Peirce on vital matters and the scientific method

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Abstract

In this paper, I try to make sense of some puzzling claims that Peirce makes in the *Cambridge conferences lectures*. I identify four tasks that a successful interpretation of those claims must accomplish. First, we must provide a plausible reading of the "no belief in science" thesis. Second, we must provide a compelling interpretation of the "no science in vital matters" thesis. Third, we must explain Peirce's distinction between two forms of holding for true. Fourth, we should be able to solve the conflict with "The fixation of belief." I start by analysing Christopher Hookway's reading of Peirce's claims because he clearly identifies these tasks and offers an interpretation with considerable merits. However, I identify a problem in Hookway's reading, since he fails to account for a normative dimension of the "no science in vital matters" thesis. Finally, I sketch my attempt to accomplish these four tasks while also avoiding this latter problem.

Keywords: Charles S. Peirce; Christopher Hookway; Belief; Assent; Instinct; Sentiment; Method of science; Vital matters; The fixation of belief

1. Introduction[1]

In the *Cambridge conferences lectures* of 1898, Peirce makes a series of claims that are puzzling in various respects. In the first lecture, "Philosophy and the conduct of life," he famously stresses that there is no place for belief in science (RLT, 112).[2] By contrast, he submits that in "vital matters" it

[1] I am really honored to contribute to this volume dedicated to the work of Christopher Hookway. As it is clear from my chapter, I regard his work as a continuous source of inspiration and insights. Chris is a unique example of how to combine philosophical acumen and historical sensibility. I was lucky to be able to benefit from his advice while I was visiting Sheffield during my PhD. But Chris is not only an exceptional philosopher. He is also a wonderful and kind person who has never confused requiring the highest standards for philosophical discussion with competing on whom has the strongest argument.

[2] I will use the following abbreviations to cite Peirce's writings: RLT for Peirce 1992; W for Peirce 1982-, followed by volume and page number; EP for Peirce 1992-1998, followed by volume and page number; R for Peirce's unpublished manuscripts in Houghton Library at Harvard University, followed by manuscript number (according to Robin, 1967).

is advisable to rely on instinct or sentiment, not on reason or science, in order to decide what to believe and how to act accordingly (RLT, 110). In the fourth lecture, "The first rule of logic," he adds that we must distinguish between two radically different forms of "holding for true" (RLT, 178). The former is what is properly called "belief" and obtains in practical contexts. The latter is scientific holding for true, which is always provisional and disregards possible practical applications.

First of all, these claims are puzzling on their own right. Take the claim that belief has no place in science. Why shouldn't the scientist be allowed to have beliefs regarding well established scientific theories, such as evolutionism, or facts, such as that the chemical formula of water is H_2O. Relatedly, why shouldn't we follow science in deciding how to act in "vital matters"? A scientific theory that has obtained consensus displays a considerable amount of evidence in its favour. Therefore, it seems that we would be well advised to rely on it in our practical considerations, given the level of rational scrutiny that it underwent. Furthermore, saying that there exist two different kinds of "holding for true" appears to be problematic, unless we are able to clearly identify criteria to tell them apart.

A second group of problems arise when we compare these claims with others contentions Peirce makes in one of his most famous texts, "The fixation of belief." There, the method of science is described as a method of fixing belief, which seems to conflict with the idea that there is no space for belief in science. Moreover, the procedure for fixing beliefs in "vital matters" resembles what in "fixation" is called the a priori method. In "fixation," however, Peirce appears to maintain that we have reason to prefer the method of science over the a priori method, where this claim is not limited to scientific contexts.

How should we answer these worries? An easy way out is to suggests that Peirce's claims should not be taken seriously, since they are just a polemic response to William James's request that he put more emphasis on vitally important topics in his lectures (see Misak 2004: 163). However, this solution cannot be considered satisfactory. In fact, Peirce's problematic claims in the *Cambridge conferences lectures* cohere with other contentions he makes in other texts of the same period and in later writings,[3] which means that we cannot simply ignore them.[4]

[3] I have Peirce's claims on instinct, common sense, and *il lume naturale* in mind. For an account of how these different topics hang together see: Boyd and Heney 2017. On Peirce's late account of "rational" instinct see: Maddalena 2003.
[4] Of course, I do not want to suggest that James's request and Peirce's willingness to differentiate his position from his did not play any role in the production of the *Cambridge conferences lectures*. These factors should be taken into

Providing a convincing and successful account of Peirce's problematic claims in the *Cambridge conferences lectures* involves at least four tasks. First, one must provide a plausible reading of the "no belief in science" thesis. Second, and similarly, one must provide a compelling interpretation of the "no science in vital matters" thesis. Third, these reading and interpretation must be related to a defensible way of making the distinction between two different forms of holding for true. Fourth, and finally, they should be able to solve or soften the conflict with "fixation."[5]

There have been various attempts to account for Peirce's claims in the *Cambridge conferences lectures*,[6] but very few of them have tackled all these tasks at once. One interpretation that did that and that, in turn, was also one of the first to take Peirce's claims seriously, has been provided by Christopher Hookway (Hookway 2000: ch. 1).[7] Hookway's interpretation circles around the distinction between "full belief," which is the kind of belief that is capable of guiding our action, and other forms of "assent," among which we also find "scientific" assent. I believe Hookway's proposal remains one of the strongest in the literature, at least because it clearly shows what is at stake if one wants to provide a successful interpretation of the aforementioned claims.

I will start by presenting Hookway's reading of Peirce's position in the first and fourth of the *Cambridge conferences lectures*. While I think that this reading has considerable merits, I will identify a problem that it cannot solve. Subsequently, I will present my own interpretation of Peirce's claims. I will focus on the "no belief in science" and "no science in vital matters" theses. My purpose will be to provide a reading of them that is coherent with "fixation" and can make sense of the distinction between two forms of holding for true.

2. Hookway's solution

account when we try to understand the historical genesis of the text. Simply, they should not be used to argue that Peirce's problematic claims in the lectures do not express his considerate position at that time.

[5] With respect to this forth task, let me make a methodological clarification. I do not want to deny the historical dimension of Peirce's thought. Clearly, his views evolved dramatically between "fixation" and the *Cambridge conferences lectures*. However, he does not explicitly mark a radical break with the first text when he makes the problematic claims that are here under discussion. For this reason, I believe it is a simple principle of charity that suggests that we must try to soften the apparent conflict with "fixation." Naturally, this attempt might fail, but we should try nonetheless to provide an account of Peirce's view that is sensitive to the way in which he himself portrays it.

[6] Proposals that have taken Peirce's claims seriously are for example those of Migotti 2005 and, more recently, Gaultier 2016 and Atkins 2016: ch. 1.

[7] Both Migotti (2005: 44-5) and Atkins (2016: 2, 7) accuse Hookway of diminishing the relevance of Peirce's claims in "Philosophy and the conduct of life." I believe that their comments are unfair to Hookway's attempt to make sense of Peirce's position.

The key move in Hookway's interpretation of Peirce's claims in "Philosophy and the conduct of life" and "The first rule of logic" is to interpret Peirce's talk of belief as referring primarily to "full" belief. Full belief is the kind of belief that can guide our actions. More importantly, what characterizes a state of full belief is that we do not have complete control over the processes that led us to the state in question. Accordingly, Hookway writes: "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" (Hookway 2000: 30).

By referring to "full belief" in this way, Hookway is able, first, to make sense of the distinction between two forms of holding for true. Obviously, one form of holding for true is full belief, understood along these lines. But what is the kind of holding for true that Peirce thinks is appropriate in science? With respect to the latter, Hookway reads Peirce as simply submitting that the form of assent that is rational in science is one that must be the result of a rational process of deliberation. Given that full belief is a state over which we do not have complete control, the kind of assent that would be rational to adopt in science cannot be *full* belief. Second, this approach allows Hookway to provide a plausible reading of the "no belief in science" thesis. Peirce's claim does not mean that, within science, we cannot form an assent toward a proposition that, in a broad sense, could be called a belief. The point is that the form of assent we should adopt is one in which "the grip of the causal processes" (Hookway 2000: 31) that have contributed to our assent is not strong enough to prevent the withdrawing of our assent when this is rationally required. Third, Hookway can also account for the "no science in vital matters" thesis. The latter does not submit that we should not rely on scientific investigation in our practical deliberation. Rather, the idea is the following: since what guides our deliberation in practical matters is *full* belief, and since full belief always results at least in part from causal processes that are not in our control, practical deliberation cannot ever rest *solely* on scientific rationality. In this sense, the "no science in vital matters" thesis simply means that it would be "self-delusional" to think that processes of deliberation in practical matters could be entirely rational and in our complete control (Hookway 2000: 30).

But what about the conflict with "fixation"? We have seen that the kind of attitude Peirce recommends for "vital matters" resembles the a priori method of "fixation." In "fixation," however, Peirce suggests that the method of science is to be preferred to the a priori method. Worse, Peirce seems to believe that we are "naturally" led toward adopting the scientific method, which implies that the a priori method, similar to the methods of tenacity and authority, is not one that we can hold

in the long run. While this is a narrative that is certainly present in "fixation," Hookway points out that Peirce premises his investigation with remarks that do not fit well into this picture. In fact, at the beginning of "fixation," Peirce stresses that he will analyze different methods of fixing beliefs from the perspective of the guiding principles that "are necessarily taken for granted in asking whether a certain conclusion follows from certain premises" (W3: 246). Hookway notes that, if we read Peirce's preference for the method of science as based on these guiding principles, his argument would be not that the method of science is the best method for fixing belief in any context, but rather that the method of science is the best method for fixing belief once we want our procedures of belief formation to be subject to rational control (Hookway 2000: 36-7). This claim would be compatible with stressing that, in some contexts, a method of inquiry that is not governed by rational self-control could be preferrable, where this softens the conflict between "fixation" and the *Cambridge conferences lectures*.

As it is clear from this reconstruction, Hookway's solution to the challenges posed by Peirce's claims in "Philosophy and the conduct of life" and "The first rule of logic" is elegant. Moreover, it addresses all four tasks we identified in the Introduction. Yet, his interpretation of the "no science in vital matters" thesis fails to capture an important normative dimension of Peirce's claim. As we saw, Hookway interprets the thesis as simply stressing that it would be self-delusional to think that our choices in practical deliberation could be completely rational and, in this sense, the result of a "scientific" approach. Therefore, if we wanted to spell out the prescription in the "no science in vital matters" thesis, it would read "do note deceive yourself in thinking that your practical choices can be guided by a purely scientific mind." It would not read "do not rely on the method of science in practical choices." However, if we consider some remarks Peirce makes on practical matters that are not vital, it seems that the prescription in vital cases cannot simply be the one identified by Hookway. For Peirce seems to think that the "no science" part of the thesis does not hold for practical choices that are not vital. For example, he writes that "[i]n everyday business, reasoning is tolerably successful" (RLT, 109), or that "[t]heory is applicable to minor practical affairs" (RLT, 112). This means that we *can* follow the method of science in practical affairs that are unimportant and where the stakes are low. But if Peirce's "no science" thesis really meant that we would be self-delusional if we thought that deliberation in practical affairs could be purely rational and scientific, this would equally apply to deliberation regarding unimportant practical matters. Given that in these latter cases the "no science" part of the thesis does not hold, it seems implausible that the thesis is only an appeal not to deceive ourselves.

The latter problem notwithstanding, Hookway's proposal is still significant, at least because it clearly shows what is at stake if we want to offer a successful interpretation of Peirce's claims in the *Cambridge conferences lectures*. In the remainder of this chapter, I will offer an interpretation of Peirce's claims that avoids the problem of Hookway's account and is able to meet the four tasks identified above. Accordingly, I will provide a rationale for the "no science in vital matters" and "no belief in science" theses. In discussing the latter, I will propose a viable way for distinguishing two kinds of holding for true. Finally, I will suggest how the conflict with "fixation" can be softened.

3. No science in vital matters

If we want to avoid the problem that we have identified in Hookway's account, our interpretation must be able to explain why the "no science" part of the "no science in vital matters" thesis holds for vital crises but not for unimportant practical affairs. Of course, it must also make the thesis intuitively acceptable.

Let me begin by considering what the thesis exactly asserts. One thing that needs clarification is what "vital matters" or "vital crises" are. First, as these labels suggest, Peirce has in mind practical situations with high stakes, that is, situations in which acting wrongly or on wrong beliefs might have high costs for us. Second, talk of "crises" (RLT, 109, 111, 112, 113) suggests that the relevant cases are marked by uncertainty. We must have reasons to ponder on which belief it would be correct to act, otherwise it would be futile to ask whether we should rely on instinct or science in order to determine it. Here, one might object that one of Peirce's examples of beliefs that are better left to instinct is our belief on the impermissibility of incest (RLT, 111). Clearly, this belief is not marked by uncertainty. Rather, it points towards a resolute and immediate answer regarding what is "wrong". However, consider how Peirce construes the example. He says that the man who would change his code of morals "at the dictate of a philosophy of ethics" so as to allow incest would be "*unwise*" (RLT, 111). Therefore, the example does present a case in which we must choose whether incest is permissible or not and where instinct and reason give contrasting indications. Third, Peirce emphasizes that in vital matters "[w]e must act" (RLT, 112). Therefore, there is a time constraint over our action such that we cannot delay decision regarding the belief that should guide it. In vital crises, withholding judgment is not an option.

Let me now turn to the "no science" part of the thesis. We have seen that the main worry regarding it is that it appears to imply that, in vital crises, we should not rely on well-established scientific assumptions. I submit that there is a way of understanding the "no science" indication that does not

have this consequence. For we can read Peirce as stressing that, *when we must decide on which belief we should act and there is no straightforward answer regarding this decision*, it would be unwise to try to settle the issue by beginning *a process of inquiry* carried out using a scientific approach. This way of reading the indication squares well with what I have said on "vital crisis." If the latter are marked by uncertainty, it seems reasonable to think that the relevant cases are not situations where science would already possess a well-established answer to our problem. Rather, the "crisis" arises when the issue regarding what to believe and how to act is open, and we must decide how to settle it. Accordingly, one of Peirce's examples of applying the scientific approach to vital matters is one where twelve men deliberate on an *unsettled* issue by appealing only to "reason" (see RLT, 110).[8]

To sum up, we can spell out the "no science in vital matters" thesis as follows: do not begin a process of inquiry that follows the scientific method when you are in a practical situation that (1) is characterized by high stakes, (2) is marked by uncertainty concerning the belief on which you should act, and (3) requires you to decide how to act in a short time.

Now that we have a clearer understanding of the thesis, we can try to make sense of it in a way that also explains why the "no science" part applies to vital crises but not to unimportant practical affairs. In order to do that, I would like to point out another puzzling aspect of Peirce's position in the *Cambridge conferences lectures*. For Peirce, on the one hand, sharply separates "instinct" and "sentiment" form "reason" and "science," but, on the other, maintains that instinct and reason follow similar paths when it comes to correcting one's views and dispositions. Once we understand how these seemingly conflicting claims can coherently go together, we can unpack why it is only in "vital crises" that the "no science" indication holds.

Let us see in which sense instinct and reason correct themselves along similar lines. Peirce writes:

> Instinct is capable of development and growth, – though by a movement which is slow in the proportion in which it is vital; and this development takes place upon lines which are altogether parallel to those of reasoning. And just as reasoning springs from experience, so the development of sentiment arises from the soul's Inward and Outward Experiences. <such as meditation, on the one

[8] Moreover, it would be misguided to draw a sharp line between "established scientific assumptions" and "common sense beliefs." After all, when an established scientific assumption stays such for a long time, it has a tendency to become part of our common sense beliefs. When we do rely on the latter beliefs for deciding how to act, it seems appropriate to say that we both follow our "sentiment" and make use of scientific assumptions.

> hand, and adversity on the other> Not only is it of the same nature as the development of cognition; but it chiefly takes place through the instrumentality of cognition (RLT, 121).

There are at least two things that I wish to emphasize in this passage. First, Peirce clearly stresses that instinct and reasoning develop along similar lines. Peirce does not explicitly identify the feature of reasoning which is in continuity with instinct. However, he points out that this feature has to do with the role played by experience. Because the chief role of experience in reasoning is to provide a "*force majeure*" (RLT, 170) that compels us to correct errors and false conclusions, it is plausible to assume that instinct and reasoning have similar ways of correcting themselves in reaction to experience. Second, Peirce marks a specific feature of the way in which instinct corrects itself: it does it *slowly*. Peirce will later develop this point in the context of his "critical common-sensism." Famously, Peirce identifies various aspects that distinguish his doctrine from the views on common sense defended by Thomas Reid and the "Scottish school." According to one of these aspects, instinctive beliefs are not unalterable but can change and evolve over time. However, Peirce observes that these changes take place unnoticeably and over a long period of time (see EP 2: 349).

Peirce not only thinks that instinctive beliefs evolve slowly. He also believes that the instinctive beliefs that we have are the result of very long processes of evolution. For example, he suggests that "[t]he regnant system of sexual rules is an instinctive or Sentimental induction summarizing the experience of all our race" (RLT, 111). This relates to a claim Peirce often makes in the *Cambridge conferences lectures*. He maintains that instinct is very reliable when we must decide what to believe in the short run. After all, instinctive beliefs have already gone through a very long process of development and correction. This process bestows at least *some* reliability to those beliefs, especially in the short run, given that in this case any attempt at fixing our belief in a way different from trusting our instinct could *not* equally be supported by a very long process of development.

While instinct is reliable for deciding what to believe in the short run, reason is reliable in the long run. When the aim of our believing or assenting is to attain the truth, *notwithstanding how long this will take*, the method of science is our best bet. There is a simple reason for this. While it is true that both instinct and reason can correct themselves, instinct can do so only *up to a certain degree*. Peirce's later critical common-sensism is here helpful again, for it clearly states a point that is also made in the *Cambridge conferences lectures*. The point that I have in mind is Peirce's contention that instinctive beliefs are "invariably vague" (EP2:350). In "Philosophy and the conduct of life," Peirce notices that "human instincts are not so detailed and featured as those of the dumb animals" (RLT, 112). In other words, for Peirce it is an essential feature of instinctive beliefs that they are

vague and can gain determination only up to a certain degree. But if, through our believing, we want to attain not simply truths, but *informative* truths, the process of correction through which our beliefs evolve must be able to proceed in a way that make those beliefs less vague and more determinate. It is here that reason, or the method of science, has a clear advantage over sentiment or instinct. However, the method of science only works *in the long run*. To work at its best, there must be *no time constraint* that impacts on our inquiry. Accordingly, in "The first rule of logic," Peirce writes that science "has nothing at stake on any temporal venture, but is in pursuit of eternal verities, not semblances to truth, and looks upon this pursuit, not as the work of one man's life, but as that of a generation after generation, indefinitely" (RLT, 177).

It is now time to take stock and see where focusing on commonalities and differences between instinct and reason has brought us. First of all, we can now make better sense of the "no science in vital matters" thesis. This is not an appeal to irrationality. On the contrary, we have *reasons* to trust instinct in vital decisions that are marked by uncertainty. Instinctive beliefs are the result of a long process of development and correction. It is this process that justifies our trust in instinct. Furthermore, we can now explain why the "no science" part of the thesis applies to vital crises but not to unimportant practical affairs. As we saw, a central feature of vital crises is that they need a decision on how to act in a short time. However, reason, or the method of science, works at its best when there is no time constraint over our inquiry, and certainly *not* in the short run. This clarifies why, in vital crises, it is "unwise" to appeal to reason or the method of science to decide what to believe.

But what about unimportant practical affairs? Why is it possible to rely on scientific reasoning in this context? One might point out that there is *always* a time constraint in practical affairs. Since these affairs concern how we should act, our inquiry into the belief that should guide our action cannot proceed indefinitely. However, that practical affairs are temporally constrained does not mean that in every practical matter we should decide what to believe in the short run. There certainly are practical matters in which there is enough time for a process of inquiry to settle an issue with at least a decent degree of reliability (even though, of course, that process of inquiry would not be carried out in ideal conditions). Moreover, because the practical affairs that are at issue are unimportant, we are not risking great costs if our decision regarding what to believe turns out to be wrong. In this sense, it is permissible to rely on reason and scientific reasoning in unimportant practical affairs. Let us now turn to the "no belief in science" thesis.

4. No belief in science

Central to Peirce's "no belief in science" thesis is his distinction between two kinds of holding for true. Since Peirce tells us that belief is inadequate in a scientific context, it appears that he needs to tell us what sort of assent is suitable instead. However, pinning down what the fundamental characteristics of scientific assent are has proven difficult, if not impossible. I will suggest that this is not necessarily a problem. Perhaps, Peirce's main point is simply that belief is *irrelevant* in science. In this sense, it must be possible to assent in a way different from believing, but we do not need to positively describe what this assent is.

As we saw, Hookway reads Peirce's "no belief in science" thesis as insisting that *full* belief is inappropriate in science. Yet, this does not say anything *positive* on scientific assent. In this respect, Hookway remarks that assent in scientific inquiry should take the form of opinions: "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) belief will not be strong enough to inhibit the further operations of rational self-control" (Hookway 2000: 31-2). Therefore, Hookway characterizes scientific assent in contrast to full belief by saying that the former is *weaker* than the latter. But this seems inadequate to differentiate between two *kinds* of holding for true.

Responding to this way of describing the difference between belief and scientific assent, Mark Migotti has argued that the *provisionality* of scientific assent should not be interpreted in terms of a difference of *strength* with belief. Migotti writes: "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…and the practical manners or modes or contexts of holding for true" (Migotti 2005: 49). What defines practical holding for true is the willingness to act on it. What defines theoretical holding for true is provisionality, where provisionality does not entail weakness. According to Migotti, examples of scientific assents that are "strong" but nonetheless provisional are those concerning propositions like: "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" (Migotti 2005: 50). While this proposal is appealing, it has been noted that it creates difficulties when we try to describe the state of mind of a scientist that, as a matter of fact, fully believes that "The atomic number of gold is 79," while she, as a scientist, should also take her assent to be provisional as Migotti suggests. Migotti sees this difficulty and cites Peirce who stresses that a scientist "ought to be in a double state of mind" (R 175; see Migotti 2005: 46). Notwithstanding Peirce's claim, it is difficult to understand what being in this double state of mind could mean (for this criticism see Gaultier 2016: 396-7).

What I want to do now is to try to make sense of Peirce's "no belief in science" thesis in a way that does not require us to positively specify what scientific assent is. I think that this is possible if we depict Peirce's strategy for arriving at the thesis as a two-step strategy, that is, one that does not simply rest on specifying the kind of assent that is appropriate in science, where this is taken to entail the "no belief" claim. The strategy that I have in mind starts by assuming Peirce's definition of belief as an attitude in which we are ready to act on the basis of the believed proposition independently of how high the stakes are (see RLT 112, 178). Assuming this definition of belief at the outset, the first step consists of arguing that belief defined in this way is *irrelevant* in scientific inquiry. This means that we can picture a course of scientific research where the scientists carrying out that research are not committed to the propositions that are taken for granted in it in a way that would involve belief in Peirce's sense of the term. In fact, we might even think that they *personally* believe some of those propositions to be ultimately false, but as long as these propositions are part of the shared background in their field, for the sake of participating to the collective investigation that rest on those propositions and is directed toward a different issue, they "accept" these propositions. I think it is possible to picture this way of "accepting" a proposition without having to "believe" in it, in Peirce's sense of the term. We might even think that this involves "assenting" to the proposition in a certain sense. However, we do not need to specify further what this sort of "assenting" implies for our psychology. We simply need to render plausible the idea that scientific inquiry can proceed through a form of assent that does not involve belief as defined by Peirce.

Now the second step. Clearly, the first step is insufficient to warrant the "no belief in science" thesis. It simply concludes that scientists may believe in scientific propositions or not. How do we arrive at the normative claim that we should *avoid* belief in science? One way to arrive at the claim can build on Peirce's characterization of belief. After all, belief implies a strong commitment to a proposition such that we are willing to act on it even when the stakes are high. It is plausible to think that at least in some cases, this strong commitment is paired with a psychological disposition to hold fast to the belief notwithstanding evidence for its falsity – note that I am not saying that this strong commitment *necessarily* requires this disposition.[9] If we can assume this, we can arrive at the "no belief in science" thesis with the following thought: since belief is irrelevant in science, and since it involves the risk of "blocking the way of inquiry" when it is paired with the disposition I have just mentioned, in science, it is generally advisable to avoid beliefs.

[9] It must be admitted, though, that Peirce sometimes suggests that this necessary connection holds.

Arriving at the "no belief in science" through this strategy has two advantages. First, as I have already remarked, we do not need to positively describe what scientific assent is. We simply need to maintain that within science it must be possible to "assent" in a way that is different from believing. Second, while we have a rationale for explaining the "no belief" part of the thesis, this does not in fact preclude conceding that there could be circumstances in which believing is permissible in science. Recall that the "no belief" claim rests on the risk that our believing is paired with a disposition to hold fast to a belief notwithstanding contrary evidence. If, as a matter of fact, the scientist that *does* believe a certain scientific proposition does not have this disposition, it would be nonsense to insist that the "no belief in science" thesis should hold for her, too.

5. The conflict with "fixation"

In the previous two sections, I sketched a reading of the "no science in vital matters" and the "no belief in science" theses with the purpose of making them more acceptable. Moreover, I have proposed an understanding of Peirce's distinction between two forms of holding for true that does not require us to positively describe what scientific assent is. In this section, I will discuss the last problematic feature of Peirce's claims that we have identified in Section 1. As we saw, Peirce's claims in the *Cambridge conferences lectures* appear to conflict with Peirce's position in "The fixation of belief." More specifically, the "no belief in science" thesis seems to be in direct opposition with the idea that the "method of science" is a method for fixing belief. Additionally, the contention that in vital matters we must trust our instincts for determining the belief that should guide our action is in contrast with the idea that the method of science is to be preferred to the a priori method. Is there a way in which we can soften the conflict between these claims?

Let me begin with the first problem. Is the "no belief in science" thesis somehow compatible with the idea that we can use the method of science for fixing belief? I believe it is. The point of the "no belief in science" thesis is that the purpose of science is not to attain a status of belief, understood as an action-directed kind of holding for true. Rather, the purpose of science is to attain the truth with no concern for its practical value (RLT, 177). But the fact that science does have this aim is not in conflict with the idea that procedures of investigation that are typical of science can sometimes be used for other purposes, such as for example fixing belief. Peirce is clear on this point:

> After a while, as Science progresses, it comes upon more solid ground. It is now entitled to 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. This reflection, however, is quite aside from the purpose of science. It does not modify its procedure in the least degree. It is extra-scientific. For Practice, however, it is

> vitally important, quite altering the situation. As practice apprehends it, the conclusion no longer rests upon mere retroduction, it is inductively supported. (RLT, 177)[10]

In the quote, Peirce clearly says that procedures of investigation that are typical of science can sometimes be the basis for a process of reflection that leads us to belief. Peirce's point is that, when we attain that status, we leave the domain of science and enter that of practice. Evidently, this is compatible with maintaining that we can follow the method of science for determining what to believe and how to act accordingly. When we do that, we are not properly doing "science," but this does not mean that we do not follow procedures that are scientific until we turn the results of our investigation into beliefs. Moreover, as we saw in Section 3, Peirce concedes that it is sometimes permissible and rational to follow "reason" or the method of science when we want to determine the belief that should guide our actions. This is not different from maintaining that the method of science is a candidate method for fixing belief. So, it seems, at least this conflict with "fixation" dissolves.

Let us now turn to the second problem. According to a traditional reading of "The fixation of belief," the purpose of Peirce's article is to show that the method of science is preferrable over the method of tenacity, the method of authority and the a priori method. Its "advantage" over the other methods consists in its greater capacity to deliver *stable* beliefs. After all, it is the only method that rests on hypothesizing an external point of reference for adjusting our beliefs, that is, reality. This conflicts with the idea that in vital crises we should follow instinct for determining what to believe. An appeal to instinct or sentiment resembles Peirce's description of the a priori method in "fixation," given that the latter is described as a procedure in which we adopt a belief because we feel inclined toward it (see W3:252). In other words, it seems that sometimes it is rational to determine what to believe by following the a priori method, in contrast to what Peirce suggests in "fixation." We already saw how Hookway attempted to solve this conflict. He proposed an alternative reading of "fixation," one in which its purpose would not be to determine the best procedure for fixing belief in any circumstance. Rather, its purpose would be to determine the best procedure for fixing belief *given the guiding principles of rational inquiry*. Submitting that the method of science is the best procedure *once we have accepted those principle*s sounds reasonable. More importantly, it leaves conceptual space for the possibility that sometimes we could be better off if we ignored them in determining what to believe.

[10] In the quote, Peirce suggests that inductive processes of confirmation are not part of science proper. This is a puzzling claim that I will not try to render plausible. In his reading of the "no belief in science" thesis, Gaultier (2016) insists exactly on this purely "abductive" account of scientific inquiry.

I find Hookway's reading of "fixation" defensible. What I want to do now is to suggest that there is a way to soften the conflict between "fixation" and the *Cambridge conferences lectures* even if we do not abandon the traditional reading of the former.[11] Suppose that we accept that the result of "fixation" is indeed that the method of science is preferrable over the other methods of fixing beliefs because of its greater capacity to deliver stable beliefs. We might however ask under which conditions that capacity can be warranted. Clearly, even within the conceptual framework of "fixation," the answer to this question is: "in the long run." The hypothesis of reality which guides the method of science can provide a reference point for adjusting our beliefs (and so for making them more stable), only if we assume a gradual process of approximation that takes time.

Additionally, the framework within which Peirce evaluates methods of fixing belief in "fixation" considers them as they evolved in a considerable amount of time. Peirce appears almost to provide a "history" of these methods, which treats the appearance of one method as resting on the unsuccessfulness of the others. Take the discussions of the method of authority and the a priori method. Peirce provides some historical examples of their use. He writes that, in Rome, the method of authority "has been practised from the days of Numa Pompilius to those of Pius Nonus" (W3:251), or that the most perfect example of the a priori method "is to be found in the history of metaphysical philosophy" (W3:252). At least part of Peirce's argument rests on showing that these historical attempts to fix beliefs using the method of authority and the a priori method were unsuccessful in delivering stable beliefs. If this is true, Peirce's question regarding "what method is more adequate to deliver stable beliefs" must be further specified as the question regarding "what method is more adequate to deliver stable beliefs *that would continue to be held through history*."

Here, one might object that Peirce's position in the *Cambridge conferences lectures* implies that instinct and sentiment can deliver stable beliefs, too. That is certainly true. However, it is important to emphasize that instinct and sentiment can deliver stable belief *only because* they have gone through a process of self-correction that resembles the procedures of science except for the fact that it is not under our conscious control. Therefore, first, if we link these claims on instinct to the a priori method of "fixation," the latter could deliver stable beliefs only insofar as it has something in common with the method of science. Moreover, second, Peirce's description of the a priori method in "fixation" does not assume this previous process of self-correction of our instinctive beliefs. As a consequence, in this respect, it is misleading to assume that Peirce's discussion of instinct in the

[11] My purpose is not to maintain that one reading is preferrable over the other. Rather, I simply want to show that the conflict can be softened even if we stay closer to a traditional approach to "fixation."

Cambridge conferences lectures perfectly matches his account of the a priori method in "fixation."[12]

What consequences do these remarks have for the relationship between "fixation" and the *Cambridge conferences lectures*? They suggest that these texts take very different perspective on the question "how should we fix our belief?" "Fixation" asks this question with a particular aim in view: securing a method that delivers stable beliefs *in the long run*. By contrasts, when Peirce claims that we should follow our instinct or sentiment in vital crises, he has in mind situations in which we should decide what to believe *in the short run*. To put the point more directly, the question of the *stability* of belief does not seem to play any role in the "no science in vital matters" thesis. Rather, the question is how we should determine our beliefs (in the short run) in a way that minimizes the risk of great losses. Therefore, it seems there should be no (major) conflict between "fixation" and the *Cambridge conferences lectures* on this point, too.

6. Conclusion

In this paper, I have tried to make sense of some puzzling claims that Peirce makes in the *Cambridge conferences lectures*. I have identified four tasks that a successful interpretation of those claims must accomplish. First, we must provide a plausible reading of the "no belief in science" thesis. Second, we must provide a compelling interpretation of the "no science in vital matters" thesis. Third, we must explain Peirce's distinction between two forms of holding for true. Fourth, we should be able to solve or to soften the conflict with "The fixation of belief." I have analysed Hookway's reading of Peirce's claims in the *Cambridge conferences lectures* because he clearly identifies these tasks and offers an interpretation with considerable merits. However, I have also pointed toward a problem in Hookway's reading, since the latter is not able to account for a normative dimension of the "no science in vital matters" thesis.

Subsequently, I have provided my interpretation of Peirce's claims. In my reading of the "no science in vital matters," I have first shown that the thesis does not imply that we cannot rely on established scientific truth in practical deliberation. Rather, the thesis can be spelled out as follows: do not begin a process of inquiry that follows the scientific method when you are in a practical situation that (1) is characterized by high stakes, (2) is marked by uncertainty concerning the belief on which you should act, and (3) requires you to decide how to act in a short time. Moreover, I have explained why the "no science" part of the thesis holds for vital crises but not for unimportant

[12] Indeed, on this particular issue, Peirce's position seems to have significantly evolved.

practical affairs. My solution rested on the fact that both instinct and reason are capable of self-correction, while instinct works best *in the short run* and reason *in the long run*.

Now turning to the "no belief in science" thesis, I have claimed that Peirce's distinction between two forms of holding for true does not need to positively describe what scientific assent is. Rather, Peirce's claim is first of all that belief is *irrelevant* in science. Since it is irrelevant, within science, it must be possible to assent in a way different from believing. Additionally, the "no belief in science" thesis can be read as stressing that it is better to avoid belief in scientific inquiry because, when believing, our commitment can be paired with a disposition to hold fast to a belief even when contrary evidence emerges. However, this is compatible with permitting belief when scientists do not have this disposition.

Finally, I have shown that the conflict with "fixation" can be softened. First, Peirce's claim that the purpose of science is to reach truth and not to attain belief is compatible with the idea that scientific procedures can be used to fix beliefs. Additionally, the fact that "fixation" treats the a priori method as inferior to the method of science is not necessarily in conflict with the perspective of the *Cambridge conferences lectures* since these texts display very different viewpoints on the question "how should we fix our beliefs?"[13]

References

Atkins, R. K. 2016. *Peirce and the Conduct of Life: Sentiment and Instinct in Ethics and Religion*. Cambridge: Cambridge University Press.

Boyd, K. and D. Heney 2017. "Peirce on Intuition, Instinct, & Common Sense." *European Journal of Pragmatism and American Philosophy* 9. URL: http://journals.openedition.org/ejpap/1035; DOI: https://doi.org/10.4000/ejpap.1035

Gaultier, B. 2016. "On Peirce's Claim that Belief Should Be Banished from Science*." Transactions of the Charles S. Peirce Society* 52: 390-415.

Hookway, C. 2000. *Truth, Rationality and Pragmatism*. Oxford: Clarendon Press.

Maddalena, G. 2003. *Istinto razionale: Scritti sulla semiotica dell'ultimo Peirce*. Turin: Trauben.

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Migotti, M. 2005. “The Key to Peirce’s View of the Role of Belief in Scientific Inquiry.” *Cognitio* 6: 43-55.

Misak, C. 2004. “C. S. Peirce on Vital Matters.” In *The Cambridge Companion to Peirce*, edited by C. Misak, 150-74. Cambridge: Cambridge University Press.

Peirce, C. S. 1982-. *Writings of Charles S. Peirce: A Chronological Edition*, edited by the Peirce Edition Project, 7 vols. so far. Bloomington: Indiana University Press.

Peirce, C. S. 1992. *Reasoning and the Logic of Things*, edited by K. L. Ketner. Cambridge: Harvard University Press.

Peirce, C. S. 1992-1998. *The Essential Peirce*, edited by the Peirce Edition Project, 2 vols. Bloomington: Indiana University Press.

Robin, R. S. 1967. *Annotated Catalogue of the Papers of Charles S. Peirce*. Amherst: University of Massachusetts Press.

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