

WARD E. JONES

IS SCIENTIFIC THEORY-COMMITMENT DOXASTIC OR PRACTICAL?

ABSTRACT. Associated with Bayesianism is the claim that insofar as there is anything like scientific theory-commitment, it is not a doxastic commitment to the truth of the theory or any proposition involving the theory, but is rather an essentially practical commitment to behaving in accordance with a theory. While there are a number of *a priori* reasons to think that this *should* be true, there is strong *a posteriori* reason to think that it is not in fact true of current scientific practice. After outlining a feature that distinguishes doxastic from practical commitment, I present empirical evidence that suggests that, like perhaps all other theoretical commitment, scientific theory-commitment is doxastic.

Scientific communities are defined by the general subject matter of the theories that they research and defend in their publications. It is to some set of these theories that the members of a community are committed. Commitment to contentful entities like theories can be either (i) a *doxastic* commitment to the truth of the theory or to some proposition about the theory, or (ii) a *practical* commitment to behaving in accordance with the theory. Which best characterizes an individual scientist's or scientific community's commitment? The contention in this paper is that there is some reason to think that it is contingently true that scientific theory commitment is generally a species of doxastic commitment. When an individual or community commits to a theory, that individual or the individuals that make up that community generally adopt a doxastic state. Much is at stake here. First, science is an extremely important institution to us, and one common and plausible view of its importance is that science is a good source of beliefs. We look to scientific experts to inform us about what the world is like with respect to many central aspects of our lives. We believe the theories to which scientists commit themselves, and we think that our beliefs are in better shape because of this.¹ If it were to turn out that scientists do not *themselves* believe the theories that they accept, then our tendency to believe their commitments is in danger of being an inappropriate use of science. Secondly, the nature of theory-commitment affects those of us who study science as philosophers, historians, and sociologists. In what is no doubt a reflection of the previous point, the central issue of concern in the study of science is the nature of the processes that lead up to scientific theory-commitment. We spend our time studying the processes leading up



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to theory-commitment because it is scientific theory-commitment that is so influential to the rest of the world. It immediately follows that the *nature* of theory-commitment will be of central importance as well. As we will see doxastic and practical commitments have deeply divergent natures, and so if scientific theory-commitment is doxastic rather than practical, then we must bear this in mind in our studies of science.

1.

A primary target in this paper might be called ‘Bayesian descriptivism’, the position that Bayesian probabilification has considerable power in describing the actual workings of science. Central to a Bayesian descriptive account of science is an emphasis on the scientist’s subjective assignment of probabilities to theories. The assignment of probabilities, Bayesians say, is the right, proper, and *only* doxastic attitude to take towards theories. Scientists do not, nor indeed should they, doxastically *accept* or *commit* themselves to a particular theory or set of theories. A scientist’s doxastic attitude to a theory is exhausted by probabilification.

In an early and influential application of Bayesianism to science, Richard Jeffrey claims that ‘the proper role of scientists is to provide the rational agents in the society which they represent with probabilities for hypotheses ...’.² Rather than *favoring* any theory or theories *per se*, the purpose of research is to assign probabilities to theories so that these theories can be used for whatever purposes. Scientists are to construct an accurate assignment of probabilities, and to make those probabilities available for interested parties to use. Making probabilities available allows individuals to make better-informed decisions in whatever actions – teaching, advising, researching, developing technology – they choose to pursue. As Jeffrey points out, scientists best serve those purposes when they assign probabilities without favor and without endorsement, for by assigning probabilities to a range of theories (without favor), the scientist can leave open the possibility that someone may wish to make use of a theory with lesser probability than another, because, say, it is easier to use, teach, or apply in the development of technology. Favoring, asserting, or committing to a single theory, however, would not encourage such flexibility in utilization.

Whatever status this has as a *normative* claim about what scientists should be doing, its status as a *descriptive* claim about what scientists actually do is highly dubious. Scientists and scientific communities express behavior that indicates more than a mere assignment of probabilities to theories. They clearly favor certain theories and groups of theories over

others. This is seen in their lectures, their articles, and their texts; scientists espouse some theories, and disparage others. Favoring is also seen in the pursuit of research; research projects are built upon favored theories. As the Bayesian sympathizer Mark Kaplan writes,

After all, the practice of defending proposition . . . is, at least to us as investigators, a very important part of our behavioral repertoire. We devote a great deal of time and energy to the wealth of books, lectures and teaching which are the conspicuous products of this practice. And we manifest great interest in what investigators are willing to defend and in the propriety of their willingness to defend what they do.³

The very notion of ‘defending’ some proposition or theory, which is a prominent aspect of science, embodies a favoring of that proposition or theory over others. Far from merely pinning numbers on theories, a large part of scientific methodology appears to involve scientists’ siding with them.

The Bayesian descriptivist is thus pushed into a dilemma. She must recognize that (i) scientists commit themselves to theories, however she does not want to either (ii) relinquish the descriptive power of Bayes’ Theorem or (iii) give up advocating probabilification as the right and exclusive doxastic attitude to take towards theories. It would seem that the best response to this problem is to categorize scientists’ favoring of theories not as a doxastic commitment to the truth of the theory, but as a *practical* commitment. This is Kaplan’s response:

My suggestion is this: that we view “X accepts P” as nothing more than shorthand for “X would defend P were her aim to defend the truth”.⁴

This is not a suggestion for how we, or scientists, should view theories themselves. It is a suggestion for how we – those of us who study or take an interest in science and its history – should view the phenomenon of theory acceptance *as it exists in science*. It is a descriptive claim about scientists and their relations to theories. When a scientist favors one theory over another, Kaplan suggests that we see her as making a decision to do something with that theory, say, to teach it or use it in making calculations. Even the decision to base further research on one theory rather than another, says Kaplan, is a practical decision. The Bayesian descriptivist takes theory-commitment to be *informed* by the probability that he assigns to a theory, but in taking commitment to be a practical affair, he will see it as being further informed by the theory’s possession of other properties that are useful to the scientist in whatever context she is using it: the theory is simple to use, say, or easy to teach. Probability plays just one role in this decision matrix, the product of which is a practical commitment.

The aim of this paper is to argue that this route out of the Bayesian descriptivist dilemma is blocked. The claim that scientific theory-

commitment is generally practical, when taken as a descriptive claim about how scientists behave, is not an accurate reflection of current scientific practice. Whether motivated by Bayesianism or, indeed, any other consideration, we have no reason to treat scientific theory-commitment as the product of a practically motivated decision. In general, scientific theory commitment is doxastic.

2.

With Kaplan, I take it to be a datum that individual scientists and communities express behavior, with reference to the theories and claims that define their field, that is properly described as ‘favoring’ or ‘disfavoring’. I will use the notion of ‘theoretical commitment’ to describe whatever the relationship is between the members of a scientific community and such theories or claims, such that this relationship *explains* the individual’s or community’s (dis)favoring behavior. If an individual or scientific community is committed to a theory or group of theories regarding its subject matter, then it will have a disposition to behave in some way – favoring or disfavoring – reflective of that commitment, and the natural way to explain the latter behavior is to appeal to the former commitment.

Like a proposition, a picture, or a figment of the imagination, a scientific theory is a contentful entity. Insofar as a theory can be about or represent something else, it can be true or false. The members of a scientific community may make commitments to many things that are not contentful entities, including each other. It is these other sorts of commitments that Kuhn sought to capture, along with theoretical commitment, under the notion of a ‘paradigm’.⁵ The present paper is concerned only with a community’s commitment to entities that can be correct or incorrect, true or false.

There are three possible types of commitments to contentful entities. One can commit oneself (1) to the truth-status of the entity, (2) to the truth-status of some proposition about the entity, or (3) to behaving in some way with regard to, or in accordance with, the entity. Because of the inclusion of the notion of truth in characterizing the first two types of commitments, the first two are types of doxastic commitments. The third is practical commitment.

The first type of doxastic commitment to a theory includes any one of a range of possible attitudes to the truth-status of the theory itself. Among them are full or certain belief that the theory is true, and partial belief that the theory is true. One can also commit to a theory by assigning what one takes to be a high enough (precise or approximate) probability

to that theory. Similarly, one can commit to a theory by assigning it a greater probability relative to other theories. Thus, this category of doxastic attitude includes not only the range of what might be called 'Lockean' full and partial beliefs, but also the conception of theory-commitment as probability-assignment-beyond-a-threshold.⁶

Notice that this first type of doxastic commitment includes both exclusive and nonexclusive attitudes. Any plausible account of scientific theory-commitment must allow for the fact that while a particular instance of theory-commitment may be exclusive, it is not necessarily so. *Holding a full belief in a theory* and *favoring a theory over all competitors* are both attitudes that limit commitment to a single theory. However, a community can be committed to a whole range of theories or models, some of which may contradict others. Committing to a theory by *assigning a probability above a certain threshold*, for example, is not exclusive; in this sense any number of contending theories may be simultaneously committed to. Similarly, *theory-rejection* is, on any plausible account of it, a form of nonexclusive commitment; one will reject any number of theories at once when one assesses their probability to be far too low compared to other available theories. Nonexclusive commitment may occur for a range of reasons: the community may not feel justified in narrowing its commitments to a single theory, it may recognize the indeterminacy of the theory to which it is committed, or it may be committed to different theories for different reasons or purposes.

A second type of doxastic commitment that one can take to a theory involves committing oneself to the theory's having some property. The difference between this and the previous doxastic commitment is that one believes not the theory itself, but a proposition *about* T. In *Progress and its Problems*, Larry Laudan endorses what he calls a 'problem-solving' view of science. Central to this view is the following claim:

The first and essential acid test for any theory is whether it provides acceptable answers to interesting questions.⁷

To commit to a theory, Laudan says, is to believe that it is and will continue to be useful in solving what the community takes to be significant problems. In *The Scientific Image*, Bas van Fraassen argues that, with respect to theories making reference to unobservable entities, scientists should limit their commitment to belief in the theory's empirical adequacy. So to commit oneself to a theory T *à la* van Fraassen would be to believe the proposition 'T is and will continue to be empirically adequate'.⁸

In Laudan's and van Fraassen's versions, theory-commitment to a theory or group of theories T is not doxastic commitment to the truth status of T *per se*, but rather doxastic commitment to a proposition of the form 'T

possesses a property P'. Advocates of such theories tend to be attracted, as both van Fraassen and Laudan are, by the relative epistemic safety of such a position. The belief that T possesses P, they hold, will be more readily justifiable than the belief that T is true.

In stark contrast to these two types of commitment, practical commitment to a theory is not a commitment to the truth of it or any proposition about it. Rather, it is a commitment to behaving in a particular way with regard to the theory. A detailed and sophisticated account of practical theory-commitment can be found in L. Jonathan Cohen's *An Essay on Belief and Acceptance*.

To accept that p is to have or adopt a policy of deeming, positing, or postulating that p – i.e., of including that proposition or rule among one's premises for deciding what to do or think in a particular context, whether or not one feels it to be true that p .⁹

The belief that p , on the other hand, is connected not to action but to 'feeling':

belief that p is a disposition, when one is attending to issues raised, or items referred to, by the proposition that p , normally to feel it true that p and false that $not-p$.¹⁰

Acceptance often accompanies belief, and in the usual case one decides to accept p because one already believes that p . In spite of this usual accompaniment, though, belief and acceptance are distinct mental states. While belief is involuntary, acceptance is under our control. Even if one believes something, one has the choice to accept or not accept it. For this reason, the belief that p can come without acceptance that p , and vice versa. A lawyer might accept that, and thus behave as if, her client is innocent, even while she believes that he is not.

It is acceptance and not belief, Cohen thinks, that properly belongs in science. When a scientist asserts that p , he is making a claim of acceptance, asserting not (necessarily) that p is true, but that he is

willing to go along with that proposition, and anything it is seen to entail, as a premise – one among many – for his predictions, explanations, further research, etc. And an involuntary belief that p would not be an adequate substitute for the scientist's voluntary acceptance that p since it would not entail this policy in the choice of premises.¹¹

The scientist's favoring of theories, Cohen says, does not flow (solely) from his belief in the truth of that theory. Rather, his avowal and defense of a particular theory is an expression of the voluntary commitment to using that theory in his teaching and future research. The scientist *decides* to stick with that theory, to use it in the future. Indeed, Cohen thinks that it is better that the scientist does not believe that his favored theory is true:

There is a danger that possession of a belief that p might make him less ready to change his mind about accepting that p if new evidence crops up or a better theory becomes available.¹²

A scientist's favoring of a theory is best characterized, Cohen concludes, as voluntary and practical acceptance of that theory, and not by a doxastic commitment towards it.

3.

Faced with the issue of whether scientific theory-commitment is either practical or doxastic, it might be thought that there is no real issue here. Won't the correct account of scientific theory-favoring be some combination of practical and doxastic commitment?

While some such account of theory-commitment may be the right one, it is important to see that this option will please defenders of neither the practical account nor the doxastic account. The defender of the practical account says that there is no doxastic commitment involved in theory-commitment. Even if doxastic commitment is present in science, theory-commitment is something else altogether. Theory-commitment is practical commitment, and practical commitment is made in addition to, or in lieu of, any doxastic commitment on the part of scientists. The defender of the doxastic account of theory-commitment, on the other hand, does not deny that scientists make practical commitments to theories, but he claims that practical commitment tends to be constitutive of doxastic commitment. Beliefs and other doxastic commitments bring with them a (*prima facie* but defeasible) commitment to acting in certain ways under certain conditions. When an individual makes a doxastic commitment he *ipso facto* makes a practical decision to, for example, defend the theory if it is under attack or pursue research in certain directions. Practical commitment exists in virtue of doxastic commitment.

These two positions are, as stated, mutually exclusive. The defender of the claim that the correct understanding of theory-commitment is either some combination of these two states or somewhere in between must stake out a third position in this conceptual space. Perhaps scientific theory-commitment is some combination of both doxastic and practical attitudes – in which the latter does not depend upon the former in the way that action depends upon belief – towards a theory. Or perhaps it will be some disjunctive amalgam of the other two, in which on some occasions scientific favoring is practical while on other occasions it is doxastic. In any case, an easy compromise is not on the cards.

4.

The defender of the practical account of theory-commitment apparently has a number of *a priori* considerations in her favor. Both theories and doxastic commitments have properties that clearly count against scientists' taking the latter towards the former. (1) Many or all theories are underdetermined by the evidence for their truth. (2) Many or all scientific theories are idealizations. (3) Many or all theories have anomalies. (4) A number of paradoxes, including the lottery paradox and the paradox of the preface, accompany doxastic commitment. (5) Doxastic commitment cannot explain how it is rational to use different (and perhaps competing) theories in different contexts.

Singly, each of these problems is formidable, and the conjunction of them makes a hearty case against taking doxastic commitments to scientific theories. However, the problem with all of these considerations is that they suggest not that scientific theory-commitment *is* practical, but rather that it *should* be. While the fact that a theory is underdetermined means that a scientist should not take a doxastic commitment to it, it surely does not mean that a scientist will not do so. While the fact that a theory is an idealization or has anomalies means that a scientist should not take a doxastic commitment, it does not mean that a scientist will not do so. While taking a doxastic attitude towards a theory means that a scientist is potentially susceptible to lottery and preface paradoxes, this does not necessarily prevent her from taking this attitude. And while taking a doxastic attitude towards a theory may make a scientist inflexible in her use of that theory across all contexts, this does not mean that she will not take such a doxastic attitude.

Whether or not the current practice of theory-commitment is in fact practical or doxastic is a contingent matter, to be empirically determined. The claim being made here is not that scientific theory-acceptance *should* be doxastic. Such a claim would, indeed, be non-empirical. Each of the five worries above are clearly relevant to the non-empirical question whether theory-commitment should be doxastic, but none of them are, as they stand, relevant to the question whether it is or is not, in reality, doxastic.

Appreciating its contingency is essential to understanding the strength and scope of the claim being made in this paper. I do not rule out the possibility that theory-commitment either was or will be predominantly non-doxastic. Nor do I rule out the possibility that current normal theory acceptance is sometimes or at some stages wholly practical. There may be instances of scientific communities accepting theories without taking any doxastic commitment to them. It may also be possible that a common

or standard attitude taken in some stages of investigation is non-doxastic. The claim is that now, generally, scientific theory-commitment is doxastic commitment, and thus that those of us who study science must keep that in mind as we describe the communities that we study.

5.

In this section, I argue that doxastic commitment is characterized by a property that does not characterize practical commitment, and in the next section, I present some empirical evidence that scientific theory-commitment is in fact characterized by this property. The property in question is this: those who make doxastic commitments have limitations regarding how they can account for why they have them. S cannot *non-epistemically* explain her doxastic commitment towards p without her commitment to that explanation leading S to lose her commitment towards p . Scientists appear to have the same limitations with regard to their own theory-commitments.

An epistemic explanation of S's commitment to p portrays S as being committed to p in order to be committed to a truth about the subject matter at hand. Non-epistemic explanations, on the other hand, portray S's commitment as otherwise motivated. The following explanation, which is ambiguous between an epistemic and a non-epistemic version, illustrates this:

- (1) S is doxastically committed to theory T because S judges T to be more elegant than any of its rivals.

What makes this explanation epistemic or non-epistemic is the scientist's own view of the matter. An epistemic version goes like this:

- (1E) S is doxastically committed to T because S judges T to be more elegant than any of its rivals, and S believes that elegance is correlated with truth.

A non-epistemic version goes like this:

- (1N) S is doxastically committed to T because S judges T to be more elegant than any of its rivals, and S simply prefers elegant theories.¹³

(1E) portrays S as being committed to T because he thinks that T is elegant, and because he has the belief that the elegance of a theory is evidence for

its truth. S is committed to T in order to be committed to a truth about the subject matter in question. Were we (as explainers) convinced that S does not hold some background belief like this, however, then our explanation will be non-epistemic. (1N) portrays S as being committed to T because he values elegant theories, but not because he thinks elegance is indicative of truth. S is committed to T for some purpose other than being committed to a truth about the matter at hand.

No believer S will offer or accept an explanation of the form (1N) of his own doxastic theory-commitments, for the following two attitudes are incompatible with each other:

- (2) I have a doxastic commitment to *p*.
- (3) I accept a non-epistemic explanation of my doxastic commitment to *p*.

This is not, however, true of practical commitments.

The notion of a doxastic commitment tells us that to be doxastically committed to *p* is to think that *p* is true. A doxastic commitment to *p* is a commitment that *p* is true. It follows that I must see my own doxastic commitment as an attempt to grasp some truth about the matter at hand. I have to see my doxastic commitments as – in a phrase from Bernard Williams¹⁴ – ‘aiming at truth’ regarding the subject matter to which the proposition *p* belongs. ‘Aiming at truth’, however, only partially captures what is unique about doxastic states, for non-doxastic states like *guessing* also involve aiming at truth; to guess is to guess at the truth.¹⁵ The difference between doxastic states and other states that aim at truth – like guessing – is that the latter can be seen by the believer as undertaken in order to achieve some goal or meet some norm *other* than that of truth. I can, in full awareness, guess *in order to* pass a test or win a trivia game. In such situations, guessing is still aiming at truth, but only derivatively so, derivative of the primary norm of passing a test or winning a game. It is possible for the guesser to recognize this, to see the stating of a truth as the indirect norm of her guessing, second to other goals: ‘I do not think that *p* is right, but I must say *something* if I am going to win ...’.

One could not say the same about one’s beliefs: ‘I must believe *something* if I am going to win ...’. The believer, in contrast to the guesser, must not see her belief – her commitment that something is true – as not primarily aiming at truth, as being held because of non-truth-indicative considerations. If I reflect upon a belief, I must see it as being held first and foremost in order to gain a truth. I cannot think that its possession is dependent upon other goals I may have; I cannot see myself as believing

that p (rather than that q) *so that* I will pass a test or win a game. To do so would be to undercut the belief, to see it as something other than a belief, to see it as, for example, a guess. We all recognize, of course, that certain of our beliefs bring us comfort or allow us to earn money, but each of these must be seen, as being derivative of the goal of truth. When I hold a belief, I must see the possession of a truth as the primary aim of my believing what I do.

In acknowledging that I have a doxastic commitment towards p , I am characterizing myself as being committed to something's being the truth about the subject matter at hand. I see myself as having, in holding this state, first and foremost the goal of truth. However, in explaining that commitment non-epistemically I effectively take such characterization away. If I were to explain my doxastic commitment to p non-epistemically, then I would be seeing my commitment as aiming for some goal other than truth, or for no goal at all. I would understand myself as believing in order to bring myself happiness or fame, because I want a simple or elegant theory, or because I am in such-and-such a social position. Therefore, holding a doxastic commitment is essentially incompatible with non-epistemically explaining why one has that commitment; the two cannot exist, in full force, together. To consider and retain one of my current doxastic states requires seeing it as fundamentally aiming at truth, but to non-epistemically explain that belief is to see it as aiming primarily at some other goal, or at no goal at all. Thus the incompatibility. I cannot see a state as doxastic while fully endorsing a non-epistemic explanation of it. It does not make sense to see myself as believing that p is true and meanwhile being convinced that I do so because of factors that have nothing to do with p 's being true. The combination of the two attitudes towards my own beliefs, or any of my own doxastic attitudes, makes for nonsense.

I am not claiming that no one else can see my doxastic attitudes as not aiming primarily at truth. If someone were to non-epistemically explain my commitment to p , then she would not see my commitment as being first and foremost truth-aimed; on the contrary, she would see me as being committed in order to gain some non-epistemic goal, or to gain no goal at all. I have to see my doxastic commitments as aiming at truth, but no one else has to see each of my doxastic commitments in that way.¹⁶ Nor am I claiming that I cannot hold a doxastic commitment without explaining it at all. My (i) being committed to the truth of p and meanwhile (ii) not having any idea why I am committed to the truth of p , are not incompatible attitudes. Lastly, I am not claiming that doxastic commitments cannot be motivated by non-epistemic concerns. The claim is that such concerns cannot be recognized by the subject without lessening the commitment. Even

if, for example, I appreciate that making some doxastic commitment would have certain non-epistemic benefits, and even if those benefits lead me to come to make that commitment, I could not think that my commitment currently depends upon my appreciation of those benefits. Non-epistemic reasons must somehow cover their own tracks. If they lead to doxastic commitment, then they must do so without the subject's being aware that they are doing so.

In sum, doxastic commitments have the following feature, which I call the *First-person Constraint on Doxastic Explanation*:

- (FPC) The *stronger* my conviction that the correct explanation of my doxastic commitment to p is non-epistemic, the *weaker* will be the commitment that it explains. As the explanation gets stronger, so will my tendency to give up the commitment being explained.

You can say that I am doxastically committed to a theory T because T is elegant or simple, but unless I think that elegance or simplicity is indicative of T's likelihood to be true, my belief that this is true will undercut, or lessen, my commitment to T. The phenomenon described in (FPC) has been asserted by a number of writers. Michael Ayers writes, 'In so far as we doubt that grounds wholly determine our belief, so far is our belief itself subjectively insecure', and Barry Stroud writes, '... some explanations of the origin of a belief are such that once we accept them we can no longer hold the belief in question ...'.¹⁷

There are reasons to believe that (FPC) is true of our doxastic commitments, other than that a non-epistemic explanation of one's belief is incompatible with holding that belief. I will briefly mention two. First, I appeal to you try and come up with a counterexample in your own belief system. Can you find a current belief that you are convinced you hold because of non-epistemic determinants, a proposition p such that your believing that p depends upon the fact that your doing so brings you benefit? (FPC) predicts that, and explains why, you cannot do so.

Now it is very easy to *imagine* coming across a counterexample to (FPC): Dr. S tells us that he believes theory T because he invented it and he wants to be famous. It is clear, however, that we would not take Dr. S's claims at face value. We will not accept his assertion of both the belief and the explanation. Instead, we will tend to re-interpret Dr. S's statements. Either he believes T and he is joking about the non-epistemic explanation, or he is merely confessing that he does not really believe T at all. Put into philosophical lingo, (FPC) is a part of the attribution conditions for beliefs. We tend not to believe someone when he presents us

with a counterexample of (FPC), and will instead re-interpret anyone who appears to violate (FPC) in such a way that we understand him to be either (i) not asserting that p , or (ii) not asserting a non-epistemic explanation of his belief that p .

Practical commitment is not constrained in the same way as doxastic commitment. I can in principle practically commit myself to a proposition or theory while appreciating that the explanation for my doing so is non-epistemic. In drawing his contrast between acceptance and belief, Cohen writes that ‘the reasons for accepting that p need not always be epistemic ones: they might be ethical or prudential’.¹⁸ I suspect that Cohen recognizes that acceptance, unlike belief, is not constrained by (FPC). The acceptor can in principle admit that she accepts for a non-epistemic reason, while the believer cannot. The lawyer who accepts that her client is innocent while not believing that he is so can explain that this is simply a part of her job; but the lawyer who believes that her client is innocent cannot say the same.

This is not a contingent feature of practical commitment, one that can be given up by the defender of theory-commitment as practical commitment. If acceptance were such that we could not see it as being motivated by non-epistemic concerns, it is hard to see what difference would be left to separate it from doxastic commitment. Acceptance would no longer be, for example, under our control; it could no longer be the end result of consciously considering our non-epistemic ends. It follows that if scientific theory-commitment is constrained in the manner described in (FPC), then we have reason to believe that scientific theory-commitment is doxastic.

6.

There is striking empirical evidence that scientists adhere to (FPC).

When, for example, scientists explain other scientists’ acceptances non-epistemically, they will inevitably resort to epistemic explanations when they explain their own. One recent example is provided by theoretical biologist Stephen J. Gould’s *Wonderful Life*, a detailed and sympathetic account of work done in the Burgess Shale, a Canadian repository of fossils first discovered and studied in the early twentieth-century. In his review of *Wonderful Life*, James Gleick observes:

Mr. Gould . . . is a scientist, not a journalist, and he must tell this story – about colleagues and friends – from the inside.

But I think that Stephen Jay Gould, the insider, falls into a trap that Stephen Jay Gould, the historian, has often warned against. A myth about science suggests that new theories arise when they are necessary to explain new facts. The messy and more interesting reality

is that 'facts' themselves tend to depend on the theories of the fact finders. When writing about Walcott's mistakes, safely in the past, Mr. Gould shows in detail how scientific decisions were colored by cultural and philosophical prejudices

Yet when writing about his colleagues, Mr. Gould lets his readers take away a simpler impression, that a rational group of scientists developed a new view of evolution because they received new evidence from the Burgess fossils.¹⁹

In explaining the commitment to theories with which he disagrees, Gould makes free use of non-epistemic determinants. Yet when it comes to explaining his own commitments, Gould turns rationalist. Gleick chastises Gould for refusing to non-epistemically explain his own commitments, but it is not clear that he is right to do so. If Gould's commitment to his favored theories is doxastic, then (FPC) predicts that Gould will epistemically explain work with which he agrees. He could not have done otherwise while accepting the claims that he does. This looks like a nice record of a scientific theory-commitment adhering to (FPC).

A broader and more structured study of theory-commitment explanation patterns is available in *Opening Pandora's Box*, a sociological study in which Gerald Gilbert and Michael Mulkay analyze interviews with a number of working biochemists.²⁰ All of the scientists interviewed were involved in a debate over the nature of oxidative phosphorylation in the 1970's. The interviews took place while the debate was still unresolved. In their analysis of the interviews, Gilbert and Mulkay describe what they call an "asymmetrical structure" in the biochemists' explanations of their own and each others' commitments. First, they found that

the actions and judgments of those scientists who are depicted as being or as having been in error are characterised and explained . . . in terms of various special attributes which they possess as individuals or as certain kinds of social actor.

In other words, scientists often explain competing positions as being non-epistemically determined. Competing scientists are presented as being in error because they, for example, are 'strong individuals who want to interpret everything in terms of their theories' and who, consequently, 'bend the data'. Alternatively, they are characterized as 'strong personalities', 'dogmatic' and inclined to avoid awkward questions, as being misled by publications which had not been subject to proper refereeing, as irrational, or as having too much invested in a theory to give it up.²¹ In stark contrast, however, Gilbert and Mulkay found that "speakers link [what they take to be] the correct view directly to experimental evidence".²² This is true whichever side the speaker takes in the debate. This 'asymmetrical structure' is well explained by (FCP) on the assumption that theory-commitment is doxastic.

(FCP) also predicts that, if theory-commitment is doxastic, then scientists will be resistant to non-epistemic explanations of their commitments. This prediction is borne out in the interactions between scientists and those involved in what is known as the Sociology of Scientific Knowledge (SSK). The proponents of SSK present themselves as offering neutral, naturalistic explanations of theory-commitment.

The sociology of knowledge is emphatically not, as its critics often mistakenly believe, itself a denigration of science; on the contrary, it is in many ways modeled upon scientific investigation, and any claims to credibility it comes to have must be closely related to those of science itself. The sociology of knowledge is a matter-of-fact, empirical field of study which happens to include, among its subject-matter, the knowledge and culture of science.²³

The philosopher of science Michael Friedman agrees with this, writing that there is simply no possibility of conflict or competition between ‘non-naturalistic’, philosophical investigations of reason, on the one hand, and descriptive, empirical sociology of science, on the other.²⁴

If SSK were only a naturalistic account of theory-commitment, then Barnes and Friedman would be correct. There is no incompatibility in principle between holding a belief and offering a naturalistic explanation of a belief. If there were, then naturalistic accounts of epistemic justification would not be seen to have any plausibility whatsoever.²⁵

In spite of their official pronouncements, SSK is not, at least not always, simply a naturalistic account of theory-commitment. Its proponents frequently offer non-epistemic explanations of scientific theory-commitments. I briefly look at two examples.

In his widely-read “Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaption by German Physicists and Mathematicians to a Hostile Intellectual Milieu”, Paul Forman rejects the use of evidential considerations to explain the rise of the commitment to acausality among central European physicists. He writes that

no specific developments in physics . . . could plausibly be regarded as the source of such acausal convictions . . .

When our converts attempted to demonstrate the necessity for this renunciation of causality, their arguments, as often as not, ought logically to have led to the opposite conclusion.

Instead, he appeals to the interest that these scientists had with not going against intellectual trends of the time. He writes that ‘what we are dealing with is, essentially, a capitulation to . . . intellectual currents in the German academic world . . .’.²⁶

In his “Phrenological Knowledge and the Social Structure of Early Nineteenth Century Edinburgh”, Steven Shapin similarly rejects altogether

the notion that the arguments of the debates between phrenologists and their opponents had any major force in determining theory-commitments. In contrast to Foreman, however, Shapin's positive account relies less on the self-interest of scientists than on their social position.

They were committed to this position . . . because they had a prior and more fundamental commitment to anti-elitism in knowledge and broad participation in culture.

[T]he Edinburgh phrenologists' commitment . . . derived from their social values and interests.

Why, then, should the early elaborators of phrenology in Edinburgh . . . be attracted to Gall's phrenological doctrines? The answer lies primarily in the outsider social and cultural status of . . . many of the members of the Phrenological Society.²⁷

Like Foreman, Shapin defends an account of theory-commitment that is not just naturalistic, but thoroughly and explicitly non-epistemic.

While perhaps thinking of itself as neutral, SSK will not be so seen to scientists themselves, if proponents of SSK offer up non-epistemic explanations of theory commitment and if scientific theory-commitment is doxastic. If theory-commitment is doxastic, then (FPC) predicts that a scientist will not be able to accept a non-epistemic account of her own theory-commitment, and conflict will arise between proponents of SSK and scientists.

In yet another prominent and thoroughly non-epistemic SSK study, the sociologist Bruno Latour observed work in a biochemistry laboratory over a period of two years. He asked the head of the institute in which the laboratory was based, Jonas Salk, to write a forward for Latour and Steve Woolgar's analysis of those observations, published as *Laboratory Life*. Although his comments are short, Salk takes the opportunity to make clear that he does not "agree with the details of this book", and he finds "it slightly uncomfortable or even painful in places".²⁸

Jay Labinger, a chemist, makes similar comments in a review of the SSK literature. Discussing H.M. Collin's *Changing Order*, Labinger writes

After presenting his interpretation, [Collins] comments that "scientists are resistant to the sort of account of experimentation that I have just given". This choice of phrase calls to mind a microbiologist, noting that the bacteria are resistant to the antibiotic that he has just applied. Why isn't that resistance cause for concern that his account might just possibly be incomplete, or misleading, or distorted in some sense?²⁹

Like Salk, Labinger takes the claims of SSK to be not only explanations, but challenges, a condemnation of their theory-commitments.

We find that scientists Labinger and Salk have difficulty with the non-epistemic claims made in SSK. We find that Gould and the scientists studied by Gilbert and Mulkay non-epistemically explain other scientists' commitments non-epistemically, while resorting to epistemic explanations

of their own. There is a good explanation of such patterns of behavior: scientific theory-commitment is doxastic. As (FPC) tells us, I cannot, in general, hold a doxastic commitment and think I hold it because of considerations that have nothing to do with the truth of that to which I am committed.

7.

A full defense of the claim that scientific theory-commitment is in fact doxastic would require far more structured and thorough empirical evidence than I have at hand. However, I have offered enough empirical evidence to suggest that scientific theory-commitment is doxastic commitment. Scientists behave as if they are doxastically committed to their theories; they appear to be unwilling to accept a theory and also accept a non-epistemic explanation of why they hold that theory. Since we are not so constrained with respect to our practical commitments, this observation supports the claim that theory-commitment is a species of doxastic, rather than practical, commitment. While it may be that scientists *should* only practically commit to theories, *as a matter of fact* they doxastically commit themselves to them.

The defender of the practical account of theory-commitment may respond by suggesting that even if theory-commitment were practical, scientists might still behave in the manner seen in the previous section. It might be that scientific *culture* dictates that it is unacceptable to explain or defend one's practical commitments non-epistemically. Such a restriction does not come from the nature of practical commitment itself, for practical commitments can in principle be non-epistemically accounted for. However, scientific communities put *external* restrictions on commitments, such that any commitment that one makes in front of a scientific community must be seen to be motivated by epistemic concerns. The reason why scientists do not tout non-epistemic concerns in favor of their theories is not that they are doxastically committed to their theories, but rather that there is a social norm against their doing so.

This objection would have some weight were it not true that the phenomenon described by (FPC) extends to theoretical discourse in general. As the epistemologist Richard Foley writes,

Why does it seem so odd for practical reasons, or for other non-epistemic reasons, to override epistemic reasons for believing, making it rational all things considered to believe what is not epistemically rational? That it does seem odd, I think, cannot be denied. Indeed, when people reflect upon what reasons they have to believe something . . . they rarely even consider the practical advantages that might accrue to them by believing it. . . . Likewise,

when someone tries to convince another person that he has reasons to believe something, they rarely even mention the practical benefits that might result from believing it . . .³⁰

In no theoretical confrontational discourse, including science, do we find proponents of positions appealing to the non-epistemic benefits of adopting their position. Defending one's theoretical position by pointing to its practical benefits is a possible dialectical move, and it is significant (if not surprising) that it is not utilized more than it is.³¹ Theoreticians do not make themselves aware of, nor do they generally discuss, non-epistemic reasons for theory-commitment. They do not defend or explain their commitment to theories by appealing benefits that they may gain from those commitments. They do not claim that the theories support their non-epistemic values, or that they will bring them fame or fortune.

The consistency, in this regard, between science and all other theoretical discourses, should come as no surprise. Whatever differences there are between the sciences and other fields, the fact that they all, at least at this point in time, involve the search for more or less correct accounts of their salient aspects of the world, is something that all such fields share. Given that science has such an aim, we should expect nothing less than doxastic commitment from scientists and their communities.

The Bayesian descriptivist says that scientists' doxastic attitude to their theories is exhausted by the assignment of probabilities to theories. In so far as a scientist goes beyond probabilification and commits to a particular theory, her commitment must then be practical. I have argued that this does not accurately reflect current scientific practice. Scientific theory commitment is characterized by a resistance to non-epistemic explanation, and as such it reveals itself to be doxastic. This says nothing about the rationality of Bayesianism, nor about the defense of this position as a normative standard for what science should become. However, as a descriptive project, it faces the significant shortcoming of being unable to count for what scientists are doing when they commit to their theories.

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NOTES

¹ For a defense of the claim that our beliefs are justified because of scientific experts, see Hardwig (1985).

² Jeffrey (1956, p. 26).

³ Kaplan (1980, p. 310).

⁴ Kaplan (1980, p. 311).

- ⁵ Kuhn (1962, Sections III–V).
- ⁶ For the former, see Locke (1689/1975, Book IV, Sections xvi and xix).
- ⁷ Laudan (1977, p. 13).
- ⁸ van Fraassen (1980, Chaps. 3 and 4).
- ⁹ Cohen (1992, p. 4).
- ¹⁰ Cohen (1992, p. 4).
- ¹¹ Cohen (1992, p. 88).
- ¹² Cohen (1992, p. 88).
- ¹³ This example is complicated by the fact that S's doxastic commitment to T is based on S's *further* doxastic commitment to the proposition that T is elegant. Commitment to this latter proposition should be kept distinct from the commitment being explained in the example, which is intended to be something like S's commitment to T's being true, or S's van Fraassen-type or Laudan-type commitment to T.
- ¹⁴ Bernard Williams' (1973) 'Deciding to Believe'.
- ¹⁵ This point comes from Velleman (2000, Chaps. 5 and 11).
- ¹⁶ This is not to say that they do not have to see the vast majority of my beliefs as aiming at truth. There are surely limits to our non-epistemic explanation of someone's beliefs.
- ¹⁷ Ayers (1991, Vol. 1, p. 148) and Stroud (1979, p. 239). The most thorough discussions of (FPC) are to be found in Winters (1979) and Jones (forthcoming).
- ¹⁸ Cohen (1989, p. 369).
- ¹⁹ Gleick (1989).
- ²⁰ Gilbert and Mulkay (1984).
- ²¹ Gilbert and Mulkay (1984, p. 68).
- ²² Gilbert and Mulkay (1984, p. 68.).
- ²³ Barnes (1982, p. xi).
- ²⁴ Friedman (1998, p. 245).
- ²⁵ The most prominent of which are the causal, reliabilist, and truth-tracking theories, all of which offer naturalistic accounts of the conditions under which beliefs are held and/or justified.
- ²⁶ Forman (1971, pp. 90–91 and 86).
- ²⁷ Shapin (1975, pp. 237, 239 and 241).
- ²⁸ Latour and Woolgar (1979, p. 14).
- ²⁹ Labinger (1995, pp. 287–288).
- ³⁰ Foley (1987, pp. 214–215).
- ³¹ Pascal is perhaps the most notable instance of someone appealing to practical benefits of taking a position in order to lead his opponents to his side. See Jones (1998).

REFERENCES

- Ayers, Michael: 1991, *Locke*, Routledge, London.
- Barnes, Barry: 1982, *T. S. Kuhn and Social Science*, Macmillan, London.
- Cohen, L. Jonathan: 1989, 'Belief and Acceptance' *Mind* **98**, 367–389.
- Cohen, L. Jonathan: 1992, *An Essay on Belief and Acceptance*, Oxford University Press, Oxford.

- Foley, Richard: 1987, *The Theory of Epistemic Rationality*, Harvard University Press, Cambridge, MA.
- Forman, Paul: 1971, 'Weimar Culture, Causality and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment', in R. McCormach (ed.), *Historical Studies in the Physical Sciences*, Vol. 3, University of Pennsylvania Press, Philadelphia, pp. 1–115.
- Friedman, Michael: 1998, 'On the Sociology of Scientific Knowledge and its Philosophical Agenda', *Studies in the History and Philosophy of Science* **29**(2), 239–271.
- Gilbert, Gerald and Michael Mulkay: 1984, *Opening Pandora's Box*, Cambridge University Press, Cambridge.
- Gleick, James: 1989, 'Survival of the Luckiest', Review of S. J. Gould's *Wonderful Life*, *New York Times Book Review*, October 22.
- Hardwig, John: 1985, 'Epistemic Dependence', *Journal of Philosophy* **82**(7), 335–349.
- Jeffrey, Richard: 1956, 'Valuation and the Acceptance of Scientific Hypotheses', in *Probability and the Art of Judgment*, Cambridge University Press, Cambridge, pp. 14–29.
- Jones, Ward E.: forthcoming, 'Explaining Our Own Beliefs: Non-Epistemic Believing and Doxastic Instability', *Philosophical Studies*.
- Jones, Ward E.: 1998, 'Self-Deception, Religious Conversion, and Pascal's Wager', *Journal of the History of Philosophy* **36**(2), 167–188.
- Kaplan, Mark: 1981, 'A Bayesian Theory of Rational Acceptance', *Journal of Philosophy* **78**(6), 305–330.
- Kuhn, Thomas: 1962, *The Structure of Scientific Revolutions*, Chicago University Press, Chicago.
- Labinger, Jay: 1995, 'Science as Culture: A View from the Petri Dish', *Social Studies of Science* **25**, 285–306.
- Latour, Bruno and Steve Woolgar: 1979, *Laboratory Life: The Social Construction of Scientific Facts*, Sage, London.
- Laudan, Larry: 1977, *Progress and Its Problems*, University of California Press, Berkeley.
- Locke, John: 1689/1975, *An Essay Concerning Human Understanding*, Oxford University Press, Oxford.
- Shapin, Steven: 1975, 'Phrenological Knowledge and Early Nineteenth-Century Edinburgh', *Annals of Science* **32**, 219–243.
- Stroud, Barry: 1979, 'Evolution and the Necessities of Thought', in Larry Sumner (ed.), *Pragmatism and Purpose*, University of Toronto Press, Toronto.
- van Fraassen, Bas: 1980, *The Scientific Image*, Oxford University Press, Oxford.
- Velleman, J. David: 2000, *The Possibility of Practical Reason*, Oxford University Press, Oxford.
- Williams, Bernard: 1973, *Problems of the Self*, Cambridge University Press, Cambridge.
- Winters, Barbara: 1979, 'Believing at Will', *Journal of Philosophy* **76**, 243–256.

Department of Philosophy
 Rhodes University
 Grahamstown 6140
 South Africa
 E-mail: w.jones@ru.ac.za