Five pragmatist insights on scientific expertise

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Abstract: A common objection to a pragmatist perspective on scientific expertise is that, while there is a well-known pragmatist theory of inquiry, which was formulated first by Peirce, then refined by Dewey and others, this theory cannot provide a clear-cut account of scientific expertise. In this paper, after addressing this objection in the second section, I claim that, on the contrary, pragmatism offers robust tools to think scientific expertise. In Sections 3 to 7, I present five important insights that one can derive from a pragmatist epistemology when responding to contemporary problems posed by expertise: about science and scientific expertise in a legal context (sections 3 and 4), about collective expertise (sections 5 and 6), and even about expertise on ignorance (section 7).

Keywords: pragmatism; scientific expertise; skepticism; criteria; ignorance

1. *Introduction*

There is no real treatise on scientific expertise in classical pragmatism.[1] There is a pragmatist theory of inquiry, which was formulated first by Peirce, then refined by Dewey and others, but this theory does not seem to directly provide a clear-cut account of expertise.

A first – and to my mind superficial – explanation for this absence would be that this pragmatist account of inquiry is at odds with important features of expertise. One can mention at least five reasons for this: *(1)* expertise answers the need for a reliable answer, *(2)* in the short term, with *(3)* a reasonable degree of certainty; *(4)* it is given by identified individuals or groups, *(5)* on the basis of an accepted method. The Peircean account of inquiry, as developed in his 1870s papers and refined over the decades, is not meant as an account of reliability;

[1] There are papers on pragmatism and expertise though, see for example Beck 2015, or, about pragmatism, expertise and democracy, Brown 2009.

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it focuses on the long run rather than on short spans of time; it presupposes an unlimited community of inquirers rather than specific individuals or groups; it does not even spell out a particular method. On *(1-3)*, Peirce states clearly that science never allows "full belief", the kind of belief we need in action. It would thus be tempting to think that science, in his view, does not provide the kind of certainty we need in court, or in other matters, where we must choose an immediate line of conduct.[2] If science is in "pursuit of eternal verities" over the course of generations, its rhythm seems to be at odds with the timeframe of expertise. Inquiry never stops, we can only aim at the "final object" of inquiry, where all inquirers, starting from very different points, will eventually converge. On *(4)*, Peirce, at least from the 1860s, claimed that the real subject of inquiry – the inquirer – is not limited to a particular, historical, community: everyone who is able to understand the questions at hand and to contribute in a significant manner is part of the inquiry.[3] As for *(5)*, assessing scientific expertise seems to presuppose an account of what makes this expertise scientific, and Peirce, perhaps the most advanced of the pragmatists in mathematical and natural sciences, as well as in philosophy of science, always refused to identify science with one single method.[4] I shall address this objection in the next section, and show why the opposition mentioned at the beginning is superficial. However, I think that focusing on this point would lead us to overlook a more important fact: pragmatism offers robust tools to think scientific expertise. Accordingly, in Sections 3 to 7, I shall present five important insights that can be derived from pragmatist epistemology when responding to contemporary problems posed by expertise.

To present these insights, I shall build on four major pragmatist claims:[5] their common anti-skepticism, their approach to the elucidation of abstract

[2] "In other words, there is no reason to believe in the theory, for belief is the willingness to risk a great deal upon a proposition. 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 to truth) and looks upon this pursuit, not as the work of one man's life, but as that of generation after generation, indefinitely". (Peirce 1960: 5.589).

[3] "Thus, the very origin of the conception of reality shows that this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of an indefinite increase of knowledge" (Peirce 1960: 5.311).

[4] "That which constitutes science, then, is not so much correct conclusions, as it is a correct method. But the method of science is itself a scientific result. It did not spring out of the brain of a beginner: it was a historic attainment and a scientific achievement. So that not even this method ought to be regarded as essential to the beginnings of science. That which is essential, however, is the scientific spirit, which is determined not to rest satisfied with existing opinions, but to press on to the real truth of nature. To science once enthroned in this sense, among any people, science in every other sense is heir apparent" (Peirce 1960: 6.428).

[5] These claims are discussed in more detail in Girel 2017a.

meanings, their attention to the "publics" and to the social nature of mind in general, and finally their understanding of knowledge as a capacity that can be fostered or impaired. Despite the movement's remarkable inner diversity, the pragmatist nature of these claims is not controversial. Pragmatists all share a staunch anti-skepticism: Peirce derided Cartesian "paper doubts", James tried to find cures to speculative melancholy and skepticism in his *Will to Believe*, Dewey wrote *The Quest for Certainty* to show that the quest for – absolute, theoretical – certainty was an ill-advised strategy to counter practical uncertainty. Skepticism, in their analyses, was in most cases the result of a misguided way of understanding science. They felt that one would certainly end up facing skepticism if one adopted epistemic goals that were too unrealistic, or an unreliable method, or if there were too many obstacles in one's way after adopting a given method to achieve a given goal.[6] Their fallibilist account of science was precisely an answer to this risk, as was the fact that they focused on the practice of science, as opposed to its idealizations. Secondly, they all have their own distinctive version of Peirce's maxim, urging us to pay attention to "practical bearings" in order to elucidate abstract terms. Thirdly, the social nature of mind can take several forms in the movement, from an emphasis on the semiotic community in Peirce to a full-blown theory of the publics in Dewey. Finally, Peirce, James and Dewey all thought that our beliefs were organically connected to our conduct, which led them to view knowledge as a capacity rather than as a mere state.

The intuition developed in Sections 3 and 4 is that skeptical risks can occur not only in our individual epistemic lives, but can also be caused by the definitions of science adopted by institutions, in particular when they regulate scientific expertise, and that a pragmatist account of abstract terms is better fitted to detect those risks and to offer countermeasures. Section 3 deals with general definitions of science in legislation, Section 4 with the criteria mentioned in the Daubert Framework regulating scientific expertise in the US, both at the federal level and in some states. In Sections 5 and 6, I address collective expertise: a "pragmatist" model of expertise can be a model where the public, in interaction with scientists and politicians, plays a prominent role (Section 5); it can also be a pragmatist way of looking at collective expertise in general, describing it in terms of collective actions and practical outputs (Section 6). Finally, there are situations where experts must testify not only about states of knowledge, but also about states of ignorance and, once again, apprehending knowledge as a capacity and beliefs as dispositions to act allows us to under-

[6] In his rereading of Descartes's first *Meditation*, Williams 2010 offers a nice reduction of "standard skepticism" along these three elements (goals, method, obstacles).

stand why this is possible (Section 7). The whole article can be read as a roadmap for the exploration of scientific expertise from a pragmatist standpoint.

2. *Inquiry and scientific expertise*

It would be misleading to think that one does not find precious resources to conceptualize expertise in Peirce. First of all, inquirers can provide reliable reports on the current state of knowledge: they do not need to wait for the "final opinion" to tell what science is capable of today. Thus, while it would be foolish to state right now what will be the eventual scientific verdict on dark matter, physicists can report reliably on present scientific achievements on the topic. When serving as experts, they can assess calls for projects on that topic, for example. Secondly, the crude opposition between the scientist *à la Peirce* and our naive view of the expert certainly tells more about our preconceptions of expertise than about expertise itself. Talisse made exactly this point when criticizing an abstract view of expertise as "elite" knowledge, far removed from the activity of research. The primary condition, in order to be an expert about extant knowledge, is to actually take part in a line of inquiry. Being an expert implies participating in research, and this participation is not understood by Peirce as "monolithic":

> …in the Peircean view, experts are not elites. That is, according to the Peircean, the status of being an expert is contingent on an ongoing engagement with inquiry; one is an expert insofar as one is continually engaged in the process of justification. Hence expertise is *not* a matter of standing above the processes of inquiry and simply issuing decrees and orders; no expert qua expert is entitled to deference. Rather, the expert is someone who must continually meet the challenges of issuing reasons, giving arguments, and meeting objections. That is, expertise is ultimately inseparable from inquiry, and, as we have seen, inquiry is an inherently inclusive process of exchanging reasons, arguments, and evidence. Yet these norms of inclusion do not require merely the removal of barriers to participation; they prescribe epistemic practice that actively *seeks out* and engages unfamiliar and unorthodox voices. concerns, and arguments. Were inquiry not constituted in part by norms of this active kind of epistemic inclusion, it could not perform its function of arriving at the best beliefs (Talisse 2013: 92).

Peirce actually served as a scientific expert early in his career, and this historical example seems to confirm that expertise, in his eyes, was then in no way disconnected from inquiry and the production of new knowledge.[7] This

[7] "Deposition of Charles S. Peirce, Wednesday, June 5, 1867" (Supreme Court 1868: 761-765).

example shows that, to him, providing expertise was not only a matter of stating an opinion, but also involved proposing a model and advancing science in a particular context. In 1867, a lawsuit caught the attention of all New Englanders: a wealthy heiress, Hetty Robinson, sued the executors of her aunt's will, producing a document allegedly bearing her aunt's signature. Since this document revoked the official will and granted Hetty Robinson almost all of the fortune bequeathed, in lieu of a much less advantageous distribution for the heiress, the whole question was to determine the validity of the new document, and in particular the authenticity of the signature. This trial, which has been remarkably documented,[8] opposed two approaches to evidence in the legal field, between classical empiricism and a new thinking imbued with the probabilistic spirit.

Each side had summoned academic witnesses. The defense had hired Oliver Wendell Holmes Sr and Louis Agassiz, two of the most respected scientists of the time. The former claimed not to see any "notable difference" between the inks of the two documents; the latter claimed to have checked the similarity of the two signatures under a microscope, and not to have observed any trace of lead, which could have indicated a pencil decal. These two strategies could be seen as belonging to classical empiricism: one is looking for a particular "impression", a single observation that will settle the case.

The prosecution had enlisted the services of Benjamin Peirce, then Professor of Mathematics at Harvard and Superintendent of the Coast Survey, assisted by his son Charles Peirce. Their own approach consisted in identifying the various downstrokes in the signature line, and they were able to observe from the outset that the thirty points they had singled out coincided *exactly* in two signatures: the will and another document. They then calculated the probability of this event occurring in general. They obtained forty-two signatures from the legatee and established, based on more than 25,000 comparisons, that there were only 5,000 cases of coincidence (*i.e.* cases in which one point corresponded to a similar point in another of the signatures). There was therefore one chance in five that a given point would be similar in two different signatures. This then enabled them – albeit based on the non-trivial, and objectionable, assumption that they were dealing with independent events – to state that the probability of producing two signatures with exactly these thirty points superposed was almost nil (corresponding to $\frac{1}{5}^{30}$).[9] The public saw this

[8] The main report is given by Fisch (Peirce 1982, 2: XXIII-XXIV) [hereafter W, followed by volume and page]; more context is provided by Menand 2001a and 2001b: 163 ff.

[9] "So vast an improbability is practically an impossibility. Such an evanescent shadow of probability cannot belong to actual life. They are unimaginably less than those least things which the law cares for" B. Peirce, quoted in Menand 2001b: 173.

as academic speculation, but it is one of the first scientific uses of probability in a lawsuit.[10]

Historians have raised criticisms about the calculation proposed by the Peirces (Meier and Zabell 1980), but the fact that their line of reasoning was sound has remained undisputed. If that is so, we can draw the following conclusions: this expertise involves an actual inquiry and fits all five criteria mentioned above: it provides a reliable answer (1), in the short run (2), with the best level of certainty available (3), by two experts, themselves members of specific epistemic communities (4), and relying on a statistical method, applied for the first time in court (5). Through this expertise, we have learnt something, for which they provide a model: that it is highly improbable to find two signatures that are exactly similar. Their model is connected to the last developments of science: the younger Peirce made a daily use of statistics in his work as a "pendulum swinger", and would shortly after apply the same resources to astronomical observations, the economy of research and the study of reaction time. Hacking sees here one of the first uses of statistics in court. This expertise also leads to reconsider the actual presuppositions of empiricism. The observable here is nothing without the mathematical apparatus required to evaluate it. What is supposed to determine the opinion of jurors is not an *isolated* fact (as Holmes and Agassiz seemed to believe) but a *relationship* between facts, in this case a relationship between favorable and unfavorable cases. Such proof was miles away from classical empiricism, as it involved a "relationalist" and probabilistic empiricism. The "conceivable practical effects" were not, even before Charles Peirce had written the first word of his pragmatism series, as rudimentary as a trace of pencil lead or the print of a pen tip.[11]

Let us assume at this point that a pragmatist theory of inquiry, such as the one developed by Peirce and his followers, not only accommodates a robust conception of expertise, but that history also shows that the practice of expertise coincides exactly with the first stages of Peirce's thought. Can we go a step further to show that a pragmatist view of expertise would not only, as a set of tools, better explain scientific expertise in court but also, as a substantive theory, be preferable to some other alternatives?

[10] In the quoted article, Menand rightly points out that DNA identifications are also based on probability (and that, in the case of the O.J. Simpson trial, the DNA evidence was of less weight than the famous glove) (Menand 2001a: 70).

[11] This was by no means Peirce's only experience in expertise, see for example W8: LXXXVI.

3. *Science in court: demarcation, skepticism and ambiguity*

Before turning to Daubert in the next section, let us see how institutions can actually endorse epistemological claims. Disputes about *what science is* have been frequent in court, one of the most famous examples certainly being the *Epperson vs Arkansas* trial[12] over the equal treatment of "Creation Science" in the classroom. As is well known, Judge Overton turned on that occasion to philosopher Michael Ruse for a series of demarcation criteria. The idea was to show that Creation Science did not meet these criteria, hence that it was no science at all but was in fact religion in disguise, so much so that it violated the Establishment clause[13] and should therefore not be allowed in public classrooms. There were five criteria:

> [Science] is guided by natural law; it has to be explanatory by reference to natural law; it is testable against the empirical world; its conclusions are tentative, *i.e.* are not necessarily the final word; and it is falsifiable (*McLean v. Arkansas, 529 F. Supp. 1255 (E.D Ark. 1982)*).

The first three criteria can notoriously be found in Hempel (in his deductive-nomological account of law and his logic of confirmation), the last two in Popper (fallibility, refutability), which is already a strange mix, since these epistemologies are at odds on many important points, starting with the role of confirmation and induction. I wished to mention this historical background before turning to Daubert, because it definitely presupposes a specific line of argumentation: *Consider a corpus C; Consider x criteria covering any scientific explanation (and nothing else); Does C fit all x criteria? If not, C is no science.*

In view of our mention of skepticism and pragmatism, such an approach is immediately open to skeptical challenges. The first danger is to uphold too dogmatic a view of science: the argument needs a substantive characterization of science *in general*, and the ensuing debate between Ruse and Laudan showed that the definition encapsulated in Overton's criteria certainly was controversial (Laudan 1982; Ruse 2009). As Laudan also remarked, even with goals such as keeping creationism out of the classroom in mind, it is certainly better to show that a set of claims, in this case creationist geology, has been *refuted*, or "debunked", than to immunize it by saying it is *irrefutable*. The danger, this time, is to spark off a second controversy over the importance and value of demarcation arguments and thus about the demarcation strategy itself.

[12] See Forrest and Gross 2004.

[13] Part of the *First Amendment*, stipulating that "Congress shall make no law respecting an establishment of religion, or prohibiting the free exercise thereof". Introducing a particular religious doctrine in the curriculum would amount to "establishing" it against other creeds or denominations.

Moreover, putting such a definition of science in a legal opinion sets a dangerous precedent: if one wants to change the science curriculum for religious or ideological purposes, a likely move will then be to try and change the very definition of science in the standards in order to circumvent the criteria, thus introducing skeptical challenges over science itself in the legislative process. This is exactly what happened with the *Intelligent Design* movement[14] in many states. Here is an example from Missouri in 2013, with a failed legislative attempt at redefining science:

> "Scientific theory": an inferred explanation of incompletely understood phenomena about the physical universe based on limited knowledge, whose components are data, logic, and faith-based philosophy. The inferred explanation may be proven, mostly proven, partially proven, unproven or false and may be based on data which is supportive, inconsistent, conflicting, incomplete, or inaccurate. The inferred explanation may be described as a scientific theoretical model *Missouri HB291 (2013)*.[15]

Surely no one would like to teach an Epistemology course on the basis of such a poor and misleading definition of science. One cannot grasp what these statements are doing by mere semantical or syntactical analysis: a minimal pragmatist reading of them – taking into consideration what these definitions will *do* – is necessary if we want to understand their practical import. If such a definition was accepted, it would immediately release the pressure on the teaching of ID in the classroom. It makes sense indeed: the demarcation criteria were not used in order to tell science from pseudoscience in general in the first place, they were a litmus test for compliance with the Establishment clause; the challenge does not try to give a better description of science, it seeks to elude the grip of Overton's criteria. To account for what exactly is happening here, the Popperian or Hempelian "surface" of the criteria will not suffice: one needs a richer pragmatist perspective. A merely formal approach to science will be blind to such a problem; a pragmatist approach, considering the "practical bearings" of the adoption of a definition, will be better suited.

[14] For example, in the 2005 Dover trial, most of Steve Fuller's expert report in favor of ID attacked the idea of demarcation as outdated. See in particular: "ID is a legitimate scientific inquiry that does not constitute 'religion' in a sense that undermines the pursuit of science more generally or, for that matter, undermines the separation of State and Church in the US Constitution".(Rebuttal of Dover Expert Reports, Kitzmiller, *et al.*, *vs* Dover School District, *et al.*, 2005: 1)

[15] If the discussion moves to another level and addresses the context of the classroom, the next gambit will be to offer uncontroversial pedagogical norms to introduce Intelligent Design or germane topics; such has been the function of the "Teach the Controversy" campaign: appealing to the pedagogical interest of scientific controversies to introduce a fabricated controversy between standard biology and ID. See Campbell 2003 for an example and Branch, Scott, and Rosenau 2010 for an analysis.

Even if the direct skeptical risks are avoided, another danger is that even bad science will qualify, provided it even remotely fits the criteria: a climate change denialist paper, for example, "playing" the effect of the sun against that of greenhouse gases, would certainly qualify. The criteria are not precise enough: they do not say anything about *which* science is worth teaching. They seem to provide a useful demarcation between science and pseudoscience, while what would be needed, if they were to be generalized for the screening of curricula, is a characterization of *good and teachable science*. Science is not only a descriptive term, it is also a normative one, and it is possibly the latter sense which is implied here.

Being mindful of such differences in reference is crucial for pragmatists. Peirce, in *How to Make our Ideas Clear* (1878), wrote his famous maxim about reaching the third grade of clearness, in addition to being familiar with a notion and having a definition for it. The "pragmatist maxim" introducing this "third grade" is the following: "Consider what effects, that might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object" (W3: 266). For example, in the context of mechanics, the notion of "force" does not refer to a shadowy entity but to the way we compound partial accelerations. "Hard" for a mineralogist means that his knife will not be able to scratch this rock. A term can be ambiguous, which is often the case in our conversations, and we generally disambiguate it by describing the context ("the practical bearings") we have in mind. Still, we must not have too narrow an idea of these practical bearings: they need not refer to particular sensations, or immediate gratifications, but should also cover more complex situations, such as making a judgment, solving a problem, building evidence… To give a pragmatist clarification is to make that background explicit by referring to what we aim to do, to our purposes, and that is exactly what was missing in the above example. In his comments on Whewell, Peirce understood scientific controversies this way. They always presuppose something beyond the terms of the controversy, which gives the latter all its weight and importance:

> A clear conception resulting from a discussion is often formulated in a definition, but […] in that case some proposition expressed or implied has always gone along with the definition. Thus along with the definition of the uniform force goes the proposition that gravity is a uniform force and along with the definition of the *Vis Viva*, and in the whole discussion concerning it, it is assumed that in the mutual action of bodies the whole effect of the force is unchanged (W2: 342).

We miss the gist of controversies if we just focus on definitions and fail to have the larger picture in mind. In the same way here, we need pragmatism

both for assessing the skeptical risks introduced by criteria *and* to account for what criteria should do and what they actually do.

4. *Daubert and the "Federal philosophy of science"*

Definitions of science can also be found in texts regulating the admissibility of scientific expertise in court.[16] Such is the case of the Daubert framework, used in federal courts and in some states of the United States. I shall not embark here into a discussion of all its philosophical aspects, since it is one of the most discussed legal texts. Instead, I shall confine myself to the un-pragmatic view of science present in what is called the Daubert "trilogy"[17] –*Daubert vs Merrell Dow* (1993), *General Electric Co. v. Joiner* (1997) and *Kumho Tire Co. v. Carmichael* (1999) –, examining it from the perspective and with the tools provided in the previous section.

What prompted this series of legal texts trying to redefine scientific expertise was initially the 1975 new Federal Rules of Evidence (FRE), which were too lax according to some observers (Huber 1991).[18] In one famous case, a psychic was even able to sue a clinic, claiming that she had lost her psychic powers after a scan, and she found an expert to assist her. In the context of *Daubert vs Merrell Dow*, a more classical case over a morning-sickness medication called Bendectin, the US Supreme Court had to rule, after a series of appeals, about the admissibility of an expertise which consisted in in vitro, in vivo analyses, pharmacological studies and a reanalysis of publications. The judges then issued a set of criteria, with additions in 1997 and 1999, which were incorporated into the FRE702:

> Many considerations will bear on the inquiry, including (1) whether the theory or technique in question can be (and has been) tested, (2) whether it has been subjected to peer review and publication, (3) its known or potential error rate and the existence and maintenance of standards controlling its operation, and (4) whether it has attracted widespread acceptance within a relevant scientific community (Daubert Standard, 509 U. S. 579, 1993).

As mentioned, this set was supplemented by others over time: *General Electric Co. v. Joiner* stated that the initial judgment could be reversed only in the

[16] It is not always the case; for another system, see Leclerc 2005. On Daubert, see Kaye 2004. On the implicit epistemology of these texts, see Haack 2005; 2010; 2016.

[17] Legal scholars have explored both the incorporation of Daubert in the Federal Rules and recalcitrance to those rules, see Bernstein and Lasker 2015.

[18] Peter Huber's standpoint is not neutral as he was, with the Manhattan Institute, a fierce critic of the legal framework regulating torts litigation (Huber 1990).

case of an "abuse of discretion" by the trial judge, and the framework was also extended to non-scientific, *e.g.* technical, expertise in *Kumho.* The criteria seem reasonable: the first one can fit either Hempel or Popper, depending on our understanding of "tested". Although it can be a problem if the case involves a medication that has been discontinued, or in situations where a test is not possible anymore, it involves a basic assumption and is not controversial *per se.* The second one is closer to the sociology of science: it is also reasonable, but a rare disease or the rare side-effects of a medication are not always documented in peer-reviewed journals. The third one is technical. The fourth is another version of a former criterion called the "Frye test". It was elaborated in the context of a 1923 trial where the expert, William M. Marston, a noted psychologist, proposed the use of a polygraph as a lie detector in favor of the defendant, James Frye. The expertise was dismissed because the judges felt that such a technique had not gained "general acceptance" in the relevant community:

> While the courts will go a long way in admitting experimental testimony deduced from a well-recognized scientific principle or discovery, the thing from which the deduction is made must be sufficiently established to have gained general acceptance in the particular field in which it belongs (FRYE vs. UNITED STATES. 293 F. 1013 (D.C. Cir 1923). 54 App. D. C., at 47, 293 F., at 1014).

Using such a test implies deciding which principles are "generally accepted" in a community. The problem is thus to know whether a judge, as opposed to an STS scholar or a scientist from the field, is in a good position to make such a judgment. In this instance, if the relevant community was that of experimental psychologists in 1923, it might be claimed that the use of the polygraph was in fact "generally accepted" (McCormick 1927, Alder 2007).

Other criteria were added in the course of time. The Court also made clear, in what is often called *Daubert II*, that the expertise should have no "inherent bias" and that, if developed in view of litigation, it should be treated with caution:

> One significant fact to be considered is whether the experts are proposing to testify about matters growing naturally and directly out of research they have conducted independent of the litigation, or whether they have developed their opinions expressly for purposes of testifying (Daubert v. Merrell Dow Pharmaceuticals, Inc., 43 F.3d 1311, 1317 (9th Cir. 1995)).

I have three series of remarks here:

1) *On the main presupposition*. As shown by Haack in her series of papers, the Supreme Court tried to derive reliability from validity. What was needed

was a reliable expert; the Supreme Court ruled that this reliability depends on whether a valid scientific method has been used. To qualify as a scientific expert, the expert has to convince the judge, in pretrial, that his/her testimony is founded upon science, upon "the" scientific method.

> In order to qualify as "scientific knowledge," an inference or assertion must be derived by *the* scientific method. Proposed testimony must be supported by appropriate validation—*i.e.*, "good grounds," based on what is known. In short, the requirement that an expert's testimony pertain to "scientific knowledge" establishes a standard of evidentiary reliability (509 U. S. 579, 1993, *italics mine*).

Trying to derive reliability from validity and mentioning Popper's authority is already a mistake in itself.[19] Moreover, the Supreme Court's implicit major premise, which we might call its "Master Argument", can be summed up this way: "there is a uniquely rational mode of inference or procedure of inquiry used by all scientists and only by scientists". Referring to "the" scientific method or to any equivalent is referring to a ghost: there is no such thing in general, and for a pragmatist it will immediately give rise to skeptical challenges. In addition, placing the judge in the situation of a gatekeeper turns him or her into a "super-epistemologist" (or "amateur scientist"), and no criteria are given to assess this kind of expertise. Peircean pragmatists, who frame inquiry as a communal endeavor, will also remark that, while the second and fourth criteria stress the social nature of science, the decision made by the judge will be a solitary one.

2) *On "Skeptical pressures".* If we have the initial picture of standard skepticism in mind, with over-optimistic goals, unreliable methods and epistemic obstacles, this set of criteria can be read as raising hurdles, and thus multiplying obstacles. This can be a good thing if it removes manifestly unscientific expertise from the courtroom. But if the criteria are interpreted as cumulative, the risk is that some science will not be able to make it into the courtroom: someone suffering from a rare disease might not have peer-reviewed research to back him up, so that the expertise might not fit the second criterion. Fingerprint identification had been used in court for over a century but its methods and results had not been published in peer-reviewed journals, fingerprints experts had to face Daubert challenges (Cole 2009). If research made in preparation for litigation is dismissed, some plaintiffs will have no case, since they need such research to substantiate their claims about undocumented effects, while corporate research made years in advance might not suffer from such a problem.

[19] "Corroboration (or degree of corroboration) is […] an evaluating report of past performance. [I]t says nothing whatever about future performance, or about the 'reliability' of a theory" (Popper 1972: 18).

It is possible, but I shall leave this question to historians, that the Post-Daubert era has been "tougher than before on expert testimony proffered by plaintiffs in civil cases" (Haack 2005: S70). David Michaels went even further, claiming that these hurdles prevented plaintiffs from having their day in court:

> While Daubert may have chased out some lawsuits based on questionable science, it serves to erect hurdles for scientific testimony and do not reflect the way science works, hurdles that may unduly protect wealthy and powerful defendants (Michaels 2008: 175).

He feared, or predicted, that the same evolution would occur at the level of regulatory agencies, pressed to adopt Daubert-like criteria:

> Likewise, the legal, economic, and political obstacles that regulators already face will seem trivial compared to what they will face if Daubert-like criteria are applied to each piece of scientific evidence used to support a regulation (Michaels 2008: 174).[20]

3) *Pragmatic concerns*. The problems mentioned in (1) and (2) can ground an argument in favor of a pragmatic approach to demarcation problems, of the kind defended by Resnik: demarcation criteria do not work in the abstract, they presuppose values, and they all have practical effects, which should be made explicit at the outset. Resnik argued in favor of including a multiplicity of interests when defining science for expertise, depending on the context: "[p]ractical interests and concerns should play an important role in answering the question 'what is science?' because they form an important part of the pragmatic features of this kind of question" (Resnik 2000: 262). Resnik further claimed that, without subscribing to a relativistic view of science, some criteria were more relevant in some contexts, such as education, law, medicine or engineering, and that, depending on the ends one was pursuing, a more conservative or a more liberal approach could be endorsed:

> Some emphasize testability or verifiability, others emphasize empirical support or reliability, and still others emphasize rational consensus, progress, problem-solving ability, explanatory power, and so on (Resnik 2000, 262).

These criteria all describe something that is an integral part of scientific research. Such a perspective would alleviate the skeptical risk induced by the "Master Argument" over science, while also helping to critically examine the kind of hurdles introduced by that series of criteria; finally, and more substantially, it would provide a more flexible framework. Caudill and LaRue, in what

[20] On this, see McGarity and Wagner 2008.

they consider to be a pragmatist contribution to the debate, have proposed corrections to the criteria, more likely in their view to be adopted by federal judges than Resnik's proposal: "*(1)* Medical Diagnosis Often Relies on Patient Reports, Not Objective Measurement Techniques, *(2)* Science Involves Uncertainty, Teamwork, and Alternative Explanatory Models, *(3)* Science Is Probabilistic, Not Certain, *(4)* Not All Scientific Knowledge Is Peer Reviewed and Published, *(5)* The Limitations of Social Science Do Not Make It Unscientific" (Caudill and LaRue 2003: 24-29). I fail to see any reason why these more flexible criteria would necessarily be more lenient, and they would surely fit the actual practice of science much better.

5. *Pragmatism and policy expertise*

With the case of Daubert, we have focused on scientific expertise provided by individual experts. Of course, the evaluation of the expert by the judge can take a dialogic form, but it addresses a single testimony, and the dialogue takes place before the expert testimony. However, this is only one of the possibilities for scientific expertise: there are more collective forms, which could also benefit from pragmatist insights and are better described in pragmatist terms, that is to say, in terms of what they aim *to do*, in terms of what they achieve, in terms of how they transform a situation.

Here, two approaches are possible. One can provide a typology of expertise where one variant will be considered "pragmatic", or more pragmatic, than the others: such will be the subject-matter of the present section. Another approach is to provide a pragmatic account of expertise, in particular collective expertise, including models that are not termed "pragmatic" or "pragmatist" by their advocates. I shall address this point in the next section. The two projects may certainly overlap but they have different goals: the first one regulates competing models of expertise, one of them being termed "pragmatic" or "pragmatist" for reasons that will be explained; the latter provides a pragmatic account of what collective expertise *does*.

I shall borrow a typology fitting the first scenario from Martin Kowarsch, as developed in Part II, Chapter 4 of Kowarsch (2016).[21] I shall focus here on Kowarsch's refinement of Habermas's typology, which is particularly helpful for

[21] Let me say clearly that I'm also well aware of the distinctly pragmatist contribution offered by Kowarsch in this monograph, in particular in his treatment of the fact/value entanglement, and that anyone interested in policymaking should read this book. As the author announces, the book provides "a philosophical framework for an appropriate contribution of the indispensable social-science expertise, particularly economics, to the public evaluation of and reasoning about climate policy options" (Kowarsch 2016: vi), a much-needed task indeed.

our discussion. Kowarsch addresses the normative underpinnings of four classical models of scientific expertise that are prevalent in policy: a "decisionist" model, a "technocratic" model, a "legitimisation" model and a "pragmatic" one.

In the first one, the Decisionist model, the ends, which cannot be established by experts (because of an alleged fact/value dichotomy and because they involve values[22]), are determined through political negotiations, and scientists provide expertise about the *means* required to achieve those ends. In this pattern, there are three distinct roles: the public can provide a formulation of the problem or raise claims, policymakers determine both the policy goals *and* the implementation of policies, and expert-scientists cast light on the appropriate means.

The Technocratic model, much discussed these last few years under the name of *epistocracy*, has a different structure: "[t]he proponents of the technocratic model argue that due to the increasing and huge complexity as well as the novelty of current policy problems, they can no longer be solved by politicians" (Kowarsch 2016: 88). If in the first model the public was able, in theory at least, to exert some pressure on policymakers regarding the formulation of the problem, and more generally the political agenda, its role is much more limited here: scientists, perhaps in collaboration with the public in the more liberal versions, are required to identify and formulate the relevant problems; to identify the relevant goals; and to prescribe the means, while policymakers, at the end of the process, simply implement the recommended policies. As Kowarsch remarks: "[t]he technocratic model suggests that scientific consensus can and should be created only through pure science itself, and denies the role of society, culture or politics in scientific knowledge production" (Kowarsch 2016: 90). Any strong mobilization from the public can only be counterproductive, and certainly irrational in some ways.

The first two models presuppose a strong separation between science and policy-making: they are variations of the "linear model", where policies somehow derive linearly from sound science, whether this scientific authority is omnipresent, as in the second model, or whether it is confined to the study of the means. The linear model also presupposes a grim picture of the public's ability to understand current challenges and to have an informed and valuable opinion about it. Both models involve substantial claims about the rationality of values and the possibility of rational public debates.

The "Legitimisation model" makes an instrumental use of scientific authority to legitimize policies: "[p]olicy options are legitimated by referencing scientific expertise, although – in contrast to the technocratic model – at least some of the players involved are well aware of the fact that the particular policy

[22] See Gronda in this volume for a perspective on this alleged dichotomy.

cannot be determined by the sciences in a largely 'value-free and objective' manner" (Kowarsch 2016: 94). This model looks like the decisionist model, but in surface only: the "science" (or "sound science") is carefully cherry-picked to fit the political ends. The epistemic authority here is just a mask for the sheer exercise of power.[23]

The "Pragmatic model" ("Pragmatistic", in Habermas) rejects both the technocratic idea that scientists, and only scientists, can settle means and ends, and the decisionist idea that the identification of means should be left to scientists only: "advocates of the pragmatic model usually state that the sciences cannot offer, roughly spoken, 'absolutely true' knowledge and that scientific knowledge is always highly value-laden" (Kowarsch 2016: 91). It involves, for the ends as well as for the means, "a critical interaction between the sciences, policy and the public" (Kowarsch 2016: 92). Seeing how this model is more pragmatic than the others might seem difficult at first sight, but the criticism of the fact-value dichotomy plays a core role here. This was already the case in Habermas's account, who insisted on the interaction between the expert and the politician: "[i]n the pragmatistic model the strict separation between the function of the expert and the politician is replaced by a critical interaction. This interaction not only strips the ideologically supported exercise of power of an unreliable basis of legitimation but makes it accessible as a whole to scientifically informed discussion, thereby substantially changing it" (Habermas 1971: 80). This last model is the most democratic one, since "social interests, as reflected in the value systems, are regulated by being tested with regard to the technical possibilities and strategic means for their gratification" (Habermas 1971: 80).

The democratic conception of the publics in Dewey, as well as the general criticism of the fact-value dichotomy (Putnam 2002), seem to give this model an advantage. The claim would thus be that, when it is possible to follow this Pragmatic Model, the "interactional" element allows us to reach the best balance by including all stakeholders. The adoption of a pragmatist perspective undermines the assumptions at the ground of the other models.

6. *Pragmatist accounts of collective expertise*

In the previous section, we have seen that one kind of model was deemed more "pragmatist" than the others: it is so if we understand "pragmatism" to involve substantial theses about the role of the public, along the lines of the third pragmatist claim presented in the introduction. If we consider the second claim, about meanings, and the fourth one, about beliefs as modes of action, another

[23] I cannot comment in detail here, but a very illuminating account is given in McGoey 2019.

pragmatist contribution can be helpful. The argument, this time, would be that all the models are better described in pragmatistic terms anyway: if technical terms are to be elucidated through reference to "practical bearings", as Peirce would have it, having a framework where expertise models are so depicted would bring us closer to a pragmatist reading of what collective expertise does.

Chateauraynaud and Debaz have developed an interesting matrix for collective forms of expertise in several texts, and particularly in their last major book (Chateauraynaud and Debaz 2017). It is no accident if we can give a pragmatist reading of their typology: for decades, Chateauraynaud has been developing an original pragmatist sociology in his lab, the *Groupe de Sociologie Pragmatique et Réflexive*.[24]

In his (Chateauraynaud 2009), Chateauraynaud, in addition to mentioning his relationship to Peirce's semiotics, Dewey's Theory of Inquiry and Habermas's theory of communicative action, provides three cornerstones of his own sociology. It is a sociology of "grips", or "grasps" ("*prises*" in French): it deals with the "means that lay or professional actors develop in their ordinary world in order to keep control on current actions, and the problems which arise when they experiment a break or a lack of grip". The idea is that, for a social world to even be possible, people and collectives need "common grips". This first claim is of course a distinctly pragmatic one, and it intersects with what Chateauraynaud has developed elsewhere about the "tangible" (Bessy and Chateauraynaud 2014). One thinks, of course, of Dewey and his distinction between the abstract external "grasp" of the rationalist and the pragmatist, interactional version of it: "[t]he essential difference is that between a mind which beholds or grasps objects from outside the world of things, physical and social, and one which is a participant, interacting with other things and knowing them provided the interaction is regulated in a definable way" (Dewey 2008: 160). Chateauraynaud provides a very detailed analysis of the ways in which this interaction can emerge or fail. The second claim involves possible transformations of society, through "the precise description of processes by which an alert or a criticism is taken seriously by different actors and enables them to transform collective devices, norms and institutions". The last series of claims, which gave rise to Chateauraynaud's notion of argumentative "ballistics", deal with disputes:

> What kind of disputing procedure is available and how [do] actors deal with the plurality of debate arenas or with the different forms of public discussion? How [do] controversies, public debates, court trials and political mobilizations affect the course

[24] A detailed and authoritative version can be found in Chateauraynaud 2011, but English-speaking readers can find a précis of Chateauraynaud's approach in *Public Controversies* (2009), where he defines his own Transformative Pragmatism.

of social transformations? […] A key issue is at stake: in what conditions new arguments could appear, could be transformed in common features and could inform the design of standard devices? (Chateauraynaud 2009: 7).

For our purpose, variants of this third claim are crucial. In *Aux bords de l'irréversible*, Chateauraynaud and Debaz provide such a categorization of expertise in the context of "chronic uncertainty" (Chateauraynaud and Debaz 2017: 126). In addition to the traditional "monologic" expertise, where experts deliver a report and, if requested, an advice or a testimony, four categories describe the new regimes of expertise quite well: (1) "Contradictory expertise", (2) "Collective expertise", (3) "Distributed expertise", and (4) "Participatory expertise". These categories do not focus on the person or the skills of the individual expert, but allow us to understand what expertise is in the context of radical ignorance and controversy; they all have a specific pragmatic texture.

How is that so? First, they point to collective behaviors, ways of doing, contexts of action. The first model refers to contradictory expertise, quite frequent in the legal realm, but also in public "arenas" and public debates: NGOs often provide this kind of counter-expertise. Social movements, in the context of "undone science", can unite in order to provide such kind of expertise. The second one, "collective expertise", has a different goal: articulating different skills and disciplines in view of a regulation. The main motive is not the conflict anymore (or not only) but the plurality of views and understandings of a complex phenomenon; it can be led by an agency, often after an environmental or sanitary crisis. The IPCC is one example, as well as the French Inserm Committee on Asbestos. The third one, as the name suggests, is distributed among labs, agencies, NGOs, and the goal is rather to explore different scenarios. The fourth one involves interactions between experts (in the classical sense) and citizens. The French *Consultation citoyenne sur le climat* (*Citizens' Convention on Climate*), gathering 150 citizens and experts, is one example.

This pragmatic emphasis also holds for the expected outcome of the process of expertise. Each form of expertise is better described by its expected result rather than by more formal features: a verdict or a decision, sometimes a policy, in the first case, and thus an overcoming and transformation of the initial conflict; a consensus, the formulation of norms and standards, in the second case; a plurality of visions of the future or of the object available for policymaking in the third case; trust in the last case, and possibly a revision of the distribution of epistemic authority.[25] That is why Peirce's pragmatist maxim would be of

[25] "This is undoubtedly what should be retained from this fourth model: its capacity to raise questions, alternatives or possibilities that the dominant actors, a fuzzy set that includes authoritative scientists, tended to dismiss out of hand" (Chateauraynaud and Debaz 2017: 131).

great help here: we use the same word, "collective expertise", to refer to quite distinct practical bearings, whether they describe the action undertaken or the practical outcome of the process.

We do not need to choose between the two approaches described in Sections 5 and 6. Pragmatism certainly involves substantial claims regarding Democracy and, except for specific contexts, the pragmatist model certainly allows for a better distribution of rationality overall, but if we think of public controversies, having a pragmatist understanding of what expertise models are doing, what kind of transformation they achieve, allow or promote, is certainly necessary to empower agents and allow them to enjoy a more lucid citizenship.

7. *Expertise on ignorance?*

A final area in which a pragmatist approach is valuable is, paradoxically perhaps, that of ignorance. In this last section, I shall proceed in two steps: first, by trying to show that there can be an expertise on ignorance, and not only on reliable knowledge, and secondly, by showing why a pragmatist epistemology is one of the best candidates to frame this kind of expertise.

"Ignorance studies", or even "Agnotology", to use Robert N. Proctor's term (Proctor and Schiebinger 2008), is now an established field of research in STS as well as in philosophy. It even has its own *Routledge Handbook* (Gross and McGoey 2015). If the idea of ignorance as an interesting subject-matter is by no means new (Ferrier 1854), the impetus for the more recent body of works was certainly given by Proctor in his *Cancer Wars*: as the subtitle of his book made clear, he explored "How Politics Shapes What We Know and Don't Know about Cancer" (Proctor 1995). Some research programs, focusing on the genetic predispositions to cancer (as in the Nixon Plan), could lead to overlooking its behavioral and environmental causes. In the context of limited time and means, any strong investment in research on one factor of a given disease or problem can result in more knowledge being accumulated about this factor and, conversely, to relative ignorance about the other factors. The idea was that, in addition to epistemology – the study of knowledge – and to sociology – the study of the social conditions and texture of knowledge –, we needed another line of inquiry, studying how and why we do not know what we do not know. In other terms, Proctor's agenda was to provide an account of the "cultural production" of ignorance, and he argued that ignorance was not only an epistemic state but also, in some contexts, an *effect*, whether its causes were structural, emergent or intentional. I have tried to show elsewhere that pragmatist theories of inquiry have allowed us to understand these processes: if inquiries are modes of action (actions under severe formal and methodologi-

cal constraints), some of the categories of action can be applied to them, *i.e.* failures, persistent failures and persistent failures caused by the actions and strategies of third parties (Girel 2017b).

Agnotological studies, or "Ignorance studies", soon became a platform and, in Proctor's case, this intuition provided the core of his monumental *Golden Holocaust* about the tobacco industry (Proctor 2011). Grounding his inquiry in the millions of pages retrieved from the internal archives of cigarette manufacturers, Proctor showed precisely how this industry was able to undermine otherwise reliable knowledge about the hazards of tobacco, thus "creating" ignorance in the public about them, putting pressure on biomedical research, on expertise, and even hiding some of the most worrying details to its own workers. This is only one of the possibilities for "ignorance studies": others have explored strategic ignorance (McGoey 2012), climate change denial (Oreskes and Conway 2010), "undone science" (Hess 2016), not to mention the understanding of scientific research as "thoroughly conscious ignorance". Let us admit, for the sake of argument, that ignorance can be an academic topic, common to epistemology, history and sociology of science. Can there be an expertise about it?

Recent history provides interesting examples. There are cases in which an expert must report on what is not known in a given field. Proctor is also an expert witness before the courts, and has been called upon in numerous lawsuits that have pitted tobacco companies against patients or families of patients. It is easy to understand why: in the lawsuits that opposed them to former consumers, tobacco companies often defended themselves by claiming that "everyone" knew that cigarettes were toxic, or addictive, but that "no one had any evidence" (Proctor 2006). The assumption was that smokers were responsible, since they had started and continued smoking knowingly, but not the tobacco companies, since there was supposedly no scientific proof of the hazard. Scientific expertise was then mobilized to trace who knew what and when, which became decisive in attributing responsibility. In this sense, there is therefore an expertise on ignorance, and by extension an expertise on the attribution of ignorance. I shall take just one example here, among numerous others. In a Canadian trial in 2012,[26] Proctor made it clear that knowledge about tobacco hazards can mean two different things: "[s]o the theory, as it developed by the historians working for the tobacco industry in the United States, was that everyone knew that smoking was bad for you – in other words, common knowl-

[26] All quotes from Proctor's testimony for Nov 28, 2012. *Cécilia Létourneau v. JTI-Macdonald Corp, Imperial Tobacco Canada Ltd. and Rothmans, Benson & Hedges Inc; Conseil québecois sur le tabac et la santé and Jean-Yves Blais v. JTI-Macdonald Corp, Imperial Tobacco Canada Ltd. and Rothmans, Benson & Hedges Inc.* <https://www.industrydocuments.ucsf.edu/docs/xmxh0225>.

edge –, but no one could prove it – in other words, expert ignorance. And this epistemology, you might say, is the most common that's put forward by historians who work for the industry". Although Proctor is in a better position to discuss what knowledge manufacturers actually had (and then to determine whether there was really such "expert ignorance"), the other side of the coin, "common knowledge" is also pivotal. "Common knowledge" can refer to what people were told, were aware of, or to what they believed, which is a different thing. In these trials, one bias among the historians working for the manufacturers was to exaggerate this "common knowledge" by interpreting every public bit of information as common knowledge. This is exactly what a historian expert can help clarify. About one expert, Proctor adds: "my criticism is that he really only looked at what people were told and not enough at what they believed". And then a bit further, about the mere circulation of information, awareness and belief: "I mean, awareness is in between, because it's fundamentally a marketing concept; it's a measure of exposure, not conviction. And that's why I object to the whole notion of awareness, it's vague. Does it mean 'were you told' or does it mean 'do you believe'?"

What does this have to do with pragmatism? We can see at once that the expertise does not only involve facts, but also doxastic states. Ultimately, the whole debate revolves around philosophical questions that were crucial for the doubt-belief pragmatist approach to inquiry: did the smokers have hypothetical beliefs, full beliefs, dispositions? How can we account for the distinction between the beliefs we profess and the beliefs we betray? There are expert reports on what people know, and what they do ignore, because knowledge is not only a cloud of information, it is a capacity. Knowing in the full sense involves using previous beliefs and information to ask questions, to extend one's knowledge, it involves the capacity to justify one's beliefs, to justify one's practical judgments, it is exactly what the pragmatists were trying to make clearer, and it is exactly what distributes responsibilities in this kind of trial.

8. *Conclusion*

In this paper, I have shown that pragmatism, and in particular pragmatist theories of inquiry, not only addressed the issue of scientific expertise, but also provided interesting tools to account for it, whether in court or in public debates. I have also claimed that the strong anti-skepticism of the movement could be a safeguard against careless criteria and, further, that a pragmatist account of meaning could cast light on the contexts in which these criteria are functioning. Telling which difference makes a difference, trying to "Make it explicit", to borrow Brandom's phrase, is one of the most enduring endeavors

of the pragmatist movement, and it is particularly needed here, as the debate has often focused on definitions, or on general forms of expertise, without exploring their practical background.

Scholarship, in recent years, has actively contributed to extending the pragmatist canon: Peirce, James, Dewey and Mead have been joined by many others, DuBois, Locke, Mary Follett and Jane Addams for the "classical period", and a wide variety of contemporary research from, say, Shusterman to Brandom, Price or Kitcher. There are groundbreaking pragmatist contributions in all walks of academia, from aesthetics to ethics and neurosciences. Having more pragmatist contributions on scientific expertise and others forms of applied knowledge would be a very useful addition. Public debates around scientific expertise raise philosophical, epistemological and practical questions, and if pragmatism were to remain silent on these questions, it would be a severe limitation of its resources.

Regarding scientific expertise, if what has been proposed is sound, it cannot be approached through the resources of one discipline only. Interdisciplinarity is often a very vague word used in answers to calls for projects, but in this case, it is strictly required. Without a dialogue with jurists, who are able to tell what a change in constitutional or legal texts will lead to, scientists from the field, who can tell what the most pressing questions are for them, and sociologists, who can provide the conceptualization and description of the social texture of expertise, a philosophical account of expertise will be incomplete and deficient.

Finally, regarding agnotology, we have seen that it made sense to make room for expertise about ignorance. Such expertise is not confined to the courtroom: there is robust research on "absences" in knowledge, on projects that were abandoned because they were at odds with social norms, on "undone science", science that could be developed with the resources we have but which is not developed until social movements ask for it. These are all cases of unintentional production of ignorance. The kind of abstract characterizations of science and scientific expertise we have studied above can contribute to unnecessary controversies and to a public distrust of science. Deciding whether they deserve their own chapter in agnotological studies is an open question.

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