Studies in History and Philosophy of Science

Stud. Hist. Phil. Sci. 33 (2002) 137–152

www.elsevier.com/locate/shpsa

The experimenters' regress: from skepticism to argumentation

Benoît Godin *, Yves Gingras

INRS, 3465 rue Durocher, Montréal, Quebec, Canada H2X 2C6

Received 31 August 2000; received in revised form 14 March 2001

Abstract

Harry Collins' central argument about experimental practice revolves around the thesis that facts can only be generated by good instruments but good instruments can only be recognized as such if they produce facts. This is what Collins calls the experimenters' regress. For Collins, scientific controversies cannot be closed by the 'facts' themselves because there are no formal criteria independent of the outcome of the experiment that scientists can apply to decide whether an experimental apparatus works properly or not.

No one seems to have noticed that the debate is in fact a rehearsal of the ancient philosophical debate about skepticism. The present article suggests that the way out of radical skepticism offered by the so-called mitigated skeptics is a solution to the problem of consensus formation in science.

Keywords: Argumentation; Skepticism; Sociology of science; Philosophy of science; Scientific controversies

> What we say . . . are claims to community. And the claim to community is always a search for the basis upon which it can or has been established. (Cavell, 1979, p. 20)

Since its publication in 1975, but especially following its formulation in his book *Changing order*, Harry Collins' central argument about experimental practice has revolved around the thesis of the 'experimenters' regress', according to which 'facts' can only be generated by 'good' instruments but 'good' instruments can only be

* Corresponding author.

0039-3681/02/$ - see front matter
PII: S0039-3681(01)00032-2

recognized as such if they produce 'facts'. Analyzing the by now well known debate surrounding the detection of gravitational waves, Collins described how a physicist (Joseph Weber) built a detector and announced to the physics community that he had detected gravitational waves, and how the experimental results were contested by other scientists who, building replicas of the apparatus, could not corroborate the original findings (Collins, 1975).

For Collins, this controversy could not be closed by the 'facts' themselves because there were no formal criteria independent of the outcome of the experiment that scientists could apply to decide whether the experimental apparatus was working properly or not. In the face of an unknown phenomenon scientists cannot be sure they have a 'good' instrument. In Collins' words: 'we won't know if we have built a good detector until we have tried it and obtained the correct outcome. But we don't know what the correct outcome is until . . . and so on *ad infinitum*' (Collins, 1992, p. 84). This is what Collins calls the experimenters' regress.

The idea that a circular relation exists between belief (or not) in an outcome and acceptance (or not) of the value of the apparatus producing it has given rise to many discussions among philosophers of science trying to show the inadequacy of Collins' analysis. No one, however, seems to have noticed that the debate is in fact a rehearsal of the ancient (Greek) philosophical debate about skepticism. We would thus like in this paper to suggest that it may be useful to look at the original skeptical debate to see how it can shed light on the discussion of Collins' thesis. It should be stressed at the outset that our main point is not simply to note that Collins was 'anticipated' (that would be useless) but to show that the way out of skepticism offered by the so-called mitigated skeptics can also be used in the discussion on consensus formation in science. Our approach is thus less a critique of Collins than a proposition that a thoroughgoing sociology of science is best formulated in terms of an explicit theory of argumentation going on in a scientific field. But first we must begin with a brief presentation of the original skeptical arguments.

1. Sextus Empiricus, Montaigne and the experimenters' regress

Skepticism has a long history, going back to the Greeks (Groarke, 1990). Strongly criticized by Plato and Aristotle, it essentially vanished from the philosophical scene until its revival in the sixteenth and seventeenth centuries after the rediscovery of the works of Sextus Empiricus, a third-century Greek philosopher. As is well known, Montaigne has had enormous influence on skeptical reasoning (Popkin, 1979), and the *Essais* (particularly chapter twelve of book II, the *Apologie de Raimond Sebond),* first published in 1580, offers a formulation of the skeptical argument that is absolutely identical to Collins' definition of the experimenters' regress. Montaigne was summarizing for a larger public Sextus' *Outlines of Pyrrhonism*, published in Latin in 1562:

> To judge the appearances that we receive from objects, we would need a judicatory instrument; to verify this instrument, we need a demonstration; to verify this

> demonstration, an instrument: there we are in a circle. (Montaigne, 1938–1942, Vol. II, p. 322)

The major problem in which skeptics are interested, and which also gives rise to the debate in sociology of scientific knowledge, is that of finding a criterion on which to ground the truth of knowledge. The basic skeptical argument is that any attempt to justify a criterion requires another criterion, which in turn has to be justified, and so on. As Sextus himself put it, in an argument identical, again, to that of Collins:

> In order for the dispute that has arisen about standards to be decided, one must possess an agreed standard through which we can judge it; and in order to possess an agreed standard, the dispute about standards must already have been decided. (Sextus Empiricus, 1933, Vol. II, p. 20)

The arguments of the skeptics have been applied against the use of reason and against the use of the senses as absolute sources of knowledge. In the first case, one affirms that logical reasoning, namely syllogism, involves a circle (Popkin, 1979, p. 38). As Sextus explains:

> When we argue 'Every man is an animal, and Socrates is a man, therefore Socrates is an animal', proposing to deduce from the universal proposition 'Every man is an animal' the particular proposition 'Socrates therefore is an animal', which in fact goes to establish by way of induction the universal proposition, we fall into the error of circular reasoning, since we are establishing the universal proposition inductively by means of each of the particulars and deducing the particular proposition from the universal syllogistically. (Sextus Empiricus, 1933, Vol. II, p. 196)

Since logic cannot provide a secure foundation to knowledge, maybe the senses can, and this argument is more interesting in the context of Collins' thesis about experiments. In his reflections on truth, Sextus argues that there are three criteria for assessing truth: senses, instruments and logic. He accordingly identifies three problems: that of the agent who judges, that of the instrument used in the judgment, and that of the process by which the judgment is arrived at. On the agent, he writes:

> It would not be possible to show that objects ought to be judged by (Man) . . . Who is to be the judge that the criterion of the Agent is Man? . . . That would be assuming the point at issue. (Sextus Empiricus, 1933, Vol. II, pp. 34–36)

Sextus then accepts, for the sake of the argumentation, that man is the agent of judgment. But if this is so, he asks, which man should we follow? He offers two solutions, one individualistic, the other social. In the first case (Sextus Empiricus, 1933, Vol. II, p. 39), we could follow the most intelligent man. But, again, who can tell which man is the most intelligent? In the second case, we could rely on consensus among all men. But Sextus maintains that consensus is practically impossible:

> For first, what is true is no doubt rare . . . Secondly, for every standard there are more people opposed to it than in agreement about it. (Sextus Empiricus, 1933, Vol. II, p. 43)

Regarding the next two questions, those of the senses and the intellect as instruments, Sextus writes:

> How will we decide that we ought to give heed to these senses and this intellect and not to those, seeing that they possess no accepted criterion by which to judge the differing senses and intellects? . . . If we shall judge the intellects by the senses, and the senses by the intellect, this involves circular reasoning inasmuch as it is required that the intellects should be judged first in order that the intellects may be tested . . . We possess no means by which to judge objects. (Sextus Empiricus, 1933, Vol. II, pp. 64–69)

Greek skepticism was never about the existence of the external world but simply about our ability to know reality (Larmore, 1998). Moreover, all Greek skeptics seem to have admitted that effects could be known (Groarke, 1990). As Sextus explains:

> When we question whether the underlying object is such as it appears, we grant the fact that it appears, and our doubt does not concern the appearance itself but the account given of that appearance,—and that is a different thing from questioning the appearance itself . . . The Skeptic states what appears to himself and announces his own impression in an undogmatic way, without making any positive assertion regarding the external realities. (Sextus Empiricus, 1933, Vol. I, pp. 19, 15)

Briefly stated, the skeptic is a pragmatist:

> Suppose there were a road leading up to a chasm, we do not push ourselves into the chasm just because there is a road leading to it but we avoid the road because of the chasm; so, in the same way, if there should be an argument which leads us to a confessedly absurd conclusion, we shall not assent to the absurdity just because of the argument but avoid the argument because of the absurdity. (Sextus Empiricus, 1933, Vol. II, p. 252)

2. Finding a way out

A lot of arguments were put forward by seventeenth-century philosophers to invalidate skeptical arguments (Popkin, 1979). Among those who rejected scholasticism but still wanted to ground knowledge in absolute foundations were Bacon and Descartes, who followed opposite routes. Bacon saw in induction a secure way to truth, whereas Descartes' deductive view of knowledge located the foundation in the evidence of the *cogito* (Larmore, 1998). Despite their opposition, it is worth noting that both agreed to find an *absolute* foundation to knowledge, albeit of a different sort than the one offered by Aristotle.

For our purposes, the position of mitigated skepticism is the most interesting, and Mersenne and Gassendi are the prominent representatives of this position. For them, the skeptical problem arises only in connection with the non-evident things hidden from us. Some things are obvious, and are easily accepted as evident even by the skeptics. Non-evident things are either absolutely non-evident (like infinity), naturally non-evident (known only by signs or intermediaries), or temporally non-evident. Though Mersenne was opposed to any speculation about hidden entities, Gassendi, an atomist, thought that these explanations were legitimate. For Gassendi, the naturally and temporally non-evident things require instruments, experience and reason, which thus play the role of criteria making their knowledge possible even though this knowledge is never of the things themselves but always as they appear to us (Popkin, 1979, p. 141). These distinctions call to mind the notion of 'degree of externality' used by Pinch (1986),[1] and they suggest that the more an 'object' is removed from direct unaided observation, the more room there is for 'interpretive flexibility', that is, for debate on the meaning of the observation.

From the point of view of classical logic, there seems to be no way out of the skeptical regress. But, as Mersenne himself observed, there is no need to try to contradict the skeptic in the abstract, for there are in practice things that nobody doubts and those are sufficient to ground our knowledge, once we admit we will never have access to the ultimate nature of things (Popkin, 1979, pp. 133–134). For the force of the skeptic follows only under the implicit assumption that true knowledge is evident and absolute. The skeptic is challenging this dogmatism, showing that absoluteness cannot itself have foundation and thus should lead one to suspend judgment concerning anything being absolute. Following Mersenne, one can escape the debate by having a pragmatic view of knowledge, and accept a doctrine 'in those places and parts which are received by all the learned and followed by them by common consent; as for other places which are in dispute, each may follow whatever he judges most likely, and which is supported by the best reasons' (Dear, 1984, p. 204).

Mitigated skepticism argues that we can have a type of knowledge that is not open to question. But this kind of knowledge is not knowledge of the real nature of things. Rather it consists of information about appearances to which it is legitimate to add hypotheses about their constitutions as long as these help in the prediction of the future behavior of appearances.

Of course, Collins is not entirely unaware of the skeptical tradition, but he makes it begin 'with the problem of why we should expect the future to be like the past . . . Inferring general rules from repeated past regular instances is called induction. Thus skepticism engenders what is known as the problem of induction' (Collins,

[1] Collins (1998, p. 306) renamed this notion 'evidential significance'. We prefer Pinch's original term, for the idea of 'externality' refers clearly to external objects, while 'evidential significance' could be mixed with another useful concept introduced by Pinch, namely 'evidential context'.

1992, p. 6). In line with this restricted view of skepticism, Collins discusses work by three authors: David Hume and ideas about cause, Nelson Goodman and the extension of the problem of induction to expectations, and Ludwig Wittgenstein's analysis of following a rule. But as the previous section has shown, skepticism did not 'begin' with the problem of induction, which is only one aspect of the skeptical tradition. Even the problem of induction has been discussed in multiple forms since Aristotle, and, according to Milton, 'the most substantial discussion of induction surviving from either the Academic or the Pyrrhonist tradition is to be found in . . . Sextus Empiricus' (Milton, 1987, pp. 56–57). Compared to the mitigated skepticism of Mersenne and Gassendi, Hume's position can be characterized as a radical inductive skepticism (Milton, 1987, p. 62). As we will now suggest, once we see that Collins' notion of experimenters' regress is conceptually related to radical skepticism, we are in a better position to understand that the only way out of this apparent regress is to deny, as does the mitigated skeptic, the implicit premise that there can be any absolute foundation to knowledge claims.

3. Socializing epistemology

Pascal once said that as long as there are dogmatists, there will be skeptics (Pascal, 1958, no. 374). Pascal's intuition about the relation between the dogmatist and the skeptic is useful to remember in the context of the debate between philosophers and sociologists of science about the empirical foundation of scientific knowledge. It suggests that some of the relativists' arguments only make sense in relation to a foundationalist (and often individualistic) epistemology. From this latter point of view knowledge can be of the ultimate nature and it can be produced by an individual scientist confronting nature through his instruments. Once this foundationalism and individualism is abandoned and the dialogical and argumentative nature of the dynamic of the scientific field is taken into account, Collins' circle is dissolved into a complex dynamic involving experimentation, calculation and argumentation. But before presenting our views on that matter, let us recall what the critics have said of Collins' thesis.

In her review of *Changing order*, Mary Hesse argues that 'Collins speaks as though replicability of individual experiments were the only recognized criterion for scientific validity' (Hesse, 1986, p. 718). But, she adds, 'replicability has never been the working scientist's sole criterion of acceptable science' (ibid., p. 719). Other criteria are always considered in the evaluation of experimental results, such as methods for the analysis of data, coherence between facts and theories, predictability and control in the natural environment, and the development of world-views. That is, arguments directly related to the experiment are always mixed with theoretical, aesthetic and pragmatic criteria such as fruitfulness, simplicity, elegance, unifying potential, absence of contradiction, and coherence with background knowledge and world-views. Hesse maintains that 'Collins regards closure as a social decision not forced by the facts. Not forced perhaps, but it does not follow that social decision has nothing to do with objective nature' (ibid., p. 721). For, she adds, 'the existence of reinterpretations of part of the experimental data in the light of theory or social convention does not entail that there are no constraints upon replicability in experimentation on the natural world, or that decisions to modify or abandon particular theories are wholly matters of social choice' (ibid., p. 710).

The decision of the scientific community to reject claims about gravitational waves, although not rule-governed, was reasonable according to a second critic. Allan Franklin argues that the decision of the community was based on epistemological criteria (Franklin, 1994, p. 472). What was criticized and shown to be wrong by the critics of Joseph Weber was the algorithm used, an algorithm not sensitive enough to different energy fluctuations. 'The critics' results were far more credible than Weber's. They had checked their results by independent confirmation . . ., they had also eliminated a plausible source of error . . ., they had calibrated their apparata' (ibid., p. 484). In sum, Collins would have conflated, according to Franklin, the difficulty of getting an experiment to work with the problem of demonstrating that it is working properly (ibid., p. 487). Using the history of the controversy over the 17 kev neutrino to counter Collins' experimenters' regress, Franklin writes that:

> the history shows us that deciding on the correct answer to the question of the existence of the 17kev neutrino involved not only numerous repetitions of the experiment, but also criticism and discussion of the experimental results, and of the methods of analysis used. The history also shows that these criticisms and discussions were taken seriously and acted upon by the scientists involved. *This was in fact applied epistemology*. (Franklin, 1995, p. 483; our emphasis)

Two pages later he adds 'It seems clear that this decision [to conclude that the 17 kev neutrino does not exist] was based on experimental evidence, discussion and criticism or, *in other words*, epistemological criteria' (ibid., p. 485; our emphasis). It is worth noting that what is here described by Franklin using the terms 'applied epistemology' and 'epistemological criteria' is simply the social dynamic of the field as we defined it in terms of argumentation and as it evolved over time.

When Franklin argues that the skeptical regress can be 'broken by reasoned argument' (Franklin, 1994, p. 465), Collins answers that 'reasonable people are always disagreeing; the most highly trained, highly intelligent experts, of the highest integrity, are always disagreeing with one another' (Collins, 1994, p. 502), thus concluding that reason alone does not necessarily lead to a single conclusion. Collins is here reformulating the classic argument of Sextus, who, as we saw above, observed that 'for every standard there are more people opposed to it than in agreement about it' (Sextus Empiricus, 1933, Vol. II, p. 43).

A third critic of Collins, Culp, suggests eliminating dependence on at least some and possibly all shared theoretical presuppositions by producing a robust body of data that convinces scientists of the objectivity of raw data interpretations (Culp, 1995, p. 441). Though Culp starts by recognizing that objectivity is an intersubjective and social outcome of the scientific community, and thus depends on presuppositions shared by that community, she suggests that these have to be eliminated in order to talk of objectivity. To that end, she proposes a robustness criterion for data: a set of techniques must be developed such that it would be an improbable coincidence for all of them to produce comparable data. How? When comparable data are produced by different techniques and when these techniques do not draw on the same theoretical presuppositions, the data are said to be robust, that is, connected to a common cause. 'This remarkable agreement in the data (interpreted raw data) would seem to be an improbable coincidence unless the raw data interpretations have been constrained by something' (ibid., p. 448).

The criticisms of Collins come close to the idea of community and argumentation as a way out of the circle, but never explicitly incorporate it in their analysis. Franklin argues implicitly that Weber's thesis was abandoned by reasoned (though situated) arguments on the part of other scientists. Culp starts from the intersubjective agreement of the scientific community and its theoretical presuppositions. Hesse argues that ideal-type science includes multiple criteria of acceptable (i.e. community-based) knowledge claims. However, none of these authors, in our view, really pursue all the potential and implications of the idea of community and argumentation. This is necessary, however, in order to break the circle which, properly read, amounts to radical skepticism.

4. Experiments, theory and arguments

Viewing scientific practice as going on in a field (discipline or specialty) defined by specific rules that evolved in time and are embodied in practices can help to avoid the opposition between what is 'social' and what is 'epistemological' (Bourdieu, 1975). We can portray scientific controversies in terms of a three-stage process (Collins, 1992; Pinch & Bijker, 1984). First, a new fact is claimed as a result of experimental work. The researcher responsible for the fact tries to convince other researchers that the fact deserves recognition. Second, because of the interpretive flexibility of data and because of the difficulty of replicating experiments, debates can emerge where conflicting views operate, often supported by totally different experiments. Third, and finally, the debate can be long-lived (such as that on the age of the earth) or relatively short-lived (such as that on cold fusion), but it usually ends with closure within the scientific field.[2]

Since argumentation as here understood (a process including all kinds of ingredients: mathematical, technical, instrumental, logical) is a collective process, the fact that a given individual may 'stick to his guns' without taking into account any counter-arguments does not prove that these arguments do not play a role in the decision process, for it is the community which decides (after a variable period of time) which position becomes dominant, thus marginalizing or even excluding people from the field in the process.[3] Closure is thus often attained at the cost of excluding prac-

[2] A closure can, of course, be only temporary.

[3] For an example of exclusion interpreted in terms of argumentation, see the case of the French geologist Deprat discussed in Gingras (1995) pp. 144–145.

titioners, but this process of exclusion is again a community-based—though most often not official—decision. All this is perfectly coherent with a model of the scientific field governed by (evolving) rules of argumentation.

We have suggested elsewhere (Gingras & Godin, 1997) that the scientific field can be conceived as a three-level system of interaction.[4] At the first level, there is an interaction between a scientist and the external world, usually mediated through scientific instruments. The scientist (more often now a team of scientists) thus confronts the world and tries to make sense of it. This brings us to the second level of interaction between experiment and theory. The theory can be that of the phenomenon or of the instrument, or both. Through their practice, scientists face resistance that forces them to adapt theories and models as well as instruments.

These two types of interactions are, above all, exercises of self-persuasion (Perelman & Olbrechts-Tyteca, 1969, pp. 40–59), but the 'self' is already socialized through its prior training in the scientific field. At these two levels, scientific activity is already a socialized dialectical rationalism between 'the world' and 'reason'; that is, not an abstract intemporal and universal reason but a scientific reason that embodies the history of the field (Bourdieu, 1991).

Researchers stop experimenting when they think the experiment performs well according to the (already socially defined) standards of their science, and when they judge that they are ready to face their peers—that is, when they think they have arrived at results they can confidently defend (Galison, 1987). The choice of when to end an experiment is a matter of practical judgment. This judgment is facilitated by the basic culture of scientists, a culture learned and internalized through training in a given field (Ziman, 1968, pp. 34–35; Bourdieu, 1975). This is why scientists can anticipate criticism (Kitcher, 1991; Perelman & Olbrechts-Tyteca, 1969, pp. 31–34). In performing that anticipation, they already presuppose the validity of certain arguments to come; they adapt in advance to criteria of receivable and acceptable arguments, criteria that are the results of the history of the field.[5] Economic and political considerations can also play a role in the decision to stop experimenting and go public with a result.

Whatever the reasons leading to it, once the decision to stop an experiment is made, scientists present their arguments to other scientists at conferences and in (peer-reviewed) scientific journals. They are then acting on a third level of interaction, where they find the members of their discipline. This interaction between scientists is the key to understanding controversies. This process is guided by the prag-

[4] The idea comes partly from Winch (1994, p. 84), who argued that the scientific investigator is involved in two sets of relations: with the phenomena he investigates, and with his fellow-scientists and their rules.

[5] Collins (1998, pp. 293–294) offers a nice example of discussion by members of a group of scientists to publish their results as to whether or not, but he does not link this example to any general approach in terms of argumentation. Lynch (1990) also uses a very localized example of real-life argumentation but analyses it in a typical ethnomethodological framework. Of course, the micro-analysis of interactions can show the richness of such argumentations, but as in chess games, the very richness of each particular move should not be observed at the expense of the underlying rules of the game, which not only constrain legitimate moves but give them their global meaning.

matic rationality of experimental life: the confidence of the members of the field increases regarding experimental phenomena when important criticisms (be they technical or theoretical) are eliminated. Scientific knowledge is thus a set of arguments (experimental or theoretical) that have *survived* objections—with no absolute certainty that they will resist *future* attacks. The process is one of *establishment of belief* (Lenoir, 1988): arriving at a collective judgment and belief. Judgment is thus the collective activity of the members of the field (most often through some of its more recognized members). As Perelman & Olbrechts-Tyteca (1969, p. 513) put it:

> All language is the language of a community, be this a community bound by biological ties, or by the practice of a common discipline or technique. The terms used, their meaning, their definition, can only be understood in the context of the habits, ways of thought, methods, external circumstances, and traditions known to the users of those terms.

To arrive at a consensus on a scientific fact is always to arrive at a pragmatic and time-situated agreement that there are no further important objections (at this point in time) to be eliminated and that there is no better explanation offered which resists objections in the scientific field. In other words, scientists think that they have the best reasons to believe and the least reason to doubt (Shapere, 1991). And again, 'best' is something that is decided by the members of the field, for there is no external judge to impose any absolute sense of what is 'best' within the field.

The social control asserted by the scientific field over the individual researcher is an idea already well documented by early sociologists and philosophers of science. Bachelard proposed long ago 'to ground objectivity in the behavior of others' and that 'any doctrine of objectivity always ends up by submitting the knowledge of the object to the control of others' (Bachelard, 1972, p. 241). Paraphrasing Wittgenstein, we would say that there is no private science. Ceasing to exchange arguments (theoretical or experimental) is ceasing to do science. The process of constructing knowledge is a way of speaking about specific experiments before a limited and specially trained audience that is authorized to transform that discourse into knowledge (Overington, 1977, p. 144). The experimenter's task is to convince his peers through persuasion: he must propose to an audience 'good reasons' (again experimental or theoretical or, more usually, both, and 'good' as judged by the actors themselves) for believing in some phenomenon. Those reasons are themselves subjected to arguments, and only those that survive objections constitute accepted scientific knowledge (Perelman, 1963, p. 117).

5. Scientific practice and the ethics of argumentation

The scientific field functions on the basis of an implicit ethics of argumentation that constrains participants.[6] The dynamic of argumentation is usually guided by the

[6] On rules of argumentation, see Habermas (1979, 1991, 1992), Alexy (1991), Bohler (1991), Rescher (1997), Ullmann-Margalit (1983), Grice (1975), van Eemeren and Grootendorst (1984).

following sequence. First, there is a presumption in favor of known facts, that is, against the adversary of recognized knowledge. Perelman talks of a presumption for the normal or the plausible, generally defined by a social group (Perelman & Olbrechts-Tyteca, 1969, pp. 93–99); and Wittgenstein talks of things that cannot legitimately be doubted because they define our language-game: 'it belongs to the logic of our scientific investigations that certain things are indeed not doubted' (Wittgenstein, 1974, p. 342). Second, and because of the presumption of known facts, the burden of proof rests on the proponent of new facts. However, a presumption is also made in favor of the proponent: one does not demand irrefutable claims (although one demands extraordinary evidence for extraordinary claims), but only plausible hypotheses, hypotheses that appear true (because they fit into the cognitive scheme of the group). Reasonableness means that propositions should be adequately conclusive. That means that they should be as certain as can reasonably be accepted in the circumstances, and that all due care and caution should have been exercised to establish them. Thus, to assert a proposition in a given field at a given time is to assert not an absolute truth but a *plausible* candidate for truth, as suggested by mitigated skepticism.

Since no one can control all areas of activity, judgment as to what is admissible as scientific knowledge depends on networks of responsible and authoritative critics held together by trust in each other's judgment (Hardwig, 1991; Shapin, 1994). For this reason, the proponent's argument is considered (provisionally) valid until a reply is made. In the absence of reply, the proponent receives favorable judgment. Opponents always have, in principle, the right to reply using the legitimate tools of the field: empirical evidence, experimentation, logical and mathematical reasoning. Finally, a dispute ends when closure is reached, that is, when no more (important) objections are raised: a winning position is reached by the proponent when most objections have been discarded (Rescher, 1977, p. 43). Debate can also end simply when someone central to a dispute dies and the rest of the group is simply, and for whatever reasons, no more interested in the topic of the debate.[7] It can also stagnate for lack of new resources and lead to a stalemate.[8]

Though this dynamic is a practical matter going on in the day to day activity of a scientific field, it has been usefully summarized by Pera (1991):

[7] For an analysis of different kinds of closure, see the introduction and the contributions by Tom L. Beauchamp and Ernan McMullin in Engelhardt and Caplan (1987).

[8] By concentrating here on argumentation, we do not mean that economic, political or institutional factors do not affect this dynamic, but simply that their role has been well investigated in the past by sociologists of science and can thus be taken for granted here for the sake of a general presentation of the central role of argumentation in scientific practice. Only particular case studies can aim to present the various and changing ways in which these factors interact. For examples, see Engelhardt and Caplan (1987). Collins' (Collins, 1999) analysis of the varying levels of interest in Weber's paper over time fits very well with our model, but his notion of 'layers' of the scientific community should be replaced by an analysis of the interactions between different and relatively autonomous fields having their own structure and logic, in order to recognise that not every actor can move from one field (science) to another (politics or administration, for example). On this, see Bourdieu (1975).

- a scientific argument is pertinent if the reasons advanced belong to the substantive factors, that is to accepted facts and theories;
- a scientific argument is valid if, on the basis of the substantive factors, it forces others to assent, remain silent or withdraw;
- a scientific debate terminates when other participants can no longer argue.

Though philosophers call these criteria ‘epistemic’, they in fact simply summarize and formalize what can be observed to be ‘rules of the game’ that have developed historically in the scientific field (Bourdieu, 1975, 1991) and as such they are fundamentally *sociological* rules of interactions in the field. What Franklin calls ‘epistemological criteria’ are in fact rules of argumentation that have evolved over time.[9] And here it is worth noting that we are not talking, as many constructivists often do, about what *could* have been argued against a given position but never in fact was, but about objections effectively raised by actors themselves active in the field at a given time. It is these arguments that form the basis of dynamic change in scientific fields.

The argumentation approach suggests that knowledge consists of ‘opinions’ which have survived objections (Perelman, 1963, p. 117). Thus, statements contained in articles become knowledge only if they are neither ignored nor contradicted (Ziman, 1968). The scientific field is the specific space of these intersubjective interactions in which experiments and calculations are submitted to critical investigation (Popper, 1962). Scientific practice involves argumentation before an audience that has rules. It is not subjective evidence (*a priori*, innate or private) that constitutes knowledge, but collective evidence mediated by ethical norms of communication—a common language, a community, and a principle of equality of participation for those who, through appropriate training, have gained access to the field.[10] As Apel has noted: ‘The precondition of mutual participation in a language-game replaces methodological solipsism’ (Apel, 1980, p. 33).

6. Conclusion

The discussions around Collins’ experimenters’ regress are in fact a new version of the old skeptical debate. First expressed in Sextus’ concerns about the standards of truth, then in Montaigne’s discussion of the circular relation between demonstration and instrument, we also find the tradition of mitigated skepticism in Karl Popper’s treatment of the problem of infinite regress:

[9] The case of medical experiments which moved from simple testing to control group and to placebo group provides a relatively recent example of how research methods evolve in time under pressure and in response to arguments and criticisms. See Matthews (1995) and McIntosh (1991).

[10] This principle is a regulative ideal for, in practice, there is always a social hierarchy which defines the relative legitimacy of the various positions taken by the actors. In other words, credibility is not evenly distributed among agents (and institutions) in the field.

> The empirical basis of objective science has nothing absolute about it . . . It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or given base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being. (Popper, 1959, p. 111)

Admitting that the 'basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of dogmas', he added that 'this kind of dogmatism is innocuous since, should the need arise, these statements can easily be tested further' (Popper, 1959, p. 105).[11]

And then came Collins, going back to a radical skepticism that, as Pascal suggested, is only the mirror image of the old Aristotelian and Cartesian belief in the possibility of absolute knowledge. But in so doing, he was in fact accepting some of the implicit arguments of his philosophical critics, which oppose 'epistemological' to 'social' criteria, as if argumentation in a scientific field was not an eminently social practice. Hence, summarizing the debate around Weber's work, he writes that:

> The list of 'non-scientific' reasons that scientists offered for their belief or disbelief in the results of Weber's and others' work reveals the lack of an 'objective' criterion of excellence. There is, then, no set of 'scientific criteria' which can establish the validity of findings in the field. The experimenters' regress leads scientists to reach for *other criteria* of quality. (Collins, 1992, pp. 87–88; our emphasis)

Placing himself on the same terrain as the philosopher, Collins seems to oppose social reasons to scientific reasons, whereas one can transcend such a ruinous opposition (accepted by philosophers) by seeing that argumentation is an essentially social practice inside a scientific field that is, the product of previous history (that is, of previous argumentation and experimentation). There is no point in talking about 'non-scientific' criteria or the search for 'one' objective criterion (except in the case of obviously non-cognitive and non argumentative aspects, like overtly political or religious statements, which in fact would be denounced by actors themselves as illegitimate).

Of course, by using scare quotes around 'scientific criteria', Collins suggests his distance from the notion, but in talking about 'other criteria' he nonetheless implicitly accepts the dichotomy he thinks he rejects. Thus, admitting that 'high theoretical arguments' can be the thing that closes a controversy he adds (as if only to make philosophers nervous) that it can *also* be closed by 'dirty tricks', thus giving the impression that *both* arguments are *equally* possible or credible (Collins, 1994, p.

[11] For those who might be surprised to see us connecting Popper with sociology of science, it may be useful to remind that Barry Barnes wrote that Popper's *Logic of scientific discovery* contains 'many arguments which could be used to support a sociological treatment of scientific knowledge', though Popper himself 'neither suggested nor stimulated such an enterprise' (Barnes, 1974, p. 179).

502). We suggest, in contrast, that in highly codified fields such as physics, *legitimate* arguments (that is, legitimate from the point of view of the community members, not from the point of view of philosophers or sociologists) are severely constrained by the past history of the field. And it is significant that in mentioning 'dirty tricks' Collins refers to parapsychology, clearly a field whose history and present state is radically different from that of contemporary gravity wave physics. Moreover, we think that this implicit acceptance of a dichotomy is an effect of a discourse constructed in a context of argumentation with (albeit against) philosophers. We suggest that once we bracket off the effects of radicality aimed at exciting philosophers of science, Collins' empirical case studies are best interpreted within a framework centered on argumentation theory which is not handicapped by any implicit radical skepticism. For without that implicit dichotomy between the rational and the social (which by the way reproduces the disciplinary dichotomy between philosophy and sociology), sociologists of science can content themselves with using the sociological concepts and tools of their trade instead of being trapped in philosophical debates for which they are often ill equipped and whose effects are to limit and confuse sociological analysis.

It may be an irony that we have had to go back to an old *philosophical* tradition beginning with Greek skepticism to see that there is no reason for any reference to 'logic' and 'argumentation' to be taken as *epistemological* rather than *sociological*. Through this route, we have seen that the debate between Collins and his philosophical opponents only makes sense because *both* camps accept this opposition, whereas Collins' empirical descriptions and analysis of scientific debates make perfect sense when resituated within the dynamic of a scientific field constrained by rules which are themselves the product of a past history of experimentation, calculation and argumentation.

References

Alexy, R. (1991). A theory of practical discourse. In N. Benhabib & F. Dallmayr (Eds.), *The communicative ethics controversy* (pp. 151–190). Cambridge, MA: MIT Press.

Apel, K.-O. (1980). *Towards a transformation of philosophy*. London: Routledge.

Bachelard, G. (1972). *La formation de l'esprit scientifique*. Paris: J. Vrin. First published 1938.

Barnes, B. (1974). *Scientific knowledge and sociological theory*. London: Routledge and Kegan Paul.

Bohler, D. (1991). Transcendental pragmatics and critical morality: On the possibility and moral significance of a self-enlightenment of reason. In S. Benhabib & F. Dallmayr (Eds.), *The communicative ethics controversy* (pp. 111–150). Cambridge, MA: MIT Press.

Bourdieu, P. (1975). The scientific field and the social conditions for the progress of reason. *Social Science Information*, *14*(6), 19–47.

Bourdieu, P. (1991). The peculiar history of scientific reason. *Sociological Forum*, *6*, 3–26.

Cavell, S. (1979). *The claim of reason: Wittgenstein, skepticism, morality and tragedy*. Oxford: Clarendon Press.

Collins, H. M. (1975). The seven sexes: A study in the sociology of a phenomenon or the replication of experiments in physics. *Sociology*, *9*, 205–224.

Collins, H. M. (1992). *Changing order: Replication and induction in scientific practice*. Chicago: University of Chicago Press. First published 1985, Beverley Hills & London: Sage.

Collins, H. M. (1994). A strong confirmation of the experimenters' regress. *Studies in History and Philosophy of Science*, *25*(3), 493–503.
Collins, H. M. (1998). The meaning of data: Open and closed evidential cultures in the search for gravitational waves. *American Journal of Sociology*, *104*(2), 293–337.
Collins, H. M. (1999). Tantalus and the aliens: Publications, audiences and the search for gravitational waves. *Social Studies of Science*, *29*(2), 163–197.
Culp, S. (1995). Objectivity in experimental inquiry: Breaking data-technique circles. *Philosophy of Science*, *62*, 430–450.
Dear, P. (1984). Marin Mersenne and the probabilistic roots of mitigated skepticism. *Journal of the History of Philosophy*, *22*(2), 173–205.
Engelhardt, H. T. Jr, & Caplan, A. L. (Eds.). (1987). *Scientific controversies: Case studies in the resolution and closure of disputes in science and technology*. Cambridge: Cambridge University Press.
Franklin, A. (1994). How to avoid the experimenter's regress. *Studies in History and Philosophy of Science*, *25*, 463–491.
Franklin, A. (1995). The appearance and disappearance of the 17 Kev Neutrino. *Review of Modern Physics*, *67*(2), 457–490.
Galison, P. (1987). *How experiments end*. Chicago: University of Chicago Press.
Gingras, Y. (1995). Following scientists through society? Yes, but at arm's length! In J. Buchwald (Ed.), *Scientific practice* (pp. 123–148). Chicago: University of Chicago Press.
Gingras, Y., & Godin, B. (1997). Expérimentation, instrumentation et argumentation. *Didaskalia*, *11*, 149–160.
Grice, H. P. (1975). Logic and conversation. In D. Davidson (Ed.), *The logic of grammar* (pp. 64–75). California: Dickenson.
Groarke, L. (1990). *Greek scepticism: Anti-realist trends in ancient thought*. Montreal: McGill-Queen's University Press.
Habermas, J. (1992). *De l'ethique de la discussion*. Paris: Cerf.
Habermas, J. (1979). What is Universal Pragmatics? In *Communication and the evolution of society* (pp. 1–68). Boston: Beacon Press.
Habermas, J. (1991). *Morale et communication*. Paris: Cerf.
Hardwig, J. (1991). The role of trust in knowledge. *Journal of Philosophy*, *88*(12), 693–708.
Hesse, M. (1986). Changing concepts and stable order. *Social Studies of Science*, *16*, 714–726.
Kitcher, P. (1991). Persuasion. In M. Pera & W. R. Shea (Eds.), *Persuading science: The art of scientific rhetoric* (pp. 3–27). Canton: Science History Publications.
Larmore, C. (1998). Skepticism. In D. Garber & M. Ayers (Eds.), *Cambridge history of seventeenth-century philosophy* (Vol. 2, pp. 1145–1192). Cambridge: Cambridge University Press.
Lenoir, T. (1988). Practice, reason, context: The dialogue between theory and experiment. *Science in Context*, *2*, 3–22.
Lynch, M. (1990). Allan Franklin's transcendental physics. *PSA 1990*, *2*, 471–485.
McIntosh, H. (1991). 1971–1991: Clinical trials evolved to become rigorous scientific testing ground. *Journal of the National Cancer Institute*, *83*(10), 668–670.
Matthews, J. R. (1995). *Quantification and the quest for medical certainty*. Princeton, NJ: Princeton University Press.
Milton, J. R. (1987). Induction before Hume. *British Journal of Philosophy of Science*, *38*, 49–74.
Montaigne, M. (1938). *Essays*. London: J. M. Dent. First published 1580.
Overington, M. A. (1977). The scientific community as audience: Toward a rhetorical analysis of science. *Philosophy and Rhetoric*, *10*(3), 143–164.
Pascal, B. (1958). *Pensées*. New York: E. P. Dutton.
Pera, M. (1991). The role and value of rhetoric in science. In M. Pera & W. R. Shea (Eds.), *Persuading science: The art of scientific rhetoric* (pp. 29–54). Canton: Science History Publications.
Perelman, C. (1963). Self-evidence and proof. In C. Perelman (Ed.), *The idea of justice and the problem of argument* (pp. 109–124). London: Routledge.
Perelman, C., & Olbrechts-Tyteca, L. (1969). *The new rhetoric: A treatise on argumentation*. Notre Dame/London: University of Notre Dame Press. First published in 1958.

Pinch, T. J., & Bijker, W. E. (1984). The social construction of facts and artifacts. *Social Studies in Science*, *14*, 399–441.

Pinch, T. (1986). *Confronting nature: The sociology of solar-neutrino detection*. Dordrecht: Reidel.

Popkin, R. H. (1979). *The history of skepticism from Erasmus to Spinoza*. Berkeley: University of California Press.

Popper, K. R. (1959). *The logic of scientific discovery*. New York: Harper and Row.

Popper, K. R. (1962). *Conjectures and refutations*. New York: Harper and Row.

Rescher, N. (1977). *Dialectics: A controversy-oriented approach to the theory of knowledge*. Albany: State University of New York.

Sextus Empiricus (1933). *Sextus Empiricus*, with an English translation by R. G. Bury, 4 vols. Vol. 1: *Outlines of Pyrrhonism*. Cambridge, MA: Harvard University Press.

Shapere, D. (1991). On deciding what to believe and how to talk about nature. In M. Pera, & W. R. Shea (Eds.), *Persuading science: The art of scientific rhetoric* (pp. 89–103). Canton: Science History Publications.

Shapin, S. (1994). *A social history of truth: Civility and science in seventeeth-century England*. Chicago: University of Chicago Press.

Ullmann-Margalit, E. (1983). On presumption. *Journal of Philosophy*, *80*(3), 143–163.

van Eemeren, F. H., & Grootendorst, R. (1984). *Speech acts in argumentative discussion*. Dordrecht: Foris Publications.

Winch, P. (1994). *The idea of a social science and its relation to philosophy*. London: Routledge. First published 1958.

Wittgenstein, L. (1974). In G. E. M. Anscombe & G. H. von Wright (Eds.), *On certainty*. Oxford: Basil Blackwell. First published 1958.

Ziman, J. (1968). *Public knowledge: The social dimension of science*. Cambridge: Cambridge University Press.