

The Key to Peirce's View of the Role of Belief in Scientific Inquiry¹

A Chave para a Visão de Peirce do Papel da Crença na Inquirição Científica

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Abstract: In the first of his 1898 Cambridge Conference lectures, Peirce declares that “what is properly and usually called *belief* [...] has no place in science at all.” Christopher Hookway and Cheryl Misak think that this thesis is ill advised; implausible in itself and out of keeping with the rest of Peirce’s philosophy. I argue on the contrary that proper understanding of Peirce’s distinction between two kinds of “holding for true”, the one practical and committed, the other theoretical and provisional, reveals the thesis in question – the claim that the former kind of holding for true “has no place in science” – to be both plausible and Peircean.

Keywords: Belief-doubt theory. Belief. Opinion. Truth. Scientific investigation. Conduct.

Resumo: *Na primeira de suas Conferências de 1898 em Cambridge, Peirce declara que “o que é comum e propriamente chamado de crença [...] absolutamente não tem lugar na ciência”. Christopher Hookway e Cheryl Misak pensam que essa tese é mal-expressa, implausível em si mesma e fora do contexto de todo o restante da filosofia de Peirce. Eu argumento, em sentido contrário, que um entendimento apropriado da distinção, por Peirce, entre duas espécies de “sustentar como verdadeiro”, uma espécie prática e comprometida, outra, teórica e provisória, revela que a tese em questão – a reivindicação de que a primeira espécie de sustentar como verdadeiro “não tem lugar na ciência” – é plausível e peirciana.*

Palavras-chave: *Teoria da dúvida e da crença. Crença. Opinião. Verdade. Investigação científica. Conduta.*

In *Philosophy and Conduct*, the first of the eight Cambridge Conference “class lectures” delivered between February 10th and March 7th, 1898 to an audience that included William James and Josiah Royce, along with Harvard students and members of the

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general public, Peirce argues that “what is properly and usually called *belief* [...] has no place in science at all”.² The purpose of this paper is to explain what this claim amounts to and why Peirce thinks it so important.

Peirce’s version of what Christopher Hookway and Cheryl Misak have come to call the “no belief in science” thesis³ can be expected to differ materially from the version (or versions) put forth by Karl Popper. Popper’s banishment of belief from science follows more or less deductively from his banishment of the knowing subject from epistemology. Peirce’s banishment of belief from science cannot be thus motivated; as its very name indicates, his doubt-belief theory of inquiry makes essential reference to certain psychological states of knowing, inquiring subjects.

In *Logik der Forschung*, Popper held that genuinely scientific theories were distinguished from non and pseudo-scientific theories by their openness to falsification.⁴ But by the time of his *Replies to My Critics* in the second of the two volumes in the *Library of Living Philosophers* dedicated to his work, he takes as his “paradigm for science” the “working of great scientists”, who follow, he maintains, the method of “bold conjectures and severe attempts at refut[ation]” (MILLER, 1985, p. 118-9): a logical doctrine about theories appears to have given way to a methodological doctrine about theorists. How this shift is to be reconciled with Popper’s continued commitment to an “objective” epistemology free of truck with knowing subjects is less than clear.

Peirce, unencumbered by *parti pris* against the subjective, wisely looks to the motivations of scientific inquirers rather than the formal structure of scientific theories from the very beginning? But why this focus is thought to engender an appreciation of the motivational advantages for scientific inquiry of sedulously keeping belief out of the picture, and how this latter view is to be reconciled with the stirring peroration on behalf of the merits of the scientific method of fixing belief which closes one of Peirce’s best known papers, the pregnantly entitled “Fixation of Belief”, have long been a source of vexation to interpreters of his philosophy. For as Hookway observes, “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*” (BCM 23, emphasis in original). So a subsidiary goal of the paper is to resolve this conundrum.

² PEIRCE, Ch. S. *Reasoning and the Logic of Things*. Ed. by Kenneth Ketner and Hilary Putnam. Cambridge MA: Harvard University Press, 1992, p. 113, emphasis in original. Further references to this work will appear within parentheses in the text as RLT followed by page number. References to the HARTSHORNE-WEISS edition of Peirce’s *Collected Papers* will be by volume and paragraph number, and references to the Richard Robin catalogue of Peirce’s manuscripts will appear as MS followed by the Robin Catalogue number.

³ Hookway’s most extensive discussion of the thesis is found in “Belief, Confidence and the Method of Science”, *Transactions of the Charles S. Peirce Society*, v. XXIX, p. 1-32, 1993, reprinted in HOOKWAY (2000, p. 21-43); Misak’s in “C. S. Peirce on Vital Matters”, *Cognitio*, São Paulo, n. 3, p. 64-82, 2002. Further references to Hookway’s paper will appear within parentheses in the text as BCM followed by the page number in HOOKWAY (2000), and to Misak’s as PVM followed by page number.

⁴ POPPER (1959, p. 78 ff.). I use the original German title in the text because its English translation is so unaccountably dreadful: *The Logic of Scientific Discovery* for a book that argues that there is no logic of scientific discovery, only a logic of scientific justification!

Neither Hookway nor Misak think very well of the “no belief in science” thesis. Misak regards it as a regrettable error, albeit one that is easily excisable from the main body of Peirce’s thought (PVM 75); Hookway thinks that it leads to “a very unsatisfactory position”, albeit one soon replaced by the “more sophisticated understanding of the ‘practice of theoretical science’” (BCM 42) vouchsafed by writings from the last decade and a half of Peirce’s prolific life. I think that the position Peirce argued for in the spring of 1898 is, *pace* Hookway, in no way anomalous, and moreover, *pace* Misak, true and interesting. What Peirce has recognized, to state the main point very roughly, is that the role of belief in genuine inquiry and the life of science is radically different from its role in action and practical decision generally and vitally important action and practical decision in particular.

On the face of it, Misak has a point: the view that science has no truck with belief is, and this would certainly be *le mot juste*, incredible. For we all, layman and scientist alike, believe many things that have been found out, not by unaided common sense, but by arduous scientific labour; we believe that the earth revolves around the sun, that the atomic number of gold is 79, and that DNA is a double-helical, backbone-out macromolecule with irregular bases stacked up inside, to give three examples in ascending order of sophistication and venerability, and descending order of clear and distinct comprehension by me. Not to mention that, as Misak is quick to point out, Peirce seems to recant the offending thesis just a few sentences after having loudly proclaimed it. For he grants that scientists are “in the habit of calling [some of their conclusions] *established truths*” (RLT 112, emphasis in original). “These established truths”, Misak declares, “are the body of background belief which we take for granted. It is what the critical commonsense philosopher focuses upon. It is belief in science” (PVM 75).

The claim that a body of established truth amounts to a body of background belief might be supported by the following argument. To call some proposition *p* an established truth, *in propria persona* and in the right tone of voice, is, ineluctably, to grant the truth of *p*; and to grant the truth of *p* is to believe that *p* is true, and so to believe that *p*. *Ergo*, anything that a scientist “is in the habit of calling an established truth” is something that he believes to be true; insofar, therefore, belief does, demonstrably, have a place in science.

Or does it? What the “Philosophy and Conduct” version of the “no belief in science” says with the ellipsis interpolated above filled in and the clarificatory sentence that follows added, is that “what is properly and usually called belief, that is, the adoption of a proposition as a *κτημα εις αιει* (a possession for all time), to use the energetic phrase of Dr. Carus, has no place in science at all”. The list of propositions accepted in science is instead expressly provisional and its contents are “but opinions, at most” (RLT 112). A decade after the Cambridge lectures, in the rich final paragraph of his *Neglected Argument for the Reality of God*, Peirce adverts again to this distinction between belief in the full and proper sense and the kind of opinion that is proper to science. Drawing attention to a peculiarity of “scientific inquiry into the origin of the three Universes”, he writes that this *sui generis*, musement-inspired, religiously oriented enterprise will, if successful, produce “not merely scientific belief, which is always provisional, but also a living, practical belief, logically justified in crossing the Rubicon with all the freightage of eternity” (6.485).

So: the full, living belief of momentous action is contrasted with the provisional, quasi-belief of scientific inquiry, the difference being that between an attitude held

provisionally and one “adopted for all time”, “with all the freightage of eternity”. While a scientist “ought to be in a *double* state of mind about [a promising hypothesis], at once ardent in his belief that so it must be, and yet not committing himself further than to do his best to try the experiment” (MS 175, emphasis added), a true believer, one might infer by contrast, will be resolutely *single-minded*, irrevocably committed to the propositions he believes. In a brief, admiring discussion of Peirce on this topic, Thomas Nagel is remarks that, although he himself “find[s] this use of the word ‘belief’ somewhat peculiar” (NAGEL, 1997, p. 128), he takes the point Peirce wants to make about science – in Nagel’s words, that “the only way we can have any hope of advancing toward the truth is to be continually dissatisfied with our opinions, to be always on the lookout for objections, and to be prepared to drop or alter our theories whenever counterevidence, counterarguments, or better-supported alternatives present themselves” (ibid.) – to be nonetheless clear.

But the thesis that full belief has no place in science now appears in danger of having been rescued from patent implausibility at the cost of latent banality. For it seems obvious that inquirers are not inquiring well when and to the degree that they are in the grip of convictions to which they will obstinately cling come hell or high water, too obvious to require the repeated emphasis on the point that we find, not only in *Philosophy and Conduct*, but elsewhere, especially in the fourth lecture of the series, *The First Rule of Reason*. Besides, one can let Peirce have his distinction between fully committed, living belief and merely provisional scientific opinion, and still maintain that, in virtue of his admission that scientists are in the habit of thinking of many of their conclusions as established truths, he tacitly grants that they do have beliefs about the subject matters into which they inquire. Calling the attitude of scientists to established truth “opinion” does nothing to change the fact that “opinion” is another word for “belief”; and a belief held ever so fallibilistically, is still, *inter alia*, a belief.

I grant that opinion ordinarily means much the same thing as belief and also that fallibilist belief is still, *inter alia*, belief. But what Peirce is getting at is not incompatible with either of these truisms. His is a thesis about the character of inquiry and the motivation required of those committed to engaging in it. Since he is not, in particular, interested in what scientists do or believe *outside their capacity as scientific inquirers*, it is, happily, not a consequence of his view that a scientist – who believes the propositions he is in the habit of calling established truths? Who believes, say, that the earth revolves around the sun, that the atomic number of gold is 79, and that DNA is a double-helical, backbone-out macromolecule with irregular bases stacked up inside – is thereby shown to be disreputably credulous.

Consider, in context, Peirce’s acknowledgement, so unaccountable to Misak given the tenor of the rest of the paragraph in which it appears, that scientists call many of their conclusions “established truths”:

[P]ure 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. Nothing is *vital* for science; nothing can be. Its accepted propositions, therefore, are but opinions, at most; and the whole list is provisional. The scientific man is not in the least wedded to his conclusions. He risks nothing upon them. He stands ready to abandon one or all as soon as experience opposes them. 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. (RLT 112)

In the first half of this passage Peirce speaks of science, the activity itself; in the second of scientists, the men and women who engage in it. The casualness of the slide between the two subjects suggests that Peirce is thinking of them together, as an indissoluble whole, two sides of the same coin. His claims about belief and conclusions and established truths pertain to scientific inquirers insofar (and only insofar) as they really are such, are genuinely engaged in the project of scientific inquiry. What he claims about scientific inquirers, so understood, is that: (a) they take themselves, collectively, to be in possession of a stock of established truths "to which no competent man demurs"; but they nevertheless (b) are not wedded to any conclusion, no matter how well established; which is to say (c) that they stand ready to abandon one or all as soon as experience opposes them; and they are able thus to exempt themselves from the forces that naturally bind them to their own conclusions at least in part because (d) they risk nothing upon these conclusions.

It might be said that Peirce's insistence that men of science "are not in the least wedded to their conclusions" is beside the point, which is that they do, admittedly, *have* conclusions, the validity of which they must accept. If they are antecedently committed to the truth of the premises from which they infer their conclusions,⁵ then in concluding that p they commit themselves not only to the validity of the inference to p from its premises, but to the truth of p; which is to say that they come to believe that p. We seem to be back where we were a couple of paragraphs ago, at the claim that no refusal to pledge eternal fidelity to one's beliefs or conclusions can stop them from being, *pro tem*, one's beliefs and conclusions; the scientist's denying these beliefs and conclusions any right of permanent abode in his mental household seems rather to presuppose than to preclude, their current residence at that address.

Peirce, however, says explicitly that "*holding for true* is of two kinds; the one is that practical holding for true which alone is entitled to the name of Belief, while the other is that acceptance of a proposition which in the intention of pure science remains always provisional" (RLT 178, emphasis in original). So insofar as any holding for true is, *eo ipso*, a belief (of some kind or in some degree), Peirce grants Misak her point; he agrees that scientists believe their deeply entrenched background theories to be true, and is therefore not saddled with the implausible, previously cited view that "application of the scientific method cannot (or should not) produce belief" (BCM 23, emphasis deleted). But he does insist that scientific inquiry has as such no room for capital B Belief, fully committed, practical holding for true, and we should ask why he would do this.

According to Hookway, the reason is that belief "will always have causes over and above any reasons we may have for holding it" (ibid, 30). Beliefs sustained by causes that are not also reasons are, insofar, beyond the reach of "rational self-control", so that "what is wrong with full belief, from a scientific point of view, is that it is not subject to my control" (ibid.). The provisional holdings for true that are alone acceptable in science, the states that Peirce, "Philosophy and Conduct", called "opinions at most" are, Hookway suggests:

⁵ Better, if they are antecedently committed to the truth of the propositions that would compendiously express the evidence that they take to warrant the conclusions.

Beliefs about which we are tentative or uncommitted, in which case the grip of the causal processes which have transformed scientific assent into (weak) 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 (ibid., 31-2).

I think that this gets off on the wrong foot. Science, as Peirce understands it, is not, fundamentally, “the epitome of rational self-control” (ibid., 31), though it both requires and enhances such control; science for Peirce is fundamentally the epitome of man’s capacity for genuine inquiry; in his own words “[science] embodies the epitome of man’s intellectual development” (7.49), and more expansively,

Science is to mean for us a mode of life whose single animating purpose is to find out the real truth, which pursues this purpose by a well-considered method, founded on thorough acquaintance with such scientific results already ascertained by others as may be available, and which seeks cooperation in the hope that the truth may be found, if not by any of the actual inquirers, yet ultimately by those who come after them and who shall make use of their results. (7.54)

The difference between rational self-control as such and genuine inquiry matters because the former concept can apply equally to the practical and to the theoretical realms, which is to say that it elides the very distinction Peirce endeavours securely to establish.

“Upon this first, and in one sense this sole, rule of reason, that in order to learn you must desire to learn and in so desiring not be satisfied with what you already incline to think, there follows one corollary which itself deserves to be inscribed upon every wall in the city of philosophy: Do not block the way of inquiry” (RLT 178): this justly famous maxim of Peirce’s appears immediately after the paragraph in which he makes the distinction noted above, between practical and scientific holding for true. Here is the sentence that negotiates the transition from the drawing of that distinction to the statement of the first rule of reason and its celebrated corollary:

To adhere to a proposition in an absolutely definitive manner, supposing that by this is merely meant that the believer has personally wedded his fate to it, is something which in practical concerns, say for instance in matter of right and wrong, we sometimes cannot and ought not to avoid; but to do so in science amounts simply to not wishing to learn; Now he who does not wish to learn cuts himself off from science altogether. (ibid.)

To be “wedded” to a proposition, Peirce explains, is to yoke one’s fate to its truth, to risk (or at least be willing to risk) a great deal on things turning out actually to be as they are believed to be. Where scientific believers risk nothing on the truth of their stock of “established truths”, full, practical believers must, by stipulation, be willing to risk everything. Full practical belief is inimical to science because it “blocks the way of inquiry”.

Consider Smith and Jones, both long time associates of Robinson, who is under suspicion of having embezzled a large sum of money from the company that employs all three individuals. Smith is convinced that Robinson is thoroughly honest and cannot be guilty, Jones, perhaps for sheer want of any credible alternative suspect, that he

simply must be guilty. Each man stakes his reputation and career on the truth of his belief. It follows, Peirce would say, and rightly, that these two men have disqualified themselves from undertaking any genuine *inquiry* into who was responsible for the embezzlement. The “absolutely definitive manner” in which they adhere to their mutually contradictory propositions prevents them from granting that there is anything substantial to learn from such an inquiry, anything beyond superfluous confirmation to be gained. As far as these two are concerned, the truth does not need to be sought; it has already come to light. The one may be deserving of praise for his loyalty to a friend, the other for his loyalty to the company and his willingness to face up to the “unpleasant fact” that someone who long appeared to be honest has, alas, turned out not to be so; what Peirce would have us recognize is that both have “cut themselves off altogether” from *investigation* into the matter, scientific or otherwise.

Hookway glosses the provisional, “mere opinion” brand of holding for true that Peirce claims to be the only kind allowable in science as belief that is “tentative or uncommitted”. He appears to regard these two properties as equivalent, both being tantamount to the simple property of being “weak”. The picture thus encouraged is that of a scale of conviction ranging from the firmer to the fainter; though epistemically ideal researchers would eschew scientific conviction altogether, human frailty can be indulged and a modicum of credulity tolerated, as long as the commitment involved remains sufficiently feeble. Peirce, however, speaks not of belief that is especially weak or faint, but of belief that is essentially provisional. And he is evidently of the opinion that a belief can be held provisionally but ardently, as in his recommendation quoted above that a scientist inclined to favour a certain hypothesis “ought to be [...] *ardent* in his belief that so it must be” (MS 175, emphasis added).

Fair enough you might say, but how is Peirce helped by this inadequacy of Hookway's attempt to make sense of his view? Does he not now owe us an account of how it is that scientists can be positively encouraged ardently, if provisionally, to believe that things *must be* as their favoured hypotheses represent them as being, but strictly forbidden fully to believe that things *actually are* that way? He does and he can provide it. For what Peirce has recognized, in effect if not in so many words, is that beliefs or holdings for true can differ in two different ways; there is on the one hand sheer degree of credence, which might be defined as the inverse of the degree to which one would be surprised to be proven wrong; and on the other hand a distinction of kind between the theoretical – more accurately, to speak ancient Greek transformed into archaic English, the “zetetic”, i.e. “proceeding by inquiry or investigation”, but I will not inflict that on you? the practical manners or modes or contexts of holding for true. Since the two dimensions cut across one another, it is possible to have either a high or a low degree of credence in either the theoretical or the practical manner. Those propositions taken to be “established truths of science” that have already figured so prominently in the discussion would be examples of things believed very firmly, yet provisionally. Weak belief in the theoretical mode would be most naturally found at the speculative forefront of current research, in the “frontier science” stratum of Henry Bauer's “knowledge filter”.

For weak belief in the practical mode we turn, for example, to people standing before a recently frozen pond, wondering whether the ice is thick enough to walk on, concluding that it probably is, and setting off, not without residual trepidation, but free of real anxiety. Strong belief in the practical mode is, of course, Peirce's full belief, belief

“properly and usually so-called”, “willingness to act upon the proposition in vital crises” (RLT 112). It is belief of this sort and this sort alone that Peirce is concerned to dismiss from science, and the point of doing so is, *pace* certain deliberate insinuations above, far from banal.

The following table illustrates, with examples, these cross-cutting dimensions of belief.

Degree of Credence

(= how strongly one expects to be right, which is the inverse of how surprised one would be to be proven wrong)

		Strong	Weak
Context/ Manner of Commitment (=the costs of being proven wrong/the degree of investment in the proposition independently of the balance of evidence with respect to it)	Theoretical	The earth revolves around the sun. The atomic number of gold is 79. DNA is a double- helical, backbone- out macromolecule with irregular bases stacked up inside.	This new multi billion dollar particle accelerator will confirm the existence of the Higgs boson.
	Practical	I believe in one God, the Father Almighty, maker of heaven and earth ... Robinson is innocent/guilty.	This ice is thick enough to walk on.

Hookway asks “what [we should] make of Peirce’s claim that scientific assurances are ‘but opinions at most’? Are they opinions or not even that?” (BCM 31), and goes on to give the “tentative, weak belief” interpretation just criticized. The correct answer to his question, we can now see, is that scientific convictions are “but opinions *at most*” because all that matters is that they are not full beliefs, binding commitments to truth in the practical mode. All doxastic commitment in the theoretical mode is as such provisional, independently of strength or firmness; and all relatively weak commitment is provisional too, as least incidentally, independently of manner or context. We trust that the ice is thick enough to bear our weight, though with less than full confidence; this is to say that we will not be altogether surprised if we end up unpleasantly surprised.

The point of holding things for true in theoretical contexts is to find things out; the point of holding things for true in practical contexts is to get things done. When the situation in which an agent finds himself constitutes a vital crisis as Peirce conceives it, a belief that guides action is, typically, unrevisable for the sufficient reason that there is no second chance, no occasion on which revision could occur. In the limiting, and for Peirce the paradigmatic case, the falsification of such a belief results in the death of the believer or worse, the worse being, as Pascal famously argued, missing out on eternal good fortune. The consequences of falsification for either Smith or Jones in the embezzlement example will be not quite as severe as this; but they will be serious nonetheless. When and if the truth of the matter does emerge, when and if it becomes plain for *all* to see that Robinson is guilty or innocent as the case may be, one of his two associates will be in the position of having, in his own person, learned a very hard lesson in human fallibility.

In science, by contrast, understood “not as the work of one man’s life, but as that of generation after generation, indefinitely” (RLT 177), there is always a second chance, and a third, a fourth, a fifth and indefinitely many more. The essential provisionality of a truly scientific commitment to an established truth is indicated signally by the fact that if the confident expectations of the man of science are foiled and a heretofore established truth is “refuted tomorrow” “[he] will be glad to have got rid of an error” (RLT 112).

And the point applies, in its own way, to the most humdrum of inquiries. Suppose that you are looking for your keys, which are, you are quite certain, somewhere on the second floor. If they are on the second floor, no amount of searching anywhere else will do any good; but if they are not on the second floor, no amount of searching there will do any good. Wherever they are, a thorough search of the relevant portion of the house ought to turn them up, and when one has thoroughly and fruitlessly turned the second floor upside down in search of the missing keys, it is likely to dawn on even the most stolidly uninquisitive that the heretofore guiding assumption might be mistaken. Such a one who, in the face of the mounting evidence against the view that the keys are on the second floor, saves himself precious minutes by trying out the unlikely idea that the keys are in the basement, and discovers that that is indeed where they are, will be just as glad “to have got rid of an error” as Peirce’s model man of science.

Not that a scientific man “glad to have got rid of an error” is necessarily a man who is glad, any more than a helpful man who, under melancholy circumstances, is happy to oblige must therefore be a happy man. A scientist who has put much intellectual toil and emotional energy into devising, refining, testing, and promoting a “cherished hypothesis”, who has – let us now have Peirce speak for himself – “made it his companion by day and by night, and given to it his strength and his life, leaving all other occupations for its sake” (5.393), cannot be expected to be exactly pleased by the news that a paper has just been published that appears to refute it entirely. But if, as I hereby stipulate it does, the paper contains material that is useful for science, its appearance, in Peirce’s view, must be a welcome event for the community of scientific inquirers. *Qua* man who devoted himself to the elaboration of the hypothesis that appears to be undermined by the results, a man who very much *wanted* to be the one who discovered that things were as he had thought they were, the unfortunate soul in the example will, I expect, be devastated. It is only as a scientific man that he must find the bad news just as useful and valuable as good news would have been. For a scientific man is identified as such by his stake, not in this or that turning out to be true, but simply in turning up the

truth, whatever it may be; the “dominant passion of his [...] soul [is] to find out the truth in some department, regardless of what the color of that truth may be” (7.605).

When Peirce explains the essential provisionality of doxastic commitment in science by saying that though “we are driven oftentimes in science to try the suggestions of instinct [...] we only try them, we compare them with experience, we hold ourselves ready to throw them overboard at a moment’s notice from experience” (RLT 112), Misak thinks that he makes a mistake. “As Duhem and Quine and Kuhn have gone so far to show us”, she explains, “no scientific theory is overthrown in a flash by a lone experience. Scientists tend to insulate their theories from rogue experiences until the theory can bear such insulation no more” (PVM, 75). Peirce’s alleged mistake can be brought into relief by asking why the protagonist of our example above need be so submissive in the face of the newly discovered evidence that is so unfavourable to his hypothesis. The example took that man’s willingness to submit to the *force majeure* of experiential fact to be a mark of his exemplary epistemic character from the Peircean point of view. But why might he not contribute just as effectively to the scientific enterprise by reacting, not with abject submission, but with redoubled effort on behalf of his view? He might look very carefully into the credentials (!) of the results on which the troublesome paper is based, or set to work on imagining ways in which the appearance of incompatibility between the new claims and the cherished hypothesis might be unmasked as “merely apparent”, not real, the underlying truth of the matter being more interesting and complicated in itself? and altogether more welcome to our hero, who might take heart from Hookway’s confident assurance that “modern historiography of science suggests [that] dogmatically continuing to believe an otherwise attractive theory which has failed in some of its predictions may prove the best strategy for improving our understanding” (HOOKWAY, 2000, p. 210).

Since he is a creature of my own devising, I can report with authority that the scientist in question thought long and hard about clinging dogmatically to his view and concluded that it would be intellectually dishonest to do so. You may imagine him or her as a Linus Pauling, only without the Nobel Prize, or even tenure, reading the Crick and Watson paper that disclosed the structure of DNA; or those poor fellows who “discovered cold fusion”, reading the day’s mail with yet more disappointing reports from colleagues unable to the Utah lab’s exciting results: or Frege reading the letter in which Russell informs him of that the *Grundgesetze* system of logic is paradoxical. The kind of work supposed in my example is of this undeniable character and caliber; no mpu (minimal publishable unit) report of some “lone, rogue experiences”, but an unexpected breakthrough (or, in the cold fusion case, a sobering refutation of what would have been an exhilaratingly marvelous breakthrough). It is in any case not Peirce’s view that the more willing a scientist is to jump ship at the first sign of inclement evidential weather the better; to be willing to abandon belief in a heretofore established truth “as soon as experience opposes it” does not require being willing to admit that experience has pronounced on the subject in virtue of a smattering of failed predictions. An unsuccessful perfunctory second floor search for those missing keys leaves it quite open whether it is now wiser to go over the same ground again more carefully, or look elsewhere.

Peirce is concerned to insist only that in science, which is to say in the course of sustained inquiry engaged in for no ulterior purpose, ventures at the truth are just that, ventures, which may or may not succeed according to how they fare at the bar of

experience in the indefinitely long run. When they fare badly enough in the short to medium term, there is no scientific point to holding on to them for the moment, not unless and until some further, unexpected developments suggest that it might be time to try them out again in newly promising circumstances. The epistemological holism appealed to by Misak and the historiography of science cited by Hookway pertain to the question of exactly when and under what conditions different scientists will or should cut their losses and give up trying to mend or improve a given theory or research program, and this question lies outside the scope of the no belief in science thesis as Peirce conceives it.

Not that Peirce is uninterested in questions about how scientists can most effectively deal with unresolved issues. It is, in fact, a merit of his emphasis on the ineluctably social character of the scientific enterprise that it allows him (and us) to agree with Susan Haack (and sound reason) that “scientific inquiry [...] is apt to go better when the community includes [...] some who are quick to start speculating about new theories when the evidence begins to disfavour the presently dominant view, and others who are more inclined patiently to try modifying the old” (HAACK, 2003, p. 197). Peirce’s name for the scientific study of how most efficiently to proceed in science was the “economy” or “economics” of research. But he makes it clear that that topic is not to the point as far as the importance of the First Rule of Reason is concerned. For immediately after stating this rule, he allows that “although it is better to be methodical in our investigations, and to consider the Economics of Research, yet there is no positive sin against logic in *trying* any theory which may come into our heads, so long as it is adopted in such a sense as to permit the investigation to go on unimpeded and undiscouraged” (RLT 178-9, emphasis in original). The need to distinguish full, practical belief from provisional scientific belief is rooted in the idea that belief of the former kind very thoroughly and very insidiously impedes and discourages honest, fruitful investigation.

Hookway observes in a footnote that, in *The Fixation of Belief*, Peirce “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” and goes on to note that “the tensions” he finds in Peirce’s thought on the subject “begin to emerge when it is asked whether these two characteristics identify the *same* states.” (BCM 23)

Although I naturally deny the imputation of unfortunate “tensions” to Peirce in his thinking on the character of belief, Hookway’s question is apt enough and should be answered in the affirmative. Insofar as it is a mental, and perhaps also or ultimately a physical, state, the belief that *p* is not a different thing according to whether the manner of doxastic commitment is full and practical or provisional and theoretical. Imagine, if you will, an exemplary Peircean scientist, a chemist let us say, accosted one night by an exemplary psychotic aficionado of Monty Python like humour: the armed nutter, loaded pistol in one hand, copy of the periodic table in the other, says to the chemist, “tell me the atomic number of gold or I will shoot you”. The chemist, assuming he is not suicidal, will answer “79”, thus indicating a willingness to rely on the truth of this proposition in a vitally important context. It would be odd indeed if whatever it is in him that realizes or constitutes his belief that the atomic number of gold is 79 were to be altered in some fundamental way by its happening to be put to such unusual, life-saving use – and it is worth noting how much more natural it would be to say that what saved him was not his

belief that the atomic number of gold is 79, but his *knowledge* that that is the correct number.⁶

The essential provisionality of belief in the context of inquiry is, I suggest in conclusion, a function of what might be called its immediate mental environment. The chemist has no current doubts about the atomic number of gold, either in the lab or on the street; but his thirst for chemical truth as a whole is far from slaked, and that is why he is happy to grant that future developments in the subject may put the long established truths of the periodic table generally, and of the location of gold within it specifically, in altogether unexpected lights. The searcher after those wretched keys begins with no doubt that they are to be found somewhere on the second floor, but he has not, by hypothesis, found them yet and since he also does not, if he is sane, take his memory (or whatever else it is that sustains his conviction that they are on the second floor) to be infallible, he too must acknowledge that his initial assumption is subject to revision, and must be prepared to look elsewhere if need be. The beliefs of Peircean inquirers are all of them subordinate to the desire to know, the Will to Learn, as Peirce puts it, alluding reproachfully to the title of James's famous essay, which had been published a year earlier in a volume dedicated to "my old friend Charles Sanders Peirce"; and in this respect, "Peircean inquirers" are simply **inquirers**, *tout court*.

⁶ Very fortuitously from my point of view, it turns out that this concocted example is not even all that fanciful. "Here", relates the Russian physicist George Gamow, "is a story told to me by one of my friends who was at that time a young professor of physics in Odessa. His name was Igor Tamm (Nobel Prize laureate in Physics, 1958). Once when he arrived in a neighbouring village, at the period when Odessa was occupied by the Reds, and was negotiating with a villager as to how many chickens he could get for half a dozen silver spoons, the village was captured by one of the Makhno bands, who were roaming the country, harassing the Reds. Seeing his city clothes (or what was left of them), the capturers brought him to the Ataman, a bearded fellow in a tall black fur hat with machine-gun cartridge ribbons crosses on his broad chest and a couple of hand grenades hanging on the belt.

"You son-of-a-bitch, you Communistic agitator, undermining our mother Ukraine! The punishment is death."

"But no", answered Tamm. "I am a professor at the University of Odessa and have come here only to get some food."

"Rubbish!", retorted the leader. "What kind of professor are you?"

"I teach mathematics."

"Mathematics?" said the Ataman. "All right!" Then give me an estimate of the error one makes by cutting off Maclaurin's series at the n^{th} term. "Do this and you will go free. Fail, and you will be shot!"

Tamm could not believe his ears, since this problem belongs to a rather special branch of higher mathematics. With a shaking hand, and under the muzzle of the gun, he managed to work out the solution and handed it to the Ataman.

"Correct!" said the Ataman. "Now I see that you really are a professor. Go home!"

Who was this man? No one will ever know. If he was not killed later on, he may well be lecturing now on higher mathematics in some Ukrainian university (GRATZER, 2002, p. 44).

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