
Belief, Confidence, and the Method of Science

I. Science and Belief: Introduction

During the 1890s, Peirce sometimes insisted that it was unscientific and, indeed, improper for investigators to believe current scientific results. In an 1898 lecture he went further, saying: ‘I hold that what is properly and usually called belief . . . has no place in science at all’.¹

Full belief is willingness to act upon the proposition in vital crises, opinion is willingness to act upon it in relatively insignificant affairs. But pure science has nothing at all to do with action. The propositions it accepts, it merely writes in the list of premises it proposes to use. (*CP* 1.635; *RLT*: 112)²

These remarks occur in the course of an argument for a sharp separation of theory and practice. Peirce has already insisted that we should attach no value to reason or scientific reflection when we attempt to settle practical or vital questions: we should, instead, rely upon instinct, common sense and sentiment. Hence, he accompanies these remarks with the assertion that ‘nothing is vital for science; nothing can be’ (*CP* 1.635; *RLT*: 112). Although many scientific results ‘are almost immediately applicable to human life’, the ‘true scientific investigator completely loses sight of the utility of what he is about’ and, if he were not to do so, ‘it would spoil him as a scientific man’ (*CP* 1.619; *RLT*: 107). A true scientist ‘is not in the least

¹ This lecture was the first of the ‘Cambridge Conferences’, delivered on 10 February 1898. This series of talks has now been published as *Reasoning and the Logic of Things* (*RLT* 1992), with a useful introduction by the editor, Kenneth Ketner. Volume 1 of Peirce’s *Collected Papers* contains an edited version of this lecture (*CP* 1.616–48) and part of an alternative draft (*CP* 1.649–77). I have given references to this edition and, where possible, to the corresponding passages in *RLT*.

² In another lecture from 1898, Peirce characterized belief as ‘a willingness to risk a great deal upon a proposition’ and continued: ‘But this belief is no concern of science, which has nothing at stake on any temporal venture but is in pursuit of eternal verities (not semblances of truth)’ (*CP* 5.589). In a manuscript of 1903, we read that ‘pure science has nothing to do with *belief*’ (*CP* 7.606; see also *CP* 1.239 fn.).

wedded to his conclusions'; since nothing is risked on them, 'he stands ready to abandon one or all as soon as experience opposes them' (*CP* 1.635; *RLT*:112).

Some of them, I grant, he is in the habit of calling established truths but that merely means propositions to which no competent man today demurs. It seems probable that any given proposition of that sort will remain for a long time upon the list of propositions to be admitted. Still it may be refuted tomorrow; and if so, the scientific man will be glad to have got rid of an error. There is thus no proposition at all in science which answers to the conception of belief. (*CP* 1.635; *RLT*: 112)³

Such statements appear to conflict with some distinctively 'Peircean', doctrines from the 1860s and 1870s.⁴ Consider the model of inquiry found in the first of the *Illustrations of the Logic of Science*, 'The Fixation of Belief'. Belief is a settled state, a stable disposition to act; so long as we are confident of a currently accepted belief, we see no need to doubt it or to inquire further into the grounds of its truth. Inquiry is always motivated by surprise, usually perceptual surprise, which disrupts our harmonious system of beliefs, converting stable belief into living doubt. Doubt is an unsettled state prompting inquiry directed at its elimination; the goal of

³ Inferences which are 'scientific . . . have no true probability and are not matters for belief. We call them in science established truths, that is, they are propositions into which the economy of endeavour prescribes that, for the time being, further inquiry shall cease' (*CP* 5.589).

⁴ Indeed, anyone unfamiliar with the writings referred to above might suppose that denying this dualism was one of the marks of 'pragmatism'. If one ignores these passages, it would be hard to question Ursula Niklas's comment that a 'particular strength of Peirce's account of the nature of meaning, given in the maxim of the pragmatic account of intellectual or scientific concepts, lies in the fact that it overcomes the traditional distinction between theory and practice' (1988: 31). As is stressed below, Peirce views science as a kind of practice, and his pragmatist maxim reflects this. Our problem is to see how this can be reconciled with the above cited claims.

The fact that there is a conflict between Peirce's apparent sharp distinction between theory and practice and his insistence that science is a 'mode of life' (*CP* 7.55) is noted by Maryann Ayim (1981: 46). She adds, plausibly: 'All the underlying tenets of Peirce's philosophy cry out against the type of rigid distinction he tried to draw between theory and practice' (p. 51). Ayim proposes that Peirce can use a third kind of science ('the sciences of review') to mediate between theoretical and practical science. This reflects her readiness to interpret Peirce's claims about 'practice' as claims about 'practical science'—which may be supported by passages such as *CP* 1.239. However *CP* 1.234 suggests a certain disdain for practical science: 'I must confess to being utterly bewildered by its motley crowd, but fortunately the natural classification of this branch will not concern us in logic.' In the light of passages cited in the text, it is natural to feel that, for Peirce, 'practical science' is not 'science properly so-called'.

the inquiry is, simply, replacement of doubt by settled belief in the truth or falsity of the disputed proposition. Hence, anything we currently believe is taken as *prima facie* acceptable; it has a presumption in its favour. It is clear that this picture is intended to apply to scientific inquiries as well as to common-sense ones. But if application of the scientific method cannot (or should not) produce *belief* at all, it is hard to see how we can view the method of science as a method for the fixation of *belief*. Hence one apparent tension between Peirce's 1898 remarks about theory and practice and his philosophical position twenty years earlier.⁵

This sense of tension is heightened when we note Peirce's anti-Cartesian rhetoric: 'let us not pretend to doubt what we do not doubt in our hearts' (W2: 212). Isn't that just what the passages from the 1890s are requiring scientists to do? If 'ought' implies 'can', then Peirce's remarks suggest that scientists can disengage their hearts from scientific propositions, but official Peircean doctrine (and common sense) may lead us to doubt that it is possible for them to do this.

The claim that scientists hold no scientific beliefs is not frequently made in Peirce's writings: it occurs in writings between 1898 and 1903; but soon after this, we find references to 'scientific belief' and comparisons of the logical and semantic properties of 'practical' and 'theoretical' belief.⁶ So perhaps the 'no belief thesis' was a temporary lapse from philosophical good sense. Indeed, the context of some of the cited remarks might discourage us from assigning them much weight. They come from the first of the Cambridge Conferences lectures in which Peirce was berating his hosts for doubting that his proposed discussion of the foundations of logic was appropriate for his audience, and for calling instead for 'detached ideas on vitally important topics'.⁷ His scepticism that philosophy had much to contribute to what is now called 'applied ethics' led to his claim that 'all sensible talk about vitally important topics must be commonplace,

⁵ For the subsequent development of my argument, it is important to note that the discussion in 'The Fixation of Belief' locates the concept of belief in two ways: beliefs are dispositions to act, states which operate in concert with desires in order to determine actions; and beliefs have a distinctive functional role in the progress of inquiry—they are settled states of assent which prompt no further inquiry into the proposition assented to. The tensions I am concerned with begin to emerge when it is asked whether these two characteristics identify the *same* states.

⁶ See, for example, *CP* 5.539 ff (1903), and, for discussion of Peirce's attempts to draw this distinction, Thompson (1953: 253 ff). And, for contrast, note: 'the scientific spirit requires a man to be ready at all times to dump his whole cartload of *beliefs*, the moment experience is against them' (*CP* 1.55, my italics).

⁷ It is also reasonable to suppose that Peirce had William James's recently published *The Will to Believe* in mind. See Skagestad (1981: 208 ff) and Apel (1981: 158 ff).

all reasoning about them unsound, and all study of them narrow and sordid' (*CP* 1.677). He was probably anxious to distance himself from William James's promise that pragmatism might serve as a vehicle for the improvement of human welfare: he was certainly determined to dissociate himself from those who anticipated vital benefit from the study of metaphysics and to urge that a true scientific spirit should govern work in that discipline. Peirce may simply have over-stretched himself in lending rhetorical weight to his conservative distaste for allowing any role for rational reflection in practical or political matters: 'In everyday business, reasoning is tolerably successful, but I am inclined to think that it is done as well without the aid of theory as with it' (*CP* 1.623; *RLT*: 109).

Men many times fancy that they act from reason when, in point of fact, the reasons they attribute to themselves are nothing but excuses which unconscious instinct invents to satisfy the teasing 'whys' of the ego. The extent of this self delusion is such as to render philosophical rationalism a farce. (*CP* 1.631; *RLT*: 111)⁸

But there is more to it than that—indeed, a hint that this is so is provided by the writings of Popper, where a similar political and ethical anti-rationalistic conservatism, together with a view of science which resembles Peirce's in some respects, lead again to the view that scientific propositions should not be believed.⁹

References to scientific belief and theoretical belief need not conflict with Peirce's 'no-belief' doctrine, for scientific belief may be intended to contrast with 'what is properly and usually called belief'. The passage from *CP* 1.635 contrasts scientific assent both with 'belief' and with full belief. If the contrast with full belief can be sustained, it may be a minor verbal matter whether we describe 'what scientists are in the habit of calling established truths' as a kind of belief: what is important is that this state is distinguished from full belief.¹⁰ The context of the 1898 lecture may have

⁸ Compare: 'If you ever happen to be thrown in with an unprofessional thief, the only really bad kind of thief, you will find that two things characterize him: first, an even more immense conceit in his own reasoning powers than is common, and second, a disposition to reason about the basis of morals' (*CP* 1.666).

⁹ See Popper (1972). A rather different form of the thesis that scientific assent is not a form of belief is fundamental to the 'constructive empiricism' of Bas van Fraassen. He holds that rather than presenting theoretical claims as true, the scientist displays them and ascribes to them such 'virtues' as empirical adequacy (1980: 10).

¹⁰ The reference to 'full belief' in these passages may suggest to the reader that Peirce is simply denying that we should be *certain* of scientific results, a lesser degree of belief ('opinion') being permissible. I hope, in the course of this chapter, to make clear that this would be a mistake.

dictated the formulation the point received without leading to a distortion of Peirce's true position. The underlying issue concerns his reasons for distinguishing two kinds of 'assent': it may be relatively unimportant that he was not always consistent about whether he wished to refer to both as forms of belief. There are several reasons for investigating these issues. First, they raise important questions about the nature of what (for the sake of neutrality) I shall call 'scientific assent' ('assent' for short). There are general problems about whether a resolute fallibilism is compatible with the possibility of our believing current scientific opinions, and Peirce's struggle with these may, I hope, illuminate the problems. In this chapter we focus primarily upon a negative claim: scientific assent does not involve the sort of *belief* that is involved in the explanation action. Chapter 2 takes the issue further, exploring some Peircean views about what is distinctive about *scientific* assertion and assent in more detail. In accepting a scientific claim, he claimed, we do not firmly commit ourselves to its truth. Our commitment is often only to the fact that it provides a one-sided approximate formulation of some truths that will be better grasped and formulated through further inquiry.

Second, the investigation forces us to re-examine the structure of 'The Fixation of Belief': we obtain a better focus on some features of that argument which, from Peirce's own point of view, should have been profoundly unsatisfactory.¹¹ Third, we can hope for insight into Peirce's views about the sources of our confidence in our practical assurances. He was clearly contemptuous of those who expect reason to solve practical problems. But he was no irrationalist; accepting the guidance of instinct and sentiment is a mark of wisdom, a guide to acting well. But the fourth, and most important reason for examining this issue relates to the development of Peirce's thought. Identifying some of the tensions in the arguments of 'The Fixation of Belief' will enable us to place in a proper perspective some of the emphases in Peirce's thought after 1900.

These later writings introduce a variety of themes in order to provide foundations for logic and science: in 1903, he emphasized his phenomenological defence of the categories and proposed that we ground logic in ethics and aesthetics; a few years later, these doctrines were less prominent and Peirce was stressing his critical common-sensism; and by 1908, the

¹¹ This relates to an issue mentioned in note 3 above. The discussion will also, I hope, help to clarify the argumentative structure of this paper—and of the series to which it belongs. It seems to me that commentators have paid insufficient attention to Peirce's explicit claims about what is going on in these papers and have misunderstood them. Related points were made in Hookway (1985: 43 ff).

role of religious belief in grounding science was receiving more attention. Examining the issues surrounding Peirce's 'no-belief' thesis will clarify the tensions in his thought leading to these developments.¹²

In section 2, an attempt is made to formulate the 'no-belief' thesis and to see how it was supported. This leads (in section 3) to a more detailed discussion of the strategy of 'The Fixation of Belief', and an attempt to show that in 1877 Peirce lacked the resources to formulate issues about theory and practice that ought to have been (and indeed were) of concern to him. We then examine in more detail the conception of scientific assent which emerges in some of his later writings (section 4). Finally (section 5), an issue about theory, practice, and the practice of science is raised which, I shall suggest, forms the background for some of the later developments in Peirce's thinking.

2. Belief: Causes and Reasons

The position Peirce defended in 1898 suggested a distinction between 'full belief' which is linked to action and the 'vital' concerns of life, and 'scientific belief' (or 'assent'), which is not. We could approach the distinction in two ways: by examining Peirce's views about practice, and identifying what is distinctive about our cognitive response to vital questions; or by examining the special features of scientific assent. It would be a mistake to treat either route as fundamental: Peirce has distinctive views about both theory and practice and they independently contribute to his responses to these issues. His understanding of judgements of practice informs his philosophy of science; and his view of scientific rationality informs his philosophical account of practice.¹³ In particular, we should resist the conclusion that Peirce's 'sentimentalist' view of practical decision was forced on him by his account of scientific reasoning; his moral conservatism was there from the beginning.¹⁴

How should we formulate the 'no-belief' thesis? Consider an agent A

¹² This chapter is largely devoted to describing the tension I have referred to and to focusing on some issues underlying the 'no-belief' thesis. The role that Peirce assigns to sentiment and instinct in rationality is discussed more fully in Chapters 10 and 11.

¹³ For further discussion, see Skagestad (1981: 46–7).

¹⁴ This is most evident in his discussions of religious belief—which answers to a vital question. For example, in 'Critique of Positivism', written before 1870, we read that 'those beliefs which come to all men alike before reflection are generally true', and this is because 'the reasons which produce fallacies depend upon a conscious process of reasoning' (W2: 127–8). More passages of the same sort are presented and discussed in Chapter 11.

who carries out scientific investigations. She proposes the hypothesis *H*, and tests it rigorously, eventually deciding that no further testing is required: the hypothesis is ‘established truth’. The strongest version of the thesis holds:

(I) It is wrong for *A* to believe *H*.

We noted passages which appear to deny that *A* should make *H* the object of a ‘full belief’ (*CP* 1.635). Perhaps this means that it is merely wrong for her to be *certain* of it: in a fallibilist spirit, she can hold it to be probable even if it is not certain. Scientific propositions, Peirce said once, are ‘but opinions at most’ (*ibid.*), which suggests, in the light of the other quotation from *CP* 1.635 above, that we may act on them in insignificant matters but not in connection with vital matters. However, as is clear from *CP* 1.689, he denied that the scientific method could even attach probabilities to conclusions: it is as unscientific to regard a conclusion as *probable* as it is to judge it certain. Hence we could reformulate (I) as:

(Ia) It is wrong for *A* to take *H* to be certain or probable.

And a corollary of this appears to be:

(II) It is wrong to use *H* as a guide in answering practical or vital questions.

This is hard to accept. Why is it wrong to act on current scientific assurances? The suggestion that such assurances are but ‘opinions at most’ may indicate that it is permissible to act on them in relatively insignificant affairs:¹⁵ why, then, should we ignore apparently relevant scientific information when confronting ‘vital crises’? One answer is that if we employ scientific opinions in ‘vital crises’ we acquire an interest in their truth: the discovery that they are false might then be a source of dismay rather than glee at having pushed the human quest for truth one step further. In that case, drawing the distinction in terms of the relative ‘significance’ of the affairs on which we act would be misleading. Suppose that relying upon a scientific belief offered my best chance of escaping from a serious life-threatening danger. Since such a crisis is momentary, I retain no interest in

¹⁵ But recall my comment above that Peirce seems to have thought that the scientific method does not even make conclusions probable.

the proposition's truth once I have reached safety and I could still welcome its refutation. Peirce's perception of the threat posed by live belief in scientific propositions focuses, it seems, upon the way in which my possessing an interest in the truth of a proposition would prevent my subjecting it to proper scientific scrutiny; and exercising a belief to resolve a one-off vital crisis need not affect that. A better example of using a scientific belief in connection with vital purposes would be the case of a micro-biologist who sinks his life savings in the commercial exploitation of his discoveries. We must avoid projects which are pursued alongside our scientific activities and which rest upon the approximate truth of scientific theories currently endorsed within the scientific tradition in which we work.

If that is all that is involved, then Peirce's anxieties seem excessive. If the theories are refuted, then the commercial projects will fail whatever the scientist's view of the matter; if they do not fail, then they do not depend strictly upon the theory in question. Moreover, since, as Peirce insists, science is a co-operative activity, there need be no risk to scientific progress in the fact that I have a commercial interest in a recent discovery so long as I have not persuaded all of my fellow inquirers to invest in my project. It is not obvious that the scientist's commercial venture will block the road of inquiry. Furthermore, Peirce often remarks that in certain circumstances it is rational to act on the basis of propositions that we do not believe but which we *hope* to be true.¹⁶ The prohibition on *belief* in scientific conclusions does not prohibit acting on the *hope* that they are true. In that case, (II) is not obviously a corollary of (I): but if (I) can be accepted while (II) is denied, the rationale for (I) (the 'no-belief' thesis) is very unclear.¹⁷

A weaker thesis would hold that Peircean scientists should not carry out scientific investigations *for the sake of* their applications. But even if we ought not to undertake investigations out of practical motives, it does not follow that we cannot believe their results. All that follows is that we should not adopt projects of various kinds: perhaps the 'man of science'

¹⁶ See, for example, *CP* 2.113. This possibility seems relevant to the kinds of case that we have considered so far. Although Peirce invoked this possibility in order to argue that fundamental laws of logic are all hopes, (see, for example, *NEM3*: 371), the examples he uses to illustrate his point make clear that it has wider application. Of course, there may be limits to what can be achieved by *hopes*: it is doubtful, for example, that a Christian life could be sustained by the *hope* that Christ is the son of God.

¹⁷ When Peirce writes that 'the investigator who does not stand aloof from all intent to make practical applications will not only obstruct the advance of pure science, but, what is infinitely worse, he will endanger his own integrity and that of his readers' (*CP* 1.619), the strong impression is given that he is opposed to (II) as well as to (I).

should not make his scientific ventures subordinate to other fundamental projects; but so long as a scientist does not have mixed motives, there seems no obstacle to his believing his results.

Peirce's thesis leads to some implausible claims about applied or 'useful' science (like engineering and surgery). Such inquiry is useful, he tells us, 'only in an insignificant degree' and 'it still has a divine spark in which its petty practicality must be forgotten and forgiven':

But as soon as a proposition becomes vitally important—then in the first place it is sunk to the condition of a mere utensil; and in the second place, it ceases altogether to be scientific, because concerning matters of vital importance reasoning is at once an impertinence towards its subject-matter and a treason against itself. (CP 1.671)

Amid the exaggerated rhetoric, Peirce is pointing to an unresolved tension in the intellectual life of an applied scientist. Such inquirers must address questions of two distinct (but confusingly similar) kinds. First: has a particular proposal survived rigorous test sufficiently well that we may exempt it from further test for the time being and use it in theory construction and experimental design? Second: has the proposal been tested sufficiently that we may take it as 'established', applying it in our engineering or surgical practice? Peirce ought to hold that only the first of these is a properly scientific question; once we address the second, we enter the realm where reason is rationalization and instinct and sentiment rule. Logical self-control can be our guide in the first but not in the second. An affirmative answer to the first makes the proposition available for scientific purposes; an affirmative answer to the second makes it more generally available. If these two questions are not kept apart, one's logical integrity is compromised.

There is a useful discussion at CP 5.589 where Peirce is contrasting the attitudes towards facts that are appropriate for science and for practice. For the time being science can be content with theories which, it is aware, contain much that is arbitrary or subjective, while 'practice requires something to go upon, and it will be no consolation to it to know that it is on the path to objective truth'. And this is because, 'the actual truth it must have, or when it cannot attain certainty must at least have high probability, that is, must know that, though a few of its ventures may fail, the bulk of them will succeed'.

When an hypothesis has survived rigorous examination, we may reflect: 'this ground has held a long time without showing signs of yielding. I may hope that it will continue to hold for a great while longer.' According to

Peirce, this judgement is 'extra-scientific': it has no role in purely scientific endeavour. But it gives practice a basis for believing the hypothesis: 'I can safely presume that so it will be with the bulk of the cases in which I shall go upon the theory.' This step towards belief has no scientific importance:

Thus those retractions which at length acquire such high degrees of certainty, so far as they are so probable, are not pure retractions and do not belong to science, as such; while, so far as they are scientific and are pure retractions, have no true probability and are not matters for belief . . . they are propositions into which the economy of endeavour prescribes that, for the time being, further inquiry shall cease. (*CP* 5.589)

We shall see below that Peirce's logic, his account of inquiry and self-control, entails that reason (rational self-control) can never produce full belief. Belief (in the proper and usual sense) will always have causes over and above any reasons we may have for holding it. If we fully believe a proposition, it enters into a causal nexus other than that of rational self-control: instinct and sentiment intrude to govern its formation. Since 'I would not allow sentiment or instinct any weight whatsoever in theoretical matters, not the slightest',¹⁸ a theoretical proposition which is believed is to be treated with mistrust. For, in such a case, my acceptance of the proposition has causes other than those of which I am aware. It is under the sway of sentiment and instinct, and so I cannot feel confident of my ability to control my use of it and my acceptance of it. So what is wrong with full belief, from a scientific point of view, is that it is not subject to my control.

Peirce's arguments here depend upon the claim that, with respect to practical matters, sentiment and instinct rule: practical reflection is mere rationalization; the belief that we have reasons for acting as we do is 'self-delusion'. Wisdom allows reflection to be tempered by instinct and sentiment.

Sentimentalism implies Conservatism; and it is of the essence of conservatism to refuse to push any practical principle to its extreme limits—including the principle of conservatism itself. We do not say that sentiment is never to be influenced by reason, nor that under no circumstances would we advocate radical reforms. We only say that the man who would allow his religious life to be wounded by any

¹⁸ *CP* 1.634. As is emphasized in Chapter 9, this statement is an exaggeration of Peirce's considered position. He did allow a role for sentiment in logic and in the use of inductive reasoning.

sudden acceptance of a philosophy of religion or who would precipitately change his code of morals at the dictate of a philosophy of ethics . . . is a man who we should consider unwise. (MS 437:14)

Now it is a corollary of this that beliefs have causes which are not reasons for holding them, and for the most part we are not reflectively aware of what these causes actually are. We should guard against the temptation to exercise rational self-control in our practical activities. Once we *believe* a scientific hypothesis, our attitude towards it is affected by this extra-scientific causal nexus as well as by rational self-control. If science is to be the epitome of rational self-control, it requires us to understand the determinants of our attitudes to hypotheses. Belief compromises logical integrity by limiting self-control.

If this is what Peirce has in mind, it seems an over-reaction to a recognizable difficulty; indeed, Peirce's response to his problem seems utopian. It is hard to believe that human inquirers possess the self-control required to immunize their scientific opinions from the sorts of malign forces which would thus threaten their ability to subject these beliefs to rational self-control. But it is also hard to believe that the community of inquirers as a whole is not alert to the sorts of dangers here referred to; one's first reaction is that in most circumstances the result of failing to achieve such immunity is unlikely to block the road of inquiry. Moreover, we can acknowledge the diverse demands upon the judgement of applied scientists without concluding that logical integrity cannot be maintained through self-awareness and self-control.¹⁹

But what should we make of Peirce's claim that scientific assurances are 'but opinions at most' (CP 1.635)? Are they opinions or not even that? In the light of the interpretation offered, I suggest that opinions are beliefs about which we are tentative or uncommitted, in which case the grip of the causal processes which have transformed scientific assent into (weak)

¹⁹ Peirce frequently returns to a particular kind of case where the existence of such a blockage may be found. Theologians attempt to use rational controlled inquiry in theorizing about religious matters, and they expect their inquiries to reach substantial conclusions on matters of doctrine. In Peirce's view, the results of their investigations are expected to make a difference to what the believer should believe, and to how he or she should live. Peirce holds that undertaking such projects denies the true character of religious belief and is a betrayal of the scientific or philosophical spirit: it tries to use reason where sentiment should rule. His sense that the resulting outlook is deeply dishonest, and his assurance of the purity of the motives of those possessed of the true scientific spirit, are reflected in his famous observation that he had never heard of someone who 'considerably increased human knowledge' being a criminal 'unless theology be knowledge' (CP 1.576).

belief will not be strong enough to inhibit the further operations of rational self-control. We might be psychologically incapable of preventing these processes giving rise to weak opinions, but scientific self-control requires us to be able to withstand processes which would produce anything stronger.

3. The Method of Science and ‘The Fixation of Belief’

As we noted in section 1, the claim that ‘belief’ has no place in science appears to conflict with the views of ‘The Fixation of Belief’. I shall now turn to the arguments of that paper. If we read it with the later distinction in mind, we can gain insight into both. We can find the later claim about scientific belief prefigured there; but we shall also be able to locate a deeper and unresolved tension in the earlier discussion. I shall sketch two competing accounts of the strategy employed in that paper. My conclusion will be that both contain a partially correct account of what was going on, and that, *in 1877*, Peirce could not bring them together into a coherently structured whole. I should guard against misunderstanding here. The claim that there were tensions in the earlier position does not entail that Peirce was confused at the time. I suspect that he was fully aware of these tensions, and that they resulted from his inability, in the mid- to late 1870s, to solve some difficult problems about reference and reality. His other work at the time, and especially in the ensuing decade, can best be read as a concerted attempt to solve these problems. As he was aware, until he had solutions to them, his arguments for the position he defended would not be fully satisfying.²⁰

In ‘The Fixation of Belief’, inquiry is described as a process initiated by the disturbance of a previously settled body of opinion. The doubt resulting from this disturbance motivates us to activities which come to an end once the uneasy state of doubt is replaced by the settled state of belief. Peirce evaluates some methods which promise to facilitate this process of inquiry. As is well known, four such methods are examined: three fail, and the method of science triumphs. The method of tenacity—‘taking any answer to a question which we may fancy, and constantly reiterating it to ourselves, dwelling on all which may conduce to that belief, and learning

²⁰ The last three sentences of this paragraph are an addition to my original paper. This is explained in more detail in the Introduction, section 5. The ideas about the development of Peirce’s work are taken further in Chapters 4 and 6. A very useful recent discussion of the interpretation of Peirce’s intriguing paper is to be found in Anderson (1995: 82–115).

to turn with contempt and hatred from anything which might disturb it' (W3: 248–9)—fails because we shall not be able to avoid meeting others with different opinions and that will inevitably shake our confidence. The method of authority advocates the creation of an institution whose role is 'to keep correct doctrines before the attention of the people, to reiterate them perpetually, and to teach them to the young; having at the same time power to prevent contrary doctrines from being taught, advocated or expressed' (W3: 250). This fails, we are told, for a similar reason: the institution cannot guarantee that we shall never encounter those who are not subject to our authority and this will lead us to view our opinions as arbitrary, thus unsettling them and producing doubt. Third comes the a priori method: 'let the action of natural preferences be unimpeded . . . and under their influence let men, conversing together and regarding matters in different lights, gradually develop opinions in harmony with natural causes' (W3: 252). This fails through making the development of inquiry 'a matter of taste', submitting opinion to the dictates of fashion. Reflecting on differences in (for example) moral standards, Peirce 'cannot help seeing that . . . sentiments in their development will be very greatly determined by accidental causes'; and he continues:

Now, there are some people, among whom I must suppose that my reader is to be found, who, when they see that any belief of theirs is determined by any circumstance extraneous to the facts, will from that moment not merely admit in words that that belief is doubtful, but will experience a real doubt of it, so that it ceases to be a belief. (W3: 253)

In view of the discussion of the 1890s, this is a very surprising statement: Peirce appears to be saying that the fact that a belief originates in sentiment makes right-thinking inquirers doubt it. Twenty years later he thought otherwise. Surprisingly, appeals to common sense and sentiment can be found in writings from the 1860s too.²¹ But we must not interrupt the story: we seek a method 'by which our beliefs may be caused by nothing human, but by some external permanency—by something on which our thinking has no effect'. And so we adopt the method of science, using methods and rules only if they can be defended by reference to the fundamental hypothesis that:

There are real things, whose characters are entirely independent of our opinions about them; those realities affect our senses according to regular laws, and, though

²¹ See note 14.

our sensations are as different as our relations to the objects, yet, by taking advantage of the laws of perception, we can ascertain by reasoning how things really are, and any man, if he have sufficient experience and reason enough about it, will be led to the one true conclusion. (W3: 254)

Reflection on four competing methods of inquiry shows that only one of them is capable of being sustained: it provides a non-accidental source for our opinions, assuring us that any settled belief it provides will be truly stable.

I have presented this argument rather as Peirce himself does: we want secure stable 'fixed' belief, and we compare four methods according to how effective they will be at providing it. So understood, the argument rests upon what appear to be contingent (and not invariable) features of human psychology: meeting others with different opinions tends to shake our confidence in opinions that we hold to 'tenaciously'. It also leads Peirce to make a claim which, there is every reason to think, he did not believe: that sentiment should not be allowed to have a role in settling our opinions; his later work does not suggest that beliefs resulting from sentiment are weak, unstable, driven by fashion, and easily doubted.

If the argument is intended to work in this fashion, it involves another massive weakness. When Peirce's account of science is worked out in full detail, it turns out that by adopting the method of science we postpone the removal of doubt for the sake of a settlement of belief which is truly stable. Adoption of the method of science provides no guarantee that we shall settle belief in the short run: indeed, one of its ethical glories is supposed to be the way in which it involves a subordination of our personal interest to a search for a settlement of belief by a wider community of single-minded scientific investigators. Although settlement of belief on the matters that interest me may not be reached until long after my death, my small contribution to this process ensures that my life has not been in vain. In that case, it is reasonable to protest, I have not obtained what I was looking for. I sought a method for removing the irritation of doubt; and I triumphantly endorse a method which requires me to put up with it in the hope that when, eventually, it is removed, the removal will be permanent. Worse: if we accept the 'no-belief' thesis, it seems, the doubt may be removed but not be replaced by settled *belief at all*.

In practical matters—when we confront 'vital questions'—this seems highly unsatisfactory. In such cases it matters less that we obtain a settlement of opinion that will be permanently settled than that we obtain a reasonably secure verdict soon. Hence the argument of the later papers

advocates something like the a priori method in connection with vital questions. But according to the published argument of 'The Fixation of Belief', that ought to yield opinions which immediately succumb to doubt.

Although this way of reading 'The Fixation of Belief' is very common, I doubt that it is the right way to do so. The text itself contains remarks about the strategy adopted in the paper, and these suggest another interpretation which avoids some of the troubling consequences just described. In section II, Peirce addresses the question of how to establish the correctness of logical principles. He proposes that certain guiding principles 'are necessarily taken for granted in asking whether a certain conclusion follows from certain premises'; and he argues that 'rules of reasoning which are deduced from the very idea of the process are the ones which are most essential', claiming that 'so long as [reasoning] conforms to these, it will, at least, not lead to false conclusions from true premises' (W3: 246). There is much evidence that Peirce takes himself to be carrying out such a 'deduction' in 'The Fixation of Belief' and the papers which follow it in the 'Illustrations of the Logic of Science'. His illustration of the 'facts from which logic starts out' begins:

It is implied, for instance, that there are such states as doubt and belief—that a passage from one to the other is possible, the object of thought remaining the same, and that this transition is subject to some rules which all minds alike are bound by. As these are facts which we must already know before we can have any clear conception of reasoning at all, it cannot be supposed to be any longer of much interest to inquire into their truth or falsity. (W3: 246)

When, towards the end of the paper, Peirce summarizes the evidence for the merits of the method of science, different styles of argument are employed. One is empirical: we are invited to be impressed by the triumphs of the method of science, particularly when we have noted that we unthinkingly use it much of the time for everyday investigations. Another is logical or presuppositional: the fundamental hypothesis of the method of science is involved in those facts taken for granted by the 'logical question':

The feeling that gives rise to any method of fixing belief is a dissatisfaction at two repugnant propositions. But here already is a vague concession that there is some one thing to which a proposition should conform. Nobody, therefore, can really doubt that there are realities, or, if he did, doubt would not be a source of dissatisfaction. The hypothesis, therefore, is one which every mind admits. (W3: 254)

One would then expect Peirce to claim that the hypothesis of reality is either itself one of the presuppositions of logic or is deducible from them. The flaws of the other methods all stem from the fact that they deny some feature of the method of science and are thus at odds with the presuppositions of the logical question. In that case, it is not so much the 'social impulse' that overthrows the earlier methods as the fact that they conflict with the underlying presuppositions of both the method of science and the logical question: the social impulse is merely evidence of that fact. If the presupposition of the method of science is indeed a presupposition of inquiry or of 'the logical question', then this would explain the power of the social impulse to constrain our opinions.

The two descriptions of what is going on in 'The Fixation of Belief' treat it as addressing different questions. The first claims that Peirce is asking:

1. Which methods of inquiry is it possible to adopt?

while the second identifies his problem as:

2. Which methods of inquiry can be vindicated by reference to the presuppositions of the logical question?

Peirce's strategy is to assume that the answer to question (1) will provide evidence in support of an answer to question (2). The later lectures suggest that this is a mistake: it is possible, and often desirable, to use methods which cannot be vindicated by reference to the presuppositions of logic.²² At best, question (2) shares an answer with question (3):

3. Which methods is it possible to adopt in inquiries that are subjected to reflective rational monitoring and control?

So long as we decline to submit our modes of belief formation to reflective, rational monitoring and control, then the methods of tenacity and

²² The position is slightly more complicated than this allows. Peirce probably holds that logical reflection can persuade us that, in connection with vital issues, we ought to trust opinions that are not grounded in logical self-control. Skagestad seems to suggest this when he says 'for the purpose of making decisive practical choices, instinct is a better guide than reason' (1981: 207) and cites Peirce's claim that animals, which never reason about vital matters, 'very rarely fall into error of any kind, and *never* into a vitally important one' (CP 1.649). For example, reason may explain that we are wise to *hope* that our sentiments will put us into harmony with truth and reason. Since this view would legitimate reliance on the a priori method only because doing so can receive a vindication which is more properly scientific, the general point stands.

authority and the a priori method might work. The weakness of Peirce's strategy of argument is that, if it works, it appears to be unreasonable (or 'unwise', or indeed impossible) to resist the demand that we exercise rational monitoring and reflective control in connection with all of our beliefs.

A brief digression concerning why Peirce's argument had this character is in order. Part of the story is that at the time of writing the 'Illustrations of the Logic of Science', Peirce was unable to give his philosophical 'system' the systematic grounding in semiotics and the theory of categories that it required: hence, he was still struggling to find satisfying arguments for positions that he was reasonably confident were correct.²³

Another part of the story concerns Peirce's complex relations to Kant. He saw himself as a broadly Kantian philosopher, who wanted to correct Kant's logic and improve on his system of categories. This makes it unsurprising that he would be interested in hunting down the presuppositions of logic, attaching importance to the fact that some proposition is presupposed by the logical question. In spite of this, however, he wanted to reject the transcendental method: showing that something was a presupposition of logic was no guarantee of its truth.²⁴ He repudiated the idea of a priori philosophical derivations; he insisted that philosophy had to be, in some fashion, scientific. Yet, we have seen, his project involves identifying what are, in some way, presuppositions. Hence, during the 1870s, he moved uneasily between at least three kinds of claim about their status: they are presuppositions of reasoning; they can be vindicated by a kind of empirical argument; and no proof of them is required because everybody believes them. If he could show that the method of science somehow reflected a presupposition of reasoning, he could hold that it is possible (and indeed wise) to turn one's back on reflective reasoning in some areas of one's life. Everyone (in principle) could acknowledge that if we were to submit some question to an investigation subject to full rational self-control, the method of science would have to be followed. But it might be possible to decline to submit some questions to such an investigation. Even in the 1870s, I suspect, Peirce wanted to say this. But, at that stage, methodological uncertainty prevented him from seeing clearly how he should or could defend this claim. This may also be responsible for the ambivalence exhibited in Peirce's comments, in those papers, about the merits of the other three methods (see W3: 255).

²³ See Hookway (1988a) and Chapter 4.

²⁴ For discussion of Peirce's attitudes towards the use of transcendental strategies in philosophy, see Oehler (1987) and Chapters 7 and 12.

4. Belief: Scientific Assent

The special character of scientific acceptance has two related sources. First, as we have seen, someone imbued with the true scientific spirit welcomes the refutation of 'established' views: anything identifiable as the elimination of error is embraced as a contribution to progress. Second, there is the special character of Peirce's logical vindication of the scientific method. Peirce believed he could show that adopting the scientific method, trusting abductive suggestions and submitting them to inductive testing, was a good strategy for contributing to scientific progress. A community of inquirers who proceeded in this fashion was fated eventually to reach the truth about the questions they raised. But logic could offer no reason for supposing that conclusions recommended by the scientific method at any particular time were true. There is no justification of the short-run reliability of induction: at best, we can say that the wealth of human experience over many thousands of years has ensured that the short-run reliability of induction in ordinary affairs of life is an instinctive or common-sense belief. In the short run, induction causes beliefs but does not, properly speaking, justify them: when we trust induction in the short run, the a priori method of fixing belief, trusting what seems reasonable or what it is natural to believe, is ultimately responsible for our believing what we do.

This ought to suggest that we should never cease inductively testing hypotheses; even observational claims should be subjected to endless empirical testing. All we can say of a scientific proposition is that, so far, it has not been refuted. In that case, there would be no such thing as scientific belief, but science would have a very odd character. It is not clear we could ever test a hypothesis, because we have to accept reports of experimental results in order to treat them as refuting current theories. Since experimental design depends upon current theory, experimentation would seem to be impossible too. And we would not be able to decide that a theory has survived severe testing and could be included in the list of 'established truths' to be used in formulating new theories and in constructing experiments. The question of the character of scientific assent concerns what our attitude to a proposition should be when we remove it from the list of propositions currently under test, and include it in the list of propositions that may be relied upon in testing other hypotheses. Ordinary experience suggests that we would then judge that, in all probability, the hypothesis is approximately true, although subsequent developments may lead us to revise or refine it, or to see it as a

limited special case of some more inclusive body of laws. The alternative picture is that we make a practical judgement which involves no commitment to the probable or approximate truth of the proposition: ceasing testing this proposition at this stage of inquiry will be the best means of making progress eventually towards the truth. In that case, we take up an 'as-if' attitude towards the possible truth of the proposition: it is now good to treat the world as if this proposition is true; but we have no right to believe that it actually is true. We might hope that it is approximately true, but that attitude shares with the practical as-if response a highly qualified, tentative, and detached relation to the proposition.²⁵ And, indeed, given the overall flavour of Peirce's philosophy of science, we might think that such a hope is unnecessary: at best we should hope that 'assenting' to the proposition will be useful as a way of making progress towards the truth.

This accords with another theme in Peirce's philosophy that became prominent during the 1890s; his increasing insistence that logical principles have the status of hopes (*NEM* 4: 19). Whenever we attempt to investigate a question, we hope that it has an answer, that convergence on one opinion would result in the long run of such investigations; we hope that we are sufficiently attuned to reality, that investigating hypotheses that seem plausible to us will serve as a useful means to reaching, sooner or later, the truth. We hope that contributing to the growth of knowledge will prove a fulfilling and satisfying project, and so on. Once again, we take a highly detached attitude towards our scientific commitments; and this is of a piece with the claim that science has no place for belief.

This connects nicely with the Kantian streak in Peirce's strategy that was noted in the last section. If showing that something is a presupposition of logic does not justify us in believing it, it can ground the hope that it is true: 'the true presuppositions are merely hopes' (*NEM* 4:19). So not only do current scientific results and currently approved theories function as objects of a kind of assent which is not straightforwardly cognitive, but so do the fundamental commitments which ground our acceptance of the scientific method and our adoption of the project of contributing to scientific knowledge: 'what is properly and usually called belief . . . has no place in science at all' (*CP* 1.635; *RLT*: 112).

²⁵ To avoid misunderstanding, I should point out that adopting an 'as if' attitude towards *current* theories is compatible with scientific realism. In fact, the detached attitude which it involves requires a realist view of theories: the scientist wants to accept a theory only if it is true, and she is aware that the current theory is accepted in the absence of any assurance that it meets this requirement. This issue is discussed further in Chapter 2, 4, and 5.

This seems a very unsatisfactory position. Peirce often speaks of the scientist's commitment to the growth of knowledge in almost apocalyptic terms. Consider a passage from 'The Fixation of Belief':

The genius of a man's logical method should be loved and revered as his bride, whom he has chosen from all the world. He need not condemn the others; on the contrary, he may honor them deeply, and in doing so he only honors her the more. But she is the one he has chosen, and he knows that he was right in making that choice. And having made it, he will work and fight for her, and will not complain that there are blows to take, hoping there may be as many and as hard to give, and will strive to be the worthy knight and champion of her from the blaze of whose splendors he draws his inspiration and his courage. (W3: 257)

The problem is that if one's assurance that progress can be made and that one is contributing to it amount to no more than a *hope*, if belief has no role in science, it is hard to see how he can 'know he was right in making that choice'. How can such detached and uncertain commitments motivate someone to continue to try to contribute to scientific activity? To return to a problem raised in our discussion of 'The Fixation of Belief': if we seek a method for removing the irritation of doubt, why should we so readily embrace a method which postpones the satisfaction we seek, holding out no more than the hope that the result of such self-denial will be an eventual settlement of opinion that will be more truly stable. If a hope is genuinely to motivate us to make inquiry our fundamental project, then, we are likely to think, some causal, psychological process must transform the hope into a living belief. And unless we believe that our currently accepted theories are likely to prove approximately correct, it is hard to understand what can motivate us to sustain our commitment to inquiry.

5. Confidence: The Life of Science

One way to challenge this sharp distinction between science and practice would be to deny the autonomy of theoretical science. John Dewey, for example, probably held that we can appreciate the value of theoretical investigations only when we place them in the context of inquiries designed to effect a real transformation in existential conditions, in response to vital concerns. The problem I am concerned with remains even if we grant Peirce the autonomy of pure theoretical inquiry. I want to suggest that his distinction between theory and practice cannot be formulated in the kind of terms he used in the 1890s.

Among the most pressing vital questions confronting an individual are those about what fundamental ends to adopt: which projects should we allow to give shape and meaning to our lives? Failure in such projects will be interpreted as failure in one's life. The questions of whether to be a scientist or philosopher, an engineer or surgeon, whether to live contentedly without ambition, and so on, are clearly among such vital questions. In that case, the decision to seek to exercise maximal rational self-control, using the method of science in order to contribute to our knowledge of reality, is a response to a vital question. In view of our comments about the argument of 'The Fixation of Belief', two difficulties face reflection about whether to adopt this project. We face, first, the question of how it is possible to adopt this project: since our fundamental motivation is to find a method for the settlement of belief, how can we be content with a method which promises very stable eventual settlement of belief but which holds out no assured prospect of stable settlement in the short run. The argument rests upon assumptions about human motivation which are neither stated nor defended. Second, if the argument of 'The Fixation of Belief' can be made to work, and if, as we have seen, it exploits the strategy of showing that only the method of science can be adopted for the stable settlement of belief, then, how is it possible to adopt a fundamental project other than contributing to science and philosophy? Furthermore, if both kinds of project are possible, how are we to choose between them? If the matter is to be settled by self-controlled rational inquiry, then, we might feel, the crucial decision has already been taken before one considers the question of how to live. Moreover, if self-controlled rational inquiry is involved, Peirce was wrong to deny its relevance to vital questions. If he is to be consistent, Peirce ought to hold that it is base and sordid to rely upon self-controlled rational inquiry when we settle such fundamental and vital issues. But if, as with other vital questions, we are guided by sentiment and instinct, then Peirce's denial that sentiment and instinct have a role in science is compromised. One passage in a draft of the 1898 lectures indicates recognition of these problems: if he were prepared to make an exception to his claim that scientific inquiry cannot be vitally important, he would 'make that exception in favour of logic; for the reason that if we fall into the error of believing that vitally important questions are to be decided by reasoning, the only hope of salvation lives in formal logic, which demonstrates its own ultimate subordination to sentiment' (*CP* 1.672). But this important insight is not developed in these writings.

Decisions about fundamental projects can be revoked; we can wonder whether to continue with our current projects, reflecting on their value

and on the value of our own contribution to them. And once we have adopted ‘the life of science’, decisions about which disciplines to work in, which specialities to enter, and which problems to tackle produce vital dilemmas which are not wholly solved by reference to the exigencies of funding. If these are ‘vital’ questions, then it would be ‘intellectual betrayal’ to expect scientific reflection to solve them. Such questions call for an answer that produces living belief. In that case, for Peirce, they cannot be answered without the aid of sentiment, instinct, and common sense. The scientist must be *confident* that the life of science is a possible one, that it can be (and will be) a rewarding one, that the contributions he or she is making are of value, and so on: this must be full belief, because it has to determine action in response to vital questions—it determines how the individual decides to live.

We see here, once again, the limitations of the perspective that Peirce had obtained when he wrote ‘The Fixation of Belief’. Reading that paper suggests that adoption of the method of science is the only possibility: we (quite generally) cannot sustain use of the other methods. In that case, the question what motivation we might have for devoting our lives to the exercise of the method of science and rational self-control seems to have little sense. From reading that paper, one would think that there was no alternative to adopting the method of science. In his later work, Peirce was forced to take seriously the fact that in many areas of life rational self-control has limited application. But during the 1890s, it seems to me, he had still not fully faced up to the question of how it is possible to live the life of science. How can we have the confidence in our contributions which is required if we are to be able to make a serious commitment to the life of science?

It is, of course, an oversimplification to deny that there is a serious response to these issues before 1900, but we can conclude that they were not addressed in ‘The Fixation of Belief’ or in the papers in which Peirce defended the ‘no-belief’ thesis.²⁶ A number of themes prominent in Peirce’s thought after 1900 contribute to a more sophisticated understanding of the ‘practice of theoretical science’. Although examining them

²⁶ A striking example of a fusion of sentiment and logic from the 1870s is Peirce’s insistence that probabilistic reasoning depends on the logical sentiments of ‘faith, hope and charity’ (W3: 281–5). But it should be noted that this occurs in the context of a strained (1877) attempt to deal with the relations of theory and practice: it can only be rational for me to rely on probabilities in connection with a particular case (a vital question) if my *sentiments* express my rational identification with the indefinite community of scientists. In 1898, we may suppose, Peirce would have seen this as a misguided attempt to show that rational considerations have a bearing on vital issues. See Chapter 9.

in detail would be a topic for another paper, I shall conclude this chapter by briefly listing some of these themes. In the 1903 pragmatism lectures, Peirce attempted to ground logic in ethics and aesthetics, developing views about the sorts of ends it was possible to adopt and to sustain in any circumstances. At the same time his classification of the sciences was being refined, and he was able to argue that mathematics, phenomenology, ethics, aesthetics, and logic lacked the kind of fallibility which grounded the claim that scientific acceptance should be so tentative. Hence, he was able to discuss the bases on which ultimate aims can be adopted and he had available a fund of certainties on which he could rely in order to do this. Two years later, he was placing most stress on the claims of common sense: we can be certain of a range of vague common-sense certainties which ground the scientific view of the world (*CP* 5.438–52; see also Chapter 8). The approximate correctness of mechanics, dynamics, and ‘rational man theory’ was part of common sense; the task of science was to replace vague certainties by precise testable laws and to explain these precise laws. Moreover, any scientific hypothesis is offered as a precisification of a vague picture whose vagueness permits considerable assurance that some filling in of its details is correct. So there is a provision for assurance about the value of the scientific enterprise and for considerable confidence in the approximate adequacy of current theory. Finally, during the second half of that decade, we learn that scientific observation is a species of religious experience, and that religious belief transforms the world into a benign and meaningful unity which invites our inquiries and investigation: our experience of the world is then suffused with the sentiment required to motivate inquiry (see Chapter 11 and Raposa 1989). Some of these themes are found in writings of an earlier period, but their prominence in this later work together with their evident relevance to the problem which I have discussed helps to make sense of the pattern of some of his later writing.