# Belief is Not the Issue:
# A Defence of Inference to the Best Explanation

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## Abstract

Defences of inference to the best explanation (IBE) frequently associate IBE with scientific realism, the idea that it is reasonable to believe our best scientific theories. I argue that this linkage is unfortunate. IBE does not warrant belief, since the fact that a theory is the best available explanation does not show it to be (even probably) true. What IBE does warrant is acceptance: taking a proposition as a premise in theoretical and/or practical reasoning. We ought to accept our best scientific theories since they are the theories that are most likely to lead to the goal of science, which is that of knowledge. In support of this claim I invoke Bill Lycan's Panglossian reflections regarding Mother Nature.[1]

1 I am grateful to Alan Musgrave for discussions, often over lunch, regarding the subject of this paper. While I don't expect him to accept my conclusions, I look forward to further lively debates.

## Introduction

A frequent pattern of reasoning, both in the sciences and in everyday life, is that known as 'inference to the best explanation' (IBE). Here's an everyday example. 'I hear a scratching in the wall, the patter of little feet at midnight, my cheese disappears – and I infer that a mouse has come to live with me'.[2] Each of these phenomena – the scratching, the patter, the disappearance of the cheese – could have another explanation. There might even exist a single, alternative explanation that covers them all. But for a variety of reasons, such as simplicity, economy, and plausibility, the mouse hypothesis seems to be the best.

Here's another example. In 1859 Charles Darwin published *On the Origin of Species by Means of Natural Selection*. In that work he cites a variety of phenomena – the geographical distribution of species, the existence of homologous anatomic structures and vestigial organs, the resemblance of embryos of differing species, and the fossil record – and suggests they are better explained given his theory of natural selection than on the alternative view of special creation. His contemporaries would have described this as a 'consilience of inductions', in which a range of different phenomena are seen to be explicable by reference to the one causal principle.[3] But particularly since Darwin was contrasting this potential explanation with another – that of special creation[4] – it is more helpfully viewed as an inference to the best explanation.

Since this pattern of explanation was given its modern name by Gilbert Harman in 1965,[5] its significance has been disputed. Just what, if anything, should be the conclusion of an inference of this kind? Does the fact that something is the best available explanation

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2 Bas C. van Fraassen, *The Scientific Image* Clarendon Library of Logic and Philosophy (Oxford: Clarendon Press, 1980), pp.19–20.

3 John Losee, *A Historical Introduction to the Philosophy of Science* 3rd edition; Opus Books (Oxford: Oxford University Press, 1993), p.133.

4 Neal C. Gillespie, *Charles Darwin and the Problem of Creation* (Chicago: University of Chicago Press, 1979), pp.67–81.

5 Gilbert H. Harman, 'The Inference to the Best Explanation', *The Philosophical Review* 74 (1965), pp.88–95.

give us adequate reason to believe in the existence of the entities that it mentions? Or should we withhold belief, since the explanation in question may be nothing more than 'the best of a bad lot'?[6] Indeed, is there any value in this line of reasoning at all? Is it, perhaps, nothing more than a dressed-up version of the fallacy of affirming the consequent?

A notable feature of these discussions is that they are associated with wider debates regarding scientific realism. Do we have adequate reason to regard our best scientific theories as (approximately or partially) true, even when they speak of entities we could never observe? Or should we conclude merely that our best theories are 'empirically adequate', offering a correct account of the observable regularities of our world? As it happens, most defenders of IBE have been scientific realists, who refuse to believe 'that a false theory would explain, in so satisfactory a manner … several large classes of facts'.[7] Many opponents, on the other hand, have been non-realists, who oppose the idea that IBE warrants belief, particularly belief in unobservable entities.

It is widely acknowledged, even by realists, that the non-realists have a point. After all, to believe a theory means holding it to be true, and we cannot simply assume that the best available explanation will be the true one. The problem here is twofold. Firstly, the true explanation may be one we have not yet discovered. Secondly, at least some of the criteria by which we judge an explanation to be the best available (such as simplicity) are not clearly truth-indicative.

But should the debate regarding IBE be so closely related to that regarding realism? My argument is that this linkage has been unfortunate. It is focused attention on the question of whether IBE gives us adequate reason to believe a theory, that is to say, to hold it to be true. It is this focus on belief that I shall argue is unhelpful. The important issue, as far as IBE is concerned, is not whether we have adequate reason to *believe* a theory, but whether we have adequate reason to *accept* it. Acceptance, I shall argue, will often go hand-in-

6 Bas C. van Fraassen, *Laws and Symmetry* (Oxford: Oxford University Press, 1989), p.143.

7 Charles Darwin, *On the Origin of Species* 2nd edition (London: John Murray, 1860), pp.480–81.

hand with belief, but it need not do so. Nor is it important that it should. It is sufficient for the progress of science, that scientists should accept the best available explanation, whether or not they happen to believe it. More importantly, we have excellent reasons to accept the best available explanation, *even if* (as non-realists argue) we have little or no reason to hold it to be true. Belief, in other words, is not the issue.

## 1. Acceptance and Belief

My argument, therefore, relies on being able to make a clear distinction between acceptance and belief. I am not basing this distinction on the way in which these terms are actually used, in everyday speech. It may be the case that in everyday usage, acceptance *is* often synonymous with belief. What I am arguing is that these terms can be used to capture a real distinction in propositional attitudes. To accept a proposition is not necessarily to believe it, even if the two often go together. There is nothing special about my use of the word 'acceptance' here. The distinction could, perhaps, be captured by using another word, such as 'assent'.[8] But talk of acceptance and belief is a helpful way of identifying two propositional attitudes that are distinct even if commonly conjoined.

A number of recent authors have also attempted to distinguish between belief and acceptance.[9] But they have done so for a variety of purposes and in a variety of ways. Some definitions of what it means to *accept* a proposition make acceptance indistinguishable from belief. D. S. Clarke, for instance, insists that acceptance actually entails belief, but assumes a broad view of acceptance that I

8 William P. Alston, 'Belief, Acceptance, and Religious Faith', in *Faith, Freedom, and Rationality: Philosophy of Religion Today*, edited by J. Jordan and D. Howard-Snyder (London: Rowman & Littlefield, 1996), pp.3–27, at p.8.

9 These include including William Alston, Michael Bratman, L. Jonathan Cohen, Keith Lehrer, John Perry, Robert Stalnaker, and Bas van Fraassen: see Pascal Engel, 'Introduction' in *Believing and Accepting*, edited by Pascal Engel, Philosophical Studies Series 83 (Dordrecht: Kluwer, 2000), pp.1–30, at p.8, as well as the literature cited below.

shall shortly reject.[10] Similarly, Paul Horwich argues that acceptance is functionally identical with belief, but assumes an instrumentalist view of acceptance – 'believing just the observable consequences of a theory'[11] – that I also reject. One can also define *belief* in ways that undermine this distinction. Herman de Regt, for instance, defines belief as 'a disposition to act'.[12] But if one defines belief in terms of a disposition to act, then it is practically indistinguishable from what I shall call 'acceptance'.

### 1.1 Assumptions Regarding Belief

So what I need for the purposes of my argument is a workable concept of acceptance. I shall spend little time on what I mean by belief. I am assuming that to believe something is to consider it to be true, the 'something' here being conceived of as a proposition, an utterance, or some other bearer of meaning. Understood in this way, belief is not to be thought of as a disposition to act, nor even as a disposition to assert. It can be defined as a disposition, but its most characteristic feature is a tendency to experience a certain kind of mental state. Belief is, as L. Jonathan Cohen writes,

> 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*, whether or not one is willing to act, speak, or reason accordingly.[13]

A person who has such a disposition will, if she is acting rationally and has no reason to do otherwise, be inclined to act in ways that are consistent with her belief. She will also be inclined, other things being equal, to assert the truth of that which she believes. (Belief can

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10 D. S. Clarke, 'Does Acceptance Entail Belief?', *American Philosophical Quarterly* 31 (1994), pp.145–55, at p.149.

11 Paul Horwich, 'On the Nature and Norms of Theoretical Commitment', *Philosophy of Science* 58 (1991), pp.1–14, at p.3.

12 Herman C. D. G. de Regt, 'To Believe in Belief: Popper and van Fraassen on Scientific Realism', *Journal for General Philosophy of Science / Zeitschrift für allgemeine Wissenschaftstheorie* 37 (2006), pp.21–39, at p.33.

13 L. Jonathan Cohen, *An Essay on Belief and Acceptance* (Oxford: Clarendon Press, 1992), p.4.

be regarded as a *complex* dispositional state.) But belief, as I understand it, should not be *defined* in terms of a disposition to act or to assert. After all, one could have a disposition to assert $p$ or to act as if $p$ were true, without believing that $p$. One could, for instance, be dissembling, merely pretending to believe.[14]

## 1.2 A Broad Definition of Acceptance

'Acceptance', however, is not so easily defined. The problem here is that the term is sometimes used in a broad sense, which distinguishes acceptance and belief so sharply as to deny that there is any relation between acceptance and truth. Andrei Buckareff, for instance, speaks of 'non-doxastic acceptance', and argues that while 'belief aims at truth, … acceptance aims at utility or success'[15] He also argues that while beliefs are such that they ought to form a coherent set – new beliefs added to the set should be consistent with the old[16] – acceptance is not subject to such a constraint.[17] But on my view of acceptance, acceptance has truth rather than some other kind of utility as its aim (see 1.3) and the propositions that we accept ought (at least ideally) to be consistent with one another (see 2.1).

L. Jonathan Cohen's definition of acceptance is also broader than the one I wish to employ. 'To accept that $p$', Cohen writes,

> is to have or adopt a policy of deeming, positing, or postulating that $p$ – that is, of going along with that proposition (either for the long term or for immediate purposes only) as a premiss in some or all contexts for one's own and others' proofs, argumentations, inferences, deliberations, etc., whether or not one assents and whether or not one feels it to be true that $p$.[18]

This broad definition of acceptance captures quite accurately, for instance, the sense in which a lawyer should 'accept' the innocence of

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14 Alston, 'Belief, Acceptance, and Religious Faith', p.7.

15 Andrei Buckareff, 'Acceptance Does Not Entail Belief' *International Journal of Philosophical Studies* 18 (2010), pp.255–61, at p.255.

16 Engel, 'Introduction', p.3.

17 Buckareff, 'Acceptance Does Not Entail Belief', p.255.

18 L. Jonathan Cohen, 'Belief and Acceptance', *Mind* 98 (1989), pp.367–89, at p.368.

his client or a jury should 'accept' the innocence of the accused. Both should act *as if* they believed him to be innocent. The problem is that the reasons that rightly motivate acceptance (in this sense) may bear no relation to the goal of truth. A lawyer, for instance, might choose to accept that his client is innocent,[19] but he does not do so with the aim of discovering whether he really is innocent. Indeed, he may already be utterly convinced that he is guilty and this belief may remain unchanged throughout the trial. But this makes no difference to his act of acceptance, understood in Cohen's sense.

There is another important difference here. Acceptance in the sense offered by Buckareff and Cohen may be entirely contextual. One may accept a proposition, in their sense, only in a particular setting, while being under no obligation to accept it outside that setting. The lawyer, for instance, is obliged to accept the innocence of his client only for the duration and purposes of the trial. If he has maintained this particular attitude throughout the trial – if he has spoken and acted in a way that assumes the innocence of his client – he has not breached his obligation to the client if he later admits (perhaps to his colleagues and after a conviction) that he always believed his client to be guilty. But in my narrower sense of acceptance, it is a global attitude: to accept a proposition is to accept it in any context in which it is appropriately employed.

As I noted earlier, the legitimacy of IBE is most commonly discussed with regard to the philosophy of science. But although there *are* contexts in which the broad definition of acceptance employed by Buckareff and Cohen is useful and appropriate, the philosophy of science is not one of them. As those authors point out, there are times when we accept a proposition, in the sense of acting on it, for entirely pragmatic reasons. But this doesn't seem to be the sense in which scientists accept the best available explanation of some puzzling phenomenon. Whether or not we have adequate reason to regard any scientific theory as true, the sciences do seem to have as their goal the pursuit of knowledge, which (at least on any traditional account) entails truth (see Section 3). If this is correct, then a conception of acceptance that does not have truth as its aim will be inappropriate.[20]

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19 Cohen, 'Belief and Acceptance', p.369.

For this reason, I shall adopt a narrower definition of acceptance, which *does* have a relation to what seems to be the goal of the sciences. The first part of my definition resembles that offered by Cohen: to accept a proposition is to employ it as a premise in one's reasoning, whether theoretical or practical, in any domain to which it might apply. But it adds a condition that I have taken from the work of Keith Lehrer. Acceptance, argues Lehrer, is to be defined by reference to its purpose or goal. And the goal or purpose of acceptance is that of 'attaining truth and avoiding error with respect to the very thing one accepts'.[21] So, on my view, to accept a proposition is *to employ it as a premise in one's reasoning, whether theoretical or practical, in any domain to which it might apply, with the goal of attaining knowledge.*

It follows that the person accepting a proposition, in this sense, and the person believing it have the same goal. They are both aiming at knowledge, which entails truth. The difference is that the person believing a proposition assumes – however provisionally and with due qualifications – that she has attained that goal. After all, that is what belief means: it means holding something to be true. But to accept a proposition is *not* to assume that the goal has been reached, since one can accept a proposition without believing it, even provisionally.[22] In accepting a scientific theory, one might even hope that it *is* true; one might hope that on this occasion the goal of knowledge has in fact been reached. But the act of accepting the theory does not commit one, even implicitly or tentatively, to the idea that it has.

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20 One can describe this view of science as 'axiological realism'. It holds that scientific theories aim at truth, whether or not we have any reason to believe they have achieved their aim. See Timothy D. Lyons, 'Toward a Purely Axiological Scientific Realism', *Erkenntnis* 63 (2005), pp.167–204, at p.167.

21 Keith Lehrer, *Theory of Knowledge* 2nd edition (Boulder, CO: Westview Press, 2000), p.13.

22 William Alston ('Belief, Acceptance, and Religious Faith', p.11) also outlines a distinction that resembles my own, but then imperils it by arguing that 'to accept that *p* is to regard it as true'.

Even on this narrow definition of acceptance – taking to be true in the course of one's reasoning with the goal of attaining knowledge – acceptance remains distinct from belief. In particular, acceptance (as I understand it) does not entail belief.[23] From '*S* accepts that *p*' one cannot validly infer that *S* believes that *p*.

Here's an example.[24] Contemporary physicists generally accept the theory of quantum mechanics developed in the 1920s by Erwin Schrödinger, Werner Heisenberg, and Max Born. This includes the intuitively surprising uncertainty principle: the idea that it is impossible to determine both the position and the momentum of a subatomic particle. A physicist could accept this theory, on the grounds that it has been enormously successful, while also failing to believe it. Indeed on the view I am defending, a scientist *ought* to accept this theory, as the best available explanation of the behaviour of subatomic particles. She ought to employ it as a premise in any further reasoning in the relevant domain. But we cannot infer from the fact that a particular physicist accepts this principle that she believes it. She may, for instance, find it impossible to believe that there is not some hidden variable that, if known, would allow for a precise prediction of both quantities. She may regard it as the best available theory, and therefore the one to be worked with, without regarding it as true.

If acceptance has knowledge as its goal and if knowledge entails truth, this might seem to entail that one cannot accept a proposition that one believes to be false. If we are dealing with simple propositions, this seems correct, but if we are dealing with scientific theories, the situation is more complex. There are at least two problems here.[25] The first is that scientific theories commonly involve idealizations or approximations, which would be true only under conditions that do not actually obtain. Surfaces are treated as though they are frictionless, even though no such surfaces exist. Fluids are treated as though they were continuous, even though they are made up of dis-

23 Alston, 'Belief, Acceptance, and Religious Faith', p.10.

24 For other examples, see Lehrer, *Theory of Knowledge*, 125 and Alston, 'Belief, Acceptance, and Religious Faith', pp.11–12.

25 In fact, there exists a third problem, which has to do with the acceptance of inconsistent theories, but I shall deal with that shortly (2.1).

crete molecules. And so on. One can accept a scientific theory as a whole, even though one recognizes that some of its assumptions are, when taken at face value, false.[26]

A related problem is that scientists may accept, in the sense of choosing to work with, a theory that has at least one indisputably false observational consequence.[27] In this situation, the theory cannot be true, at least as it stands. It may, of course, be *partially* true, in the sense of making true predictions within a certain domain, under certain conditions, or as a limiting case.[28] But it cannot be regarded as true *tout court*, that is to say, without some such qualification. In these circumstances a scientist will be reluctant to say that he or she *believes* the theory.[29] But she may consider herself to have excellent reason to *accept* it.

Such cases provide further evidence of the need to distinguish belief and acceptance. But they do not undermine the idea that the acceptance of scientific theories has knowledge as its goal. A scientist may admit that partial truth may be the best we can achieve at the present moment or that an idealized law – such as one that ignores friction – is the best way of working towards successful predictions. But this does not mean that she has abandoned goal of 'attaining truth and avoiding error with respect to the very thing [she] accepts'.[30] Galileo Galilei seems to have been the first scientist to propose laws of motion that ignored (for the moment) 'the resistance of

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26 I am grateful to Lucy Weston-Taylor for reminding me of this.

27 I am grateful to an anonymous referee for drawing my attention to this fact.

28 Alan Musgrave, 'Revisiting Critical Rationalism: Critical Rationalism, Explanation, and Severe Tests', in *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science* edited by Deborah G. Mayo and Aris Spanos (New York: Cambridge University Press, 2010), pp.88–112, at pp.101–3.

29 Ernan McMullin, 'A Case for Scientific Realism' (1984), in *Philosophy of Science: Contemporary Readings* edited by Yuri Balashov and Alex Rosenberg; Routledge Contemporary Readings in Philosophy (London: Routledge, 2002), pp.248–81, at p.275.

30 Lehrer, *Theory of Knowledge*, p.13.

the medium'.[31] To accept such laws is not to abandon the goal of truth: Galileo, after all, vigorously rejected instrumentalist interpretations of science.[32] It is to recognize (as he did) that the path to that goal may be indirect.

## 2 Inference to the Best Explanation

What, then, can we make of inference to the best explanation (IBE)? I shall begin with a promising account of IBE, which frankly acknowledges the non-realist's objection. It holds that although the fact that something is the best available explanation does not show it to be (even probably) true, it does make it reasonable for us to believe it. I shall then set out my own view, by way of contrast. It will differ in two respects. First of all, it will suggest that IBE warrants acceptance rather than belief. Secondly, it will set out this conclusion in terms of an 'ought'. Rather than saying that it is reasonable to believe the best available explanation, it will argue that we ought to accept it.

### 2.1 IBE and Belief

In the context of a vigorous defence of scientific realism, Alan Musgrave offers a novel defence of IBE. This involves a rejection of what he calls 'justificationism', the view that 'a justification for believing must be a justification for the belief'.[33] The key distinction here is that between the *act of believing* and the *content of the belief*. Musgrave argues that we may have adequate reason for an act of believ-

31 Galileo Galilei, *Dialogues Concerning Two New Sciences* [*Discorsi e dimostrazioni matematiche intorno a due nuove scienze*] (1638) translated by Henry Crew and Alfonso de Salvio (New York: Dover Publications, 1914), pp.251–53.

32 Galileo Galilei, 'Considerations on the Copernican Opinion' (1615), in *The Galileo Affair: A Documentary History*, translated and edited by Maurice A. Finocchiaro; California Studies in the History of Science (Berkeley, CA: University of California Press, 1989), pp.70–86, at pp.78–79.

33 Alan Musgrave, 'Popper and Hypothetico-Deductivism', in *Handbook of the History of Logic, Vol 10: Inductive Logic*, edited by Dov M. Gabbay, John Woods, and Stephan Hartmann (Amsterdam: Elsevier, 2009), pp.205–34, at p.226.

ing that fall short of an argument for the truth of what we believe. As we have seen, critics of IBE argue that the fact that something is the best available explanation does not demonstrate it to be (even probably) true. Musgrave agrees, but on the basis of this distinction he argues that IBE nonetheless warrants an act of believing. His guiding principle here is the idea that we acting reasonably in believing the best available explanation, even if the fact that it is the best available explanation does not show it to be (even probably) true.

It follows, on Musgrave's view, that the form of inference to the best explanation is as follows.

(a) The surprising fact *E* is observed.

(b) Hypothesis *H* would be a satisfactory explanation of *E*.

(c) No available competing hypothesis would *E* as well as *H* does.

(d) It is reasonable to believe the best available potential explanation of any fact, provided that explanation is a satisfactory one.

(e) *Therefore* it is reasonable to believe *H*.

If this is correct, IBE is justified not because we can assume that the best available theory is true, but because it is reasonable to believe such a theory.

This is an attractive view, for a variety of reasons. Its central problem has to do with premise (d). As opponents of realism argue, the fact that proposition *p* is the best available explanation of some fact does *not* show it to be even probably true. But if this is the case, it is difficult to see what reason one has to believe it, that is to say, to regard it as true. I shall argue in a moment that the fact that a proposition (or theory) is the best available explanation does give us good reasons to accept it. But acceptance, I have suggested, is not identical with belief.

There is another reason to reject Musgrave's premise (d). It is the fact that scientists sometimes seem justified in accepting inconsistent theories.[34] General relatively and quantum mechanics are apparently inconsistent theories, yet both are widely accepted. This is, of course, also a problem for my view of acceptance, since I have insisted that one should not accept inconsistent propositions. One

34 Mark Colyvan, 'The Ontological Commitments of Inconsistent Theories', *Philosophical Studies* 141 (2008), pp.115–23, at pp.116–17.

should not accept a proposition *p* if one has already accepted another proposition *q* that entails not-*p*. The difficulty here resembles that discussed earlier, namely that scientists can accept a theory that entails at least one false observational consequence (see 1.3). But as I argued on that occasion, recognizing this fact does not entail abandoning the idea that the goal of acceptance is knowledge (and therefore truth). If scientists do accept inconsistent theories, their acceptance is merely provisional, in the hope that one day the inconsistencies may be resolved.

Even given this qualification, my view seems preferable to Musgrave's. One can make some sense of the idea that scientists could provisionally *accept* inconsistent theories. But it is difficult to make sense of the idea that scientists could *believe* inconsistent theories, even in some provisional sense. They could certainly believe each of two inconsistent theories, if they did not realize they were inconsistent.[35] But it is difficult to see how they could continue to *believe* both theories, once they grasped the inconsistency. At least on the traditional assumption that inconsistent propositions cannot both be true, to *believe* inconsistent theories would mean regarding both as true, even though one knows that one of them is false.

A defender of Musgrave might respond that scientists can choose to work with inconsistent theories *without* believing them. But to 'work with' a theory – to take it as a premise in one's reasoning within the relevant domains – is precisely what I mean by acceptance. The response recognizes the very distinction between belief and acceptance on which my argument rests. The sole difference in the case of inconsistent theories is that one accepts the theories in the hope that one day the inconsistency will be resolved.

## 2.2 IBE and Acceptance

How, then, does my view of IBE differ from that offered by Musgrave? Modelling my schema on his, I can formulate my pattern of inference to the best explanation as follows.

(a) The surprising fact *E* is observed.

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35 John N. Williams, 'Inconsistency and Contradiction', *Mind* 90 (1981), pp.600–2.

(b) Hypothesis *H* would be a satisfactory explanation of *E*.

(c) No available competing hypothesis would explain *E* as well as *H* does.

(d) We ought to accept the best available potential explanation of any fact, provided that explanation is a satisfactory one.

(e) *Therefore* we ought to accept *H*.

I have already discussed the distinction between acceptance and belief. But some comment is in order on the deontological character of premise (d). Why have I chosen to cast this key premise in terms of an 'ought' rather than, as Musgrave does, in terms of 'reasonable' belief?

One reason for avoiding talk of 'reasonable' belief (or acceptance) is that the idea of the reasonable can be regarded as a merely permissive. It identifies, from a range of options, what one may believe or accept, not what one ought to believe or accept.[36] There *are* occasions when a permissive notion of rationality seems applicable. Take, for instance, the situation in which a scientist would be acting rationally in accepting either of two incompatible theories. This would be the case, for instance, if it were simply unclear which is the better. Differing judgements here might be equally defensible.[37] One might judge that theory *A* is the best available, while another might judge that theory *B* is the best available. But something stronger seems to be required when a theory *has* been identified as the best explanation. In such circumstances, the scientist is not merely *permitted* to accept it; she *ought* to do so.

What kind of an 'ought' is this? There are several possibilities. In a discussion of the ethics of belief, Richard Feldman argues that the 'ought' in such discussions is best regarded as a *role ought*.[38] It is comparable, in this respect, to the 'ought' in the sentence, 'A good pianist ought to be able to play Beethoven's Moonlight Sonata'. My

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36 Van Fraassen, *Laws and Symmetry*, pp.171–72 .

37 Alan Musgrave, *Essays on Realism and Rationalism* Series in the Philosophy of Karl R. Popper and Critical Rationalism 12 (Amsterdam: Rodopi, 1999), p.250 n.291.

38 Richard Feldman, 'The Ethics of Belief', *Philosophy and Phenomenological Research* 60 (2000), pp.667–95, at p.676.

own preference is for a slightly different view: that the 'ought' in question is that which arises in discussions of *practical* reasoning. It is comparable to the 'ought' in the sentence, 'If you want to become a good pianist, you ought to practise daily'. If you will a goal, then on pain of irrationality you ought to will the means that is most likely to lead to that goal.[39]

## 3 Why We Ought to Accept the Best Explanation

The key premise, then, in my revised abductive schema is premise (d): the claim that we ought to accept the best available potential explanation of any puzzling fact, on condition that it is itself a satisfactory one. Can this be defended? Why *should* we accept the best available potential explanation?

I am assuming that the goal of the sciences is knowledge, and that it is knowledge rather than simply truth.[40] On this view, science aims not merely at true beliefs, but at true beliefs for which we have adequate reasons. What I am arguing here is that if we are committed to the extension of our knowledge, then we ought to accept the best available explanation of whatever puzzling fact it is we are attempting to explain. The principle upon which I am relying is a principle of practical reason: it holds that if we are committed to a goal, then under pain of practical irrationality, we are also committed to the best means of attaining that goal. And the reasons why we should accept the best available explanation are pragmatic: they have to do with adopting the best available means to an end.

As I suggested earlier, one of the problems facing defenders of IBE is that it is notoriously difficult to establish a link between explanatory virtues and truth. Nor are we much further ahead if we argue (with Musgrave) that IBE warrants merely reasonable belief, for belief entails regarding something as true. Fortunately, it is easier to

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39 Both views avoid the 'belief voluntarism' objection, based on the idea that we cannot (directly) choose what we believe. But this objection is even less applicable to acceptance, since even if we cannot choose what we believe, we can surely choose what we accept.

40 Alexander Bird, 'What is Scientific Progress?', *Noûs* 41 (2007), pp. 64–89, at p.67.

establish a plausible link between explanatory virtues and what it is reasonable to accept.

In an illuminating discussion of these issues, Bill Lycan suggests that one cannot offer a *defence* of the kinds of explanatory virtues to which inference to the best explanation appeals. We cannot show, against the sceptic, that theories possessing these virtues are true or even probably true. All we can offer, he argues, is an *explanation* of why it is that we do favour theories of this kind. His explanation takes the form of what he himself calls 'Panglossian reflections' regarding a benevolent mother nature.[41] Lycan begins by noting our customary rules of theory-preference, which rely on the idea that there exist certain 'explanatory virtues'. Given any two theories, $T_1$ and $T_2$, the following rules are customarily employed.

1. Other things being equal, prefer $T_2$ to $T_1$ if $T_2$ is simpler than $T_1$.
2. Other things being equal, prefer $T_2$ to $T_1$ if $T_2$ explains more than $T_1$.
3. Other things being equal, prefer $T_2$ to $T_1$ if $T_2$ is more readily testable…
4. Other things being equal, prefer $T_2$ to $T_1$ if $T_2$ leaves fewer messy unanswered questions behind…
5. Other things being equal, prefer $T_2$ to $T_1$ if $T_2$ squares better with what you already have reason to believe.[42]

We can explain our preference for these rules by assuming, for a moment, that we were designed by 'a skillful and benevolent Mother Nature', who wanted us to form beliefs of the most useful kind.[43] What kinds of theories would this benevolent designer have designed us to prefer? Firstly, she would have built us to prefer simpler hypotheses. The reasons she would have done so are practical ones: such theories are more efficient to work with and run a lesser risk of error. Secondly, and for similar reasons of efficiency, she would have built us to prefer hypotheses of greater explanatory power. Thirdly,

41 William G. Lycan, *Judgement and Justification* Cambridge Studies in Philosophy (Cambridge: Cambridge University Press, 1988), p.143.

42 Lycan, *Judgement and Justification*, p.130.

43 Lycan, *Judgement and Justification*, p.140.

she would have led us to prefer theories that have implications that are readily observable in our interactions with our environment. She would not, in other words, have wanted us to adopt theories whose falsity would never become evident to us. Fourthly, she would have led us to prefer 'neater systems of beliefs to messy ones full of pathways that lead nowhere',[44] since beliefs are like maps: guides to action that should help us find our way around. Fifthly, she would have built us to be conservative in matters of belief, not radically altering our beliefs more than necessary, since such alteration has costs in terms of energy and resources. Such considerations, Lycan argues, explain the epistemic preferences that we do, in fact, have.

We should not read too much into the way Lycan has formulated his argument. His talk of a benevolent mother nature is merely a device. It may be regarded as a fanciful way of describing the effects of natural selection, but it need not be so regarded. Even if it were entirely a fiction, it would highlight some important facts about the utility of theories that display these explanatory virtues. Such theories, it suggests, have features that can aid us in our search for the truth. If truth is our goal, we should prefer those theories that are simple, have great explanatory power, and are testable, comprehensive, and consistent with what we already know.

Lycan rightly argues that such features do not give us adequate reason to believe that a theory will be true, or even probably true. But they can provide us with a defence of IBE, if we understand IBE as warranting nothing more than acceptance. I have argued for a view of acceptance that has truth, or (more precisely) knowledge as its goal. What Lycan's Panglossian reflections show is that theories exhibit these features are more likely to bring us to our goal. They are the theories that are most likely to contribute to the growth of our knowledge. This is not a reason to believe such a theory, but it is a reason to accept it, at least until a more satisfactory theory emerges.

At this point, a reader might object that I have fallen back into the very pragmatic notion of acceptance that I previously claim to have rejected. The objection is understandable. There *is* a sense in which acceptance, as I understand it, is motivated by pragmatic con-

44 Lycan, *Judgement and Justification*, p.142.

siderations, for the considerations that weigh in favour of IBE are indeed pragmatic. What is important is that it is a pragmatism that is narrowly focused: it has knowledge, rather than some other kind of utility, as its goal. When we accept a theory, it is because it is likely to lead us closer to this goal. So unlike Buckareff and Cohen, I have not severed the link between acceptance and truth. The fact that a theory is the best available explanation may not give us adequate reason to regard it as true, but if our goal is knowledge (and therefore truth), it does give us excellent reason to accept it.

It follows that my view of scientific theories is not 'instrumental' in the traditional sense. It does not regard a scientific theory as comparable to a tool, such as a hammer, which in no sense 'represents' the things it produces.[45] Every analogy limps, but we might say that on my view, adopting a theory more closely resembles choosing the best route up a mountain. If our aim is to reach the summit, we are acting rationally in choosing the route that seems most likely to take us there. Indeed on pain of practical irrationality we *ought* to choose the most likely route. But the route is itself part of the mountain and, for all we know, once we have reached the top of this particular path, we may be at the summit.

## Conclusion

We are not, I have argued, warranted in believing a theory – in holding it to be true – because it is the best available explanation. But the fact that it is the best available explanation means that we ought to accept it: we ought to take it as a premise in our theoretical and practical reasoning in any domain to which it applies, whether or not we happen to believe it. The reasons for this are pragmatic. A theory that displays the qualities that we traditionally value – such as simplicity, explanatory power, a high degree of testability, and consistency with what we are already believe – is likely to be a useful tool in the task of increasing our knowledge of the world. Do we ever have reason to think that our knowledge of the world has been increased? That is another question; the answer may be that we do not. But if

45 Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation* (London: Routledge and Kegan Paul, 1961), p.130.

our goal is knowledge, then not only is it reasonable to accept the best available explanation of any puzzling fact; we *ought* to do so.

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