property of truth is an account that is built from the ground up—from considering first-order inquiry (from considering whether p), then the deflationist can and should be happy with it (2007, 80).

Is this not clear, however, that Peirce's presentation of truth, even as reconstructed by Misak, is really built from the ground up. Peirce and Misak want to hold onto a general statement about what truth is and they justify this by saying that they are explaining the aim of inquiry: "If the naturalist follows Peirce in seeing truth as indefeasible belief, then it is unproblematic for the naturalist to say that we aim at truth" (Misak 2007, 70). I disagree, and argue that we should not claim that there is a general aim of inquiry.[6] Part of saying that inquiry is practical is saying that it is always specific, aiming to answer a particular question in a given context. Saying that the general aim of science is to find the truth is an empty statement that is far too general.

[6] Misak criticizes Arthur Fine's position on this and hints that he may have changed his mind (2007, 70 n. 4), but I seem no sign of that in his later work. On the local and the general see Fine (2001, 118).

Continuing on the topic of bottom-up accounts, I think that it would be very helpful to consider a parallel situation in John Norton's recent groundbreaking work on the problem of induction (Norton 2021, 2024). Norton argues that there is no general or universal justification of induction. Rather, induction is justified in specific concrete scientific settings, using principles that are relevant to that context. This is not to say that nothing general can be said about induction, in his second volume he develops a general theory, but it is still grounded in particular cases, and it is built from the ground up, rather than starting with a completely general principle of induction, as was attempted in the past. He calls his view a material theory of induction, and I think there is an extremely important lesson to be learned from it that we can apply to the search for accounts of truth. Just as there is no general justification of induction, there is no general definition of truth, except for the empty disquotation schema. Rather, in giving an account of the use of the word true, we should be looking at particular scientific or everyday context and see what justifies the usage there. This is in keeping with Peirce's principle that we look for practical effects that made a concrete difference.

If someone were to ask, for example, whether it is really true that cigarette smoking causes lung cancer, we should answer yes, and as justification point to the vast number of robust studies that have shown a link between smoking and lung cancer. I think that we should also stop right there. There is no need to say that furthermore we believe that the studies will never be overturned no matter how much further research is done, let alone that the link between smoking and cancer corresponds with a real link that is found in external reality. No, all we need to do (and really all that we can do) is point to the relevant studies and perhaps also explain why they are good. We say that it is true because we have very good evidence.

How much good evidence is needed before we can call a statement true? Answering that question could put us precariously close to defining truth. I do not think that there can be a general criterion for how much evidence is enough, just as, if we hold fast to the deflationist idea that there is no definition of truth, there cannot be a general criterion for what counts as true. Now my example is extremely simple and unsophisticated, but there is no reason that we cannot look for more nuanced ways of talking about justifying the use of truth in numerous concrete settings. Hasok Chang's work could be seen in exactly this way. He should not say that he is providing a definition of truth, but rather accounts of the use of truth in particular scientific contexts.

In his excellent recent book on realism, Chang defends a new pragmatist stance on the philosophy of science, very congruent with what I have been advocating here. However, Chang also includes a revival of the pragmatic meaning of truth and here I think that he goes too far. First, Chang dismisses deflationary accounts of truth, arguing that they say too little and pointing to Misak's 2007 paper (Chang 2022, 164 n.4). However, Misak's criticisms of the deflationary approach should not at all be seen as a license to define truth. Misak quite clearly endorses the deflationist idea that there is no definition of truth. Furthermore, as we have already seen, it is possible for deflationists to say more about truth than the disquotation scheme, as long as they do not define truth.

Let us look at what Chang proposes as an account of truth. In the context of empirical practices, he says that what we mean by "primary empirical truth" is what he calls operational coherence:

> A statement is true to the extent that there are operationally coherent activities that can be performed by relying on its content (Chang 2022, 167).

The idea is that the content of the statement gives us guidance on how to successfully proceed in our scientific and everyday activities. He argues that truth is not something that stands apart and external to scientific practice, rather, it is inside scientific practice. Knowing how to proceed in a concrete sense is a sign of operational coherence. It is good that this is an account of truth in scientific (and everyday) practice, but in moving beyond the deflationary account, Chang seems to be providing a definition of truth. Indeed, citing Dewey, Chang seems to embrace the viewpoint that the criterion of truth should be what truth actually is:

> The operational coherence of activities relying on a true proposition are not *consequences* or *indications* its truth, from which we may infer the truth. Rather, operational coherence is *constitutive* of truth. As Dewey puts it ([1907] 1977, 68-9: "the effective working of an idea and its truth are one and the same thing—this working being neither the cause nor the evidence of truth but its nature" (Chang 2022, 170).

Recall that above I mentioned Russell's critique, that a criterion for truth is being equated with truth. Chang and Dewey seem to be embracing the view that Russell criticizes. One would think (I did) that Dewey and Chang are giving a traditional definition of what truth is, but Chang takes up Russell's critique and argues that it misses the point (Chang 2022, 203). Furthermore, Chang rejects the traditional view of definition, so he would not claim to be giving a definition of truth that meets the expectations of Moore and Russell. In that sense, he is in agreement with the fundamental point of the deflationists, that there is no definition of truth that meets the

traditional standards. However, he wants to go beyond what the deflationists have provided, and is content to call this a definition, in a new sense of the term. In his book *Inventing Temperature*, he tells us that his sympathies lie with the idea of meaning as use, which is connected to the later work of Wittgenstein (Chang 2004, 150-151). I can agree with him here, but short of having a worked out philosophy of language, I would argue that it is important to emphasize that pragmatists are not giving a definition of truth in the traditional sense. Deflation gives us a path for doing that.

Of course, Chang's general project includes keeping many traditional terms but recasting them in a new philosophical light. He wants to maintain the term 'realism', as well 'truth' and, it appears, 'definition.' I do believe that Chang is correct when he says that we want to be able to say that our theories are true, not just that they are warranted, as Dewey proposes, or supported by all the evidence so far (Chang 2022, 205-206). Holly Andersen also makes a good case for maintaining every day and scientific uses of the word true (Andersen 2023, 95 ff.). I would argue that just as fallibilists can say that they know something, the deflationist can also use the word 'true.' Andersen and Chang are surely correct that we continue to say that things are true in everyday and in scientific contexts, even if a static binary account of truth paired with fallibilism would seem to incline us to say that officially, we never reach truth, rather just highly justified beliefs and statements. The solution here is to reject the static binary account of truth that is promoted by traditional philosophers. Those arguing that a fallibilist cannot use words like 'I know' or 'is true' are simply equating knowledge with certainty, or truth with a static and eternal conception, and that simply begs the question against the pragmatist understanding of the knowledge and the truth that we have that is short

of certainty. Similarly, I would argue that the deflationist can use the word ‘true’ without providing a traditional definition. It is enough to give an account of how the word ‘true’ is used.

Conclusion

The deflationists have been at pains to argue that a substantial definition of truth is not necessary. The classical pragmatists were much more trying to give an account of the role the truth plays in inquiry. The difference really is one of emphasis and both sides should be able to accept the position of the other. However, I believe it is fair to say that the classical pragmatists did not make it clear enough that what they said about truth should not be taken as a traditional definition, given that everyone seemed to take them to be giving such a definition. Indeed, Misak has complained that the point that Peirce was not trying to define truth has needed to be stressed over and over (Misak 2004, vii-viii). My point is that the diagnosis of the problem with truth, that we should not be giving a traditional definition because there is no universal essence of truth, only became clear with the advent of deflationary accounts. In this sense, deflationary accounts made a real advance over classical pragmatism, I would argue, even if pragmatists were trying to do the right thing all along. This is why I think that new pragmatists should embrace deflationary accounts of truth and apply their common stance to understanding science as a practice.

I heard a talk by Amie Thomasson in Berkeley that provides a way for pragmatists and deflationists to move forward. She suggests turning to systemic functional linguistics to provide an account of our uses of truth, starting with our practices of acceptance and rejection of propositions. Such an approach, she says, “gives us reason to think that we can fully account for

the presence of truth-talk in our lives and theories, without the need to 'posit' some property we are tracking, which requires a kind of worldly 'explanation'" (Thomasson 2025). I find her approach to be very attractive and see it as a way of combining the deflationary approach and what pragmatists have said about truth.

Philosophers of science should follow the common core of principles that define pragmatic philosophy as they try to understand scientific practice. Indeed, these teachings of pragmatism have already been accepted widely by philosophers of science. My sketch of the core teachings of pragmatism cannot claim to be definitive, undoubtedly one could add further commitments or refine what has been given here, but I hope to have set out ideas that will be useful in the study of science, as would be expected of a pragmatic stance.

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