

# Scientists' Argumentative Reasoning

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**Abstract** Reasoning, defined as the production and evaluation of reasons, is a central process in science. The dominant view of reasoning, both in the psychology of reasoning and in the psychology of science, is of a mechanism with an asocial function: bettering the beliefs of the lone reasoner. Many observations, however, are difficult to reconcile with this view of reasoning; in particular, reasoning systematically searches for reasons that support the reasoner's initial beliefs, and it only evaluates these reasons cursorily. By contrast, reasoners are well able to evaluate others' reasons: accepting strong arguments and rejecting weak ones. The argumentative theory of reasoning accounts for these traits of reasoning by postulating that the evolved function of reasoning is to argue: to find arguments to convince others and to change one's mind when confronted with good arguments. Scientific reasoning, however, is often described as being at odds with such an argumentative mechanisms: scientists are supposed to reason objectively on their own, and to be pigheaded when their theories are challenged, even by good arguments. In this article, we review evidence showing that scientists, when reasoning, are subject to the same biases as are lay people while being able to change their mind when confronted with good arguments. We conclude that the argumentative theory of reasoning explains well key features of scientists' reasoning and that differences in the way

scientists and laypeople reason result from the institutional framework of science.

**Keywords** Science · Reasoning · Argumentation

## 1 Introduction

Scientists reason. Reasoning, we claim, is a psychological ability meant to deal with social interactions. More precisely, reasoning is geared towards convincing others of the truth of some proposition and being convinced only by good arguments. It is geared towards argumentation. We will show that these claims about the evolved function of reasoning, subsumed under the argumentative theory of reasoning (Mercier and Sperber 2011a), are well supported by historical and psychological evidence from science studies. Reciprocally, we will hint at new insights the argumentative theory of reasoning could bring to our understanding of scientific practices and their social aspects.

The social view of reasoning put forward by the argumentative theory seems at first hard to reconcile with the typical understanding of the place of reasoning in science. More often than not, the social aspects of scientific knowledge production have been decried as forces working against rationality.<sup>1</sup> Rationality, by contrast, is equated

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<sup>1</sup> Barnes et al. (1996) have done much arguing against this presupposition, which favors a normative analysis of scientific knowledge production and prevent a naturalistic one. These authors also often accuse their detractors of making this assumption. For instance, Bloor (1997) answering Cole (1995). Note also that, in the same article, Bloor looks at the psychological origin of biases and the social dynamic that leads to cancelling out individual biases: a point elaborated within the argumentative theory of reasoning (Sperber and Mercier 2012).

with the thinking of the lone scientist. Scientists might have inherited the problem they deal with from others; they might have social interests; but ultimately it is through reasoning, with its power to uncover the truth, that scientists devise new theories. Yet the dichotomy that separates, on the one hand, the social and the contextual phenomena and, on the other hand, the individual and epistemic facts, has long been decried (Bloor 1976). Decades of sociology of science have shown that science is an enterprise that involves social interactions in non-trivial ways: the distribution of labor (e.g. Giere 2002), the historical construction of epistemic standards (e.g. Knorr Cetina 1991), and the collective assessment of theories (e.g. Latour 1993) are cases in point. But reasoning remains the last and most important bastion of individualism in epistemology and science studies.<sup>2</sup> When we assert that the function of reasoning is argumentative, we give to social interactions a key role in reasoning. Reasoning is something that happens in the individual mind, yet, it is a cognitive ability that is geared for dealing with social interactions. How does reasoning's argumentative function play out in the context of science? And what are the consequences for science, its practices, its institutions, and its epistemic status?

Psychologists of science have looked at the social embedding of scientific thinking: ideas of great scientists are generated in, and on the basis of, a rich social context (e.g. Osbeck et al. 2013). We claim that theories—or at least the justifications that support them—are not only developed *from* others' ideas and values, but also *for* others, within an argumentative dynamic. Thus, scientists' individual reasoning is more related to social interactions than what psychologists of science have acknowledged. Conversely, sociologists of science have been reluctant to consider individual cognition. Some have feared that integrating the psychology of reasoning into their work would be tantamount to a return to non-naturalistic rational redescriptions of belief formation. Rational redescription is a normative approach, which consists in specifying what reasons *should* have been considered and which rational inferences should have been made (Lakatos 1970). Our approach avoids this pitfall by remaining naturalistic throughout: we specify the role of reasoning in science as a psychological process. As such, we hope it can contribute to bridging the gap between psychology and sociology of science.

Our paper proceeds as follow. We start by offering a definition of reasoning and contrasting an individualistic to a social view of reasoning. In particular, we outline a theory that makes of reasoning a mechanism aimed at

argumentation. This argumentative theory of reasoning makes several predictions regarding the traits and functioning of reasoning. In several cases, these predictions clash with common beliefs about the way scientists reason. Sections 3, 4, 5 and 6 review these predictions and gather evidence showing that scientists' reasoning skills are essentially the same as everyone else's. We also point to some significant differences in the efficacy of reasoning in science compared to other applications of reasoning and suggest that they are due to differences in the social context in which science takes place rather than to any intrinsic particularity in scientists' reasoning.

## 2 Characterizing Scientific Reasoning

### 2.1 What Reasoning Is

While most people agree about the importance of reasoning in our mental lives, the type of cognitive mechanism scholars call 'reasoning' varies widely. Some use 'reasoning' to refer to just about any inferential mechanism of 'higher', non-perceptual, cognition, others use the term to describe explicit, conscious thinking (e.g. Kahneman 2003). In the psychology of science, reasoning can refer to any of the mental activities of scientists, especially given that these psychological processes are presumably truth oriented and satisfy some rationality criteria. Reasoning can also denote a subset of cognitive processes that satisfies some epistemic criteria—inductive reasoning as described logical positivists, for instance. The norm of rationality is here what operates the distinction between reasoning and other activities. But some more recent uses of the term 'scientific reasoning' are of a clearly descriptive nature. Model-based reasoning, for instance, is the label used to describe a specific category of cognitive processes—making use of a model to derive information about a target domain—that is shown to play an important role in science (Magnani and Nersessian 2002).

For the purposes of this article, we would like to identify a specific cognitive activity that is close to what we commonly understand as 'reasoning.' We use reasoning to refer to inferential mechanisms that pay attention to, and produce, reasons (see, Mercier and Sperber 2011a). This characterization of reasoning has the advantage of distinguishing intuitive inferences and inferences based on reasons, grasping an important aspect of human cognitive processes. Cognitive science has shown that we continuously perform complex inferences without necessarily being aware of it or having considered reasons for doing so. For instance, when John tells Paula "It's going to rain," Paula infers that it is going to rain where John is. Crucially, she doesn't have to go through reasons to draw this

<sup>2</sup> This points reflects the more general debate in sociology between social determination and rational agency. The role of reasoning is made very explicit in Boudon (1995).

inference; she doesn't have to think "If John had wanted me to understand it was going to rain somewhere else, he would have said so, so it must be going to rain where he is." Such inferential mechanisms, which do not rely on reasons, perform 'intuitive inferences' or, for short, intuitions. By contrast, if John had said "It's going to rain because the weather report said so," he would have offered Paula a reason (the weather report said so) for accepting a conclusion (it's going to rain). When John finds this reason, and when Paula evaluates it, they do not engage only in intuitive inference. They actively consider reasons, engaging in what can be called 'reflective inference' or *reasoning*. Reasoning thus characterized is a metarepresentational mechanism with a specific function: finding and evaluating reasons that bear on a conclusion's value.

Most scientific cognition is not reasoning in our restricted use. In particular, scientific model-based reasoning includes inferences produced by cognizing the model. Such inferences need not be reflective. For instance, a scientist might use a drawing to represent a complex phenomenon. The aspects of the drawing might generate intuitive inferences that are then, possibly reflectively, applied to solve the problem at hand (Nersessian 2008, chap. 3). Which cognitive processes are intuitive and which are reflective is an interesting question that is better asked once we have identified reasoning 'proper' among the multiple types of human inferences.

Evans and others (Evans 2003; Sloman 1996; Stanovich 2004) assert that reasoning corresponds to conscious, explicit, effortful, and slow thinking—it forms the basis of dual-process theories of reasoning. However, many cognitive processes that share these traits are not reasoning as we characterized it. For instance, one can use perception in a conscious, effortful manner—when trying to perceive some dim astronomical phenomenon, for instance. It is also better to distinguish reasoning from other conscious, explicit mechanisms that share some of its properties, such as some forms of planning or theory of mind (Mercier and Sperber 2011b).

Even though scientific cognition is not all reasoning (by our definition), even in this restricted view reasoning plays a very important role in science. Most scientific beliefs are held because of reasons. Indeed, one striking aspect of science is that it goes beyond intuitive beliefs—beliefs held primarily thanks to intuitive mechanisms. Illustrations abound: we intuit that the earth is flat and not moving, or that matter is dense. At the same time, we have reasons to think that the earth is spherically shaped, that it moves around the sun at 30 km per second and that matter is mostly void. Such beliefs arise because scientific practices include the development of reflective beliefs, i.e. propositions that are believed to be true because we have reasons to do so (Sperber 1997). Note that saying that most

scientific beliefs are reflective beliefs does not mean that intuitive inferences play little role in scientific thinking, it only means that some intuitions are not taken at face value. In fact, developing reflective beliefs is a cognitive process made of numerous intuitive inferences—including the intuitive dimension of reasoning itself, the intuition that a given representation is a good reason to hold another representation (Mercier and Heintz 2013). The important point for our argument, however, is that scientific cognitive practices, however rich and multiple, however anchored in, or scaffolded upon, intuitive beliefs (Heintz 2013), include a systematic appeal to reasons.

## 2.2 Individualistic and Social Perspectives on Reasoning

The study of reasoning has been dominated by what can be dubbed the 'classical view of reasoning'. Explicitly or implicitly, philosophers and psychologists generally treat reasoning as a mechanism that aims at improving the reasoner's beliefs and decisions, usually by examining the reasoner's own reasons in order to discard unjustified beliefs and not to make poorly supported decisions. While Descartes was the strongest proponent of the classical view, we find it more recently in the work of cognitive psychologists such as Daniel Kahneman (2003), for whom "one of the functions of System 2 [reasoning] is to monitor the quality of both mental operations and overt behavior" (p. 699).

This classical view of reasoning is related to the standard view of science in the form of the 'lonely genius'. According to this view, science—at least the most important aspect of it—is a solitary endeavor: when scientists aren't conducting experiments, they engage in intense but lonely reflection. This is especially true of the 'geniuses' such as Newton, Darwin or Einstein. Stephen Shapin has traced the origins of this trope—the solitary thinker—to Greek thought and demonstrated its importance in the popular view of scientists since the scientific revolution (Shapin 1991). These brilliant thinkers seemingly escape the biased and superficial nature of everyday reasoning not by arguing together, but through intense, focused ratiocination.

Decades of sociology of science have shown that this view is erroneous: most scientific practices make sense only within the social context in which they take place; and all scientific achievements result from the efforts of many people. But despite the weight of social studies of science, scientific reasoning seems to resist the invasion of sociological analysis. On the science war front, both camps see reason as the ultimate place that is safe from sociological analysis. Defenders of the rationality of science against relativism could put rationality just there: in scientists' reasoning capacities. Protagonists on the other side of the

front have seen in reasoning another attempt to resist naturalistic inquiries. Reasoning, understood as the last stronghold of rationality, is just a philosophical myth to be disregarded altogether. Latour provides a good example of arguments along those lines.<sup>3</sup> However, whether this criticism is right or not, it can only target a normative view of reasoning, not a naturalistic one. Unfortunately, when Latour refuses to consider reasoning, he does not save, but hinders the naturalistic enquiry of a key mental and social practice in science: producing and evaluating reasons.

The classical view of reasoning is not only incompatible with the conclusions of sociologists of science, but also with psychological data. More often than not, reasoning does not do what the classical view would have it do, as it fails to critically examine our reasons. Instead reasoning looks for confirmatory evidence and disregards contradictory evidence of one's prior belief (myside bias<sup>4</sup>). Moreover, reasoning satisfies itself with shallow justifications and weak arguments. As a result, reasoning often bolsters the reasoner's beliefs, whether they are right or wrong, rather than correct them (references for all these claims will be provided as they are explored in the context of scientific reasoning).

To account for these puzzling findings, Hugo Mercier and Dan Sperber have formulated a different hypothesis about the function of reasoning (Mercier and Sperber 2011a; Sperber 2001). Reasoning didn't evolve to serve the solitary reasoner; it evolved to argue: to find arguments in order to convince others and to evaluate others' arguments in order to decide if one should change one's mind. This argumentative theory of reasoning accounts for reasoning's most striking traits (Mercier 2013). The myside bias, observed low investment in evaluating one's own arguments, and several other aspects of reasoning are, as we will show, consequences of the first function of reasoning: convincing an audience. The other function of reasoning is to evaluate others' arguments so as to update one's beliefs

<sup>3</sup> "In order to reach that aim [developing a naturalistic anthropology of science], we have to abandon many intermediary beliefs: belief in [...] the power of reason" (Latour 1993: p. 150). In the context of the 'science war,' where scientists opposed some theories of science and conceptual methods in social studies of science, Latour points out what he takes to be the misguided assumptions of his contradictors: "I quickly unearthed what appeared to me to be a fundamental presupposition of those who reject 'social' explanations of science. This is the assumption that force is different in kind to reason; right can never be reduce to reason" (p. 153). By contrast, we stick with the old fashioned idea that there is a difference between being constrained by physical force and being convinced by the force of an argument. The latter, however, can also be studied in a naturalistic way with cognitive science.

<sup>4</sup> We prefer to use "myside bias" rather than the more commonly used "confirmation bias" because reasoning does not have a bias to confirm everything, but rather to find reasons that support the reasoner's side.

only when there are good reasons to do so. We will show that we are quite good at telling apart good from bad arguments, when these arguments are put forward by others. As a result, when two people argue, if one person is wrong and the other is right, the latter is more likely to convince the former than the other way around. This means that groups tend to outperform individuals on reasoning tasks, sometimes by a wide margin. The first goal of this article is to show that these properties of reasoning, well documented among laypeople, also apply to reasoning in science. A secondary goal is to specify the social and institutional contexts that make scientific thinking nonetheless as successful as we know it. In this essay, we especially focus on the first goal, even though we briefly broach the second.

### 3 The Myside Bias

#### 3.1 How to Demonstrate a Genuine Myside Bias

One of the most distinctive traits of the argumentative theory is to account for the myside bias as an adaptive feature of reasoning. When reasoning produces arguments to convince an audience, it is adaptive to mostly find arguments that support the reasoner's position. Our goal in this section is to specify what constitutes a genuine myside bias and to show that scientists have such a bias when they reason to produce arguments.

In his review of the topic, Nickerson defines the myside bias as the "seeking or interpreting of evidence in ways that are partial to existing beliefs, expectations, or a hypothesis in hand" (Nickerson 1998, p. 175). He cites dozens of experiments purporting to demonstrate the existence of such a bias, including some experiments conducted with scientists.

However, we do not want to lightly accuse scientists of being partial or biased before checking whether it is not just a feature of some rational (in the normative sense) cognitive process. And indeed, the interpretation of many of the results cited by Nickerson has been disputed. In particular, many findings can be interpreted not as resulting from biases, but as the consequences of epistemically sound Bayesian inferences with very strong prior beliefs. The essence of a Bayesian inference is to take a prior belief (also known as base rate) into account and update it with new information. It has been argued that Bayesian inference well describes scientific reasoning (Horwich 1982; Howson and Urbach 2005). Numerous psychology experiments have been used to criticize both lay people and experts' failure to take prior beliefs into account when estimating the probability of an event. But taking priors into account can also look like a myside bias. For instance, as pointed out by Koehler (1993), if a scientist's assessment of, say, an article, is tainted by her

prior views on the topic, this is typically understood as an indictment of her objectivity. However, in a Bayesian framework, it is sensible to give little weight to a result that seems implausible based on one's priors: the result is more likely to be bogus than the prior belief is likely to be false.

If a given behavior can be easily explained using a standard normative framework such as Bayesianism, it is uneconomical to invoke the *myside bias*. Accordingly, a first trademark of the genuine *myside bias* is that it cannot be accounted within such normative frameworks, at least if one considers an individual in isolation. Moreover, by violating normative standards, a genuine *myside bias* often has epistemically deleterious consequences, such as strengthening one's beliefs with no rational reason.

In spite of these caveats making the *myside bias* relatively difficult to demonstrate, the experimental evidence indicates that the *myside bias* is a robust and prevalent feature of reasoning. Mercier and Sperber (2011a) have reviewed the evidence, at least concerning lay people. In the next section, we focus on evidence related scientific reasoning.

### 3.2 Myside Bias Among Scientists

In light of these considerations, what evidence is there of a *myside bias* in scientists' reasoning? Here we consider three types of evidence: experimental, ethnological and historical.

Michael Mahoney conducted some early experiments on scientists' *myside bias*. One relied on the 2–4–6, a standard task used to establish the existence of a *myside bias*, comparing the behavior of physical scientists, psychologists and ministers (Mahoney and DeMonbreun 1977).<sup>5</sup> Both groups of scientists behaved like typical participants; only the ministers seemed better able to question their own hypotheses. The scientists rarely put forward tests that they thought would support their hypotheses; they were even very likely to stick to falsified hypotheses. As a result, fewer than 40 % of them were able to arrive at the correct rule, a result that is hard to account for within any normative framework, given the simplicity of the task.

The limitation of this experiment is that it lies outside the scientists' field of expertise. Demonstrations of *myside bias* when scientists reason about their own field are more pertinent. Mahoney (1977) also offered such a demonstration by having psychologists review a fake article that either supported the scientists' theories or not. The psychologists were much more critical when the article's result opposed their favored theory. Koehler (1993) obtained

similar results while specifying that such apparently biased evaluations were coherent with a Bayesian interpretation. However, Koehler also noted several elements strongly suggesting that a genuine *myside bias* was at play. For instance, in the Mahoney experiment the reviewers rated even something as neutral as data presentation better when the article's conclusion supported their views. In the Koehler experiment, the difference in the ratings between an article that supported or did not support the scientist's view grew stronger as the criteria grew more malleable, suggesting that scientists were using the wiggle room offered by soft criteria to justify rejecting articles with undesirable conclusions.

Some ethnographic data also points towards the presence of a *myside bias* in scientists' reasoning. Particularly revealing is Kevin Dunbar's study of scientists dealing with unexpected results. An unexpected result should be more interesting than an expected result, since it carries more information. However, reasoning as an argumentative capacity should have a peculiar way of paying attention to unexpected information, at least when it flouts the reasoner's argumentative goals: reasoning should find reasons to disregard inconvenient information. Some of Dunbar's observations show exactly that: "individual scientists ... usually attributed inconsistent evidence to error of some sort and hoped that the findings would go away" (Dunbar 1995, p. 380). In a more extensive analysis of this phenomenon, Fugelsang et al. (2004) report that this strategy of blaming inconsistent results on technical problems was resorted to in nearly 90 % of the cases. The Duhem–Quine thesis explains that this strategy can always be appealed to without being inconsistent. It can also be a rational, Bayesian, strategy if the prior belief in the truth of the tested hypothesis is very high. However, it is unlikely that the tests used by scientists have such a high average of false negative, and the opinion of other scientists on the same result suggests that some rationalizing is at play, as we will see later.

Dunbar notes that scientists can also react in a more rational manner, for instance when an unexpected result only requires a minimal change in their hypotheses to be accommodated (Dunbar 1995). More interestingly, scientists also take unexpected results in stride when they are observed early in a research program and when they challenge core hypotheses of their field (Dunbar 1997). This makes sense if one considers that only in a few cases will such core hypotheses 'belong' to the scientists in the sense that scientists have a high stake in proving them right. On the contrary, scientists can greatly benefit from mounting a credible challenge to such core hypotheses. Finally, Dunbar notes that senior scientists are less likely than their junior colleagues to discount inconvenient results. We will come back in the next section to this apparent attenuation of the *myside bias*.

<sup>5</sup> The 2–4–6 does not provide a straightforward demonstration of the *myside bias*, but it can still be interpreted as strongly suggesting its presence (see, Klayman and Ha 1987; Mercier and Sperber 2011a).

Many episodes in the history of science can be interpreted as indicating a myside bias. A good way to find such episodes is to look for egregious breaches of normative standards of rationality that further a scientist's argumentative aims. For instance, when Linus Pauling was embroiled in a battle over the efficacy of vitamin C as a cure for cancer, he tried to discredit his opponents' clinical trial. To do so, he published a mathematical model of the requirements for a sound clinical trial in cancer research (Pauling and Herman 1989). The only study to fall short of these standards was a recent and widely publicized failure to find any link between vitamin C and remissions in cancer patients. The fact that Pauling's model singled out, out of thousands of clinical trial, the study that had done the most to weaken his position is very unlikely to be the result of an unbiased process. Crucially, Pauling's case is only relevant to the point in hand if the bias was unconscious: if he believed he was doing his best to establish what is the truly case. While this matter is obviously difficult to settle definitively, Pauling's private behavior suggests that he genuinely believed in the efficacy of vitamin C and in the validity of his efforts to prove as much (Collins and Pinch 2005).

Social historians of science have also analyzed the potential biases occurring when scientists side with one of two competing theories during scientific controversies (e.g. Pickering 1980). As part of their work, scientists have to judge what are the important data and what data can be disregarded, and they have to decide what is a reliable method and what is not. Thus, methods and evaluative criteria are sometimes forged at the same time as they are being used for telling apart empirical claims. This provides scientists with wiggle room for the interpretation and selection of evidence. In this context, scientists are biased to the extent that they favor the methods and criteria that help supporting their already held beliefs. But, the argument goes, in the absence of independent and foundational scientific method, they are nearly sure to do so.

The scope of this type of circularity between method selection and theory choice is disputable, since methodological and epistemological beliefs are typically more entrenched than scientific empirical beliefs. Yet, numerous historical studies show the biased reliance on advantageous, from the argumentative point of view, interpretative framework. For instance, during the controversy between Millikan and Ehrenhafts over the elementary electrical charge, Millikan selected out many observations that Ehrenhafts would have kept (Holton 1978). Millikan claimed there was an elementary electric charge and measured it to be  $4.774 (\pm 0.009) \times 10^{10}$  esu (unit of electric charge, statcoulomb); Ehrenhafts claimed, on the contrary, that there was smaller electric charges. Millikan published his oil drop experiment in 1908: it provided a

good experiment for measuring the elementary electric charge. But in 1910, Ehrenhaft reported having found electric charges ranging from  $7.53 \times 10^{10}$  esu down to  $1.38 \times 10^{10}$  esu. Ehrenhaft empirical results about "sub-electrons" was challenging all believers in  $e \sim 4.6 \times 10^{10}$  as the quantum of charge. During the Millikan–Ehrenhaft controversy, each was criticizing the method, and the experimental and mathematical analyses of the other. Ehrenhaft, in particular, calculated—on the basis of Millikan's data!—a wide spread of values for electric charges, which would not warrant Millikan's calculation of  $e$ . Of course, Millikan also criticized Ehrenhaft's calculation.

In the dispute between these two serious and recognized scientists, where does the bias lie? From the analysis of Millikan's notebook, Holton (1978) found out that several data points from the oil drop experiments were missing from Millikan's 1913 paper: 58 out of 140 observations of falling oil drops were reported. There is no need to discuss the extent to which Millikan used strict preset criteria for dismissing observations as uninformative here: philosophers and historians of science alike agree that the selection is guided by a theoretical assumption saying that there exist an elementary charge, which provides warrant to the selection (Holton 1978; Barnes et al. 1996). With all that, it seems that, this time and in contrast to Pauling's case, it is the bias in favor of a theoretical assumption that truly helped Millikan to tell apart reliable from unreliable observations. He received the Nobel prize in Physics in 1923. At that time, Ehrenhaft was not really taken seriously anymore. Ehrenhaft had started with believing in the existence of a basic electric charge of  $4.6 \times 10^{10}$  esu, but was then convinced by his experimental results that it was not the case.

Even though, in such examples, it is difficult to demonstrate the presence of a myside bias with absolute certainty, the evidence is certainly suggestive of its presence. More generally, the experimental, ethnographic and historical evidence reviewed here strongly indicates that, as laypeople, scientists exhibit a myside bias when they reason.

## 4 Evaluating One's Own Reasons

### 4.1 The Cost Benefit Analysis

From the perspective of the argumentative theory, it makes sense to have a myside bias: conviction is better achieved by finding arguments for one's point of view than against it. However, psychological studies show that reasoning not only has a myside bias, but also that it tends to produce relatively poor, superficial reasons (e.g. Kuhn 1991; Perkins 1985). For instance, when asked to defend their

positions on social issues, many people provide circular explanations, and few manage to generate genuine evidence (Kuhn 1991). This trait seems to be harder to reconcile with the argumentative perspective: shouldn't a mechanism designed to convince produce very strong arguments?

Undoubtedly, failing to convince one's audience carries some costs: not only the missed opportunity to influence someone, but also the risk that poorly thought out arguments might make one look incompetent. However, this is not the only cost that should be taken into account. Although the costs are of a different nature—time and energy that could be devoted to other tasks—finding good arguments is also costly. A well-designed reasoning mechanism should only look for a better argument if it is worth it. In a typical informal discussion, the costs of failing to convince are limited: if a first argument fails, more can be offered, and no one expects a Ciceronian oration. Moreover, figuring out what argument is going to speak to any particular audience's beliefs, preferences and desires can be very difficult. Instead of spending a lot of effort anticipating someone's counterargument, it is generally more efficient to advance the first vaguely decent argument we can think of; if it fails to convince, not only can we have other chances, but our interlocutor is likely to help us by saying why she disagrees and why she wasn't swayed by the argument.

To the extent that this type of informal discussion constitutes the normal context for reasoning, it is only to be expected that reasoning shouldn't bother looking for strong arguments at the outset. Instead, people should be able to adapt their arguments as their audience provides them with feedback leading to an increase in the appropriateness and general quality of the arguments as the discussion moves on (for some suggestive evidence, see Kuhn and Crowell 2011; Resnick et al. 1993). This doesn't mean, however, that reasoning should be incapable of adjusting its settings if the balance of costs and benefits varies. In a context in which producing mediocre arguments can have costly consequences and finding better arguments isn't too difficult, we should expect reasoning to engage in more internal filtering of arguments, leading to a higher average quality in argument production.

#### 4.2 The Case of Science

The first prediction stemming from these considerations is that argument quality should increase when people exchange arguments. Dunbar's observations of lab meetings seem to fit this pattern: scientists often start with easily refuted arguments and then move on to increasingly well-supported hypotheses to account for their results (Dunbar 1995).

While it would be hard to contest that discussions have allowed scientists to refine their arguments, the actual exchange of arguments seems less necessary in science. The 'solitary genius' view of science is in large part a figment of the popular imagination (Shapin 1991), but it is not wholly unfounded: Newton's *annus mirabilis* was a lonely one, Darwin spent years refining his theory with little feedback, Einstein was academically isolated when he conceived of both relativity theories. How can the argumentative theory of reasoning account for such fantastic feats of (apparently) solitary reasoning? It seems that scientists, much more than laypeople, have the ability to refine their arguments on their own.<sup>6</sup>

The first factor that is likely to improve the tendency of scientists to evaluate their own arguments is the costs associate with the lack of such evaluation. In science, weak arguments often carry a cost: papers get rejected, reputations suffer. To take an extreme example, Darwin clearly understood the risks he was taking by publishing his theory: that's why he wanted to hone his arguments so well. This explains why scientists, especially in their public statements—presentations, articles, books—can raise the level of argumentation through prior ratiocination. Scientists mentally simulate argumentation because the requirements, with regard to its quality, are higher. Note, however, that the cost-benefit analysis remains essentially social, as it balances the chances of convincing others and those of looking foolish when a weak argument is produced.

The second factor that should improve ratiocination when scientists reason about their field of expertise is the degree of shared beliefs. In informal conversations—deciding where to go on a vacation for instance—preferences, desires and personal idiosyncrasies are likely to play an important role. Such factors are difficult to anticipate in producing arguments, making the reliance on the back and forth of conversation to improve argument quality critical. By contrast, such personal factors are supposed to be absent in scientific arguments. There should be no (at least, far less) need to anticipate other scientists' tastes or personal histories. Instead, scientific arguments must rely on shared elements: not only shared factual or theoretical beliefs, but also shared epistemic beliefs—beliefs about what a good argument looks like. A highly regulated discipline in that matter is mathematics: everything that can figure in a proof is preset (some axioms, or some corpus of mathematical properties and theorems) and the inferential steps are highly regulated (for instance, it is not possible, in contemporary mathematics to say “we see from the figure

<sup>6</sup> Regarding the possibility of anticipating counter-arguments, the view developed here is somewhat more optimistic than that exposed in Mercier and Sperber (2011a, b).

that...”). Kuhn’s notion of paradigm also implies a high degree of shared knowledge among scientists about what can be taken for granted when providing reasons. Years of training make the search for the premises for held conclusion much less costly: these premises are to be found in the well-rehearsed common beliefs of the scientific community.

When scientists share many beliefs, the anticipation of counter-arguments is made much more productive. If, in the course of ratiocination, a scientist finds a counter-argument, it is likely that someone would have found it too, so it needs to be anticipated; if she doesn’t find any, then others mightn’t either. As a result, a scientist is much more likely to improve her arguments through ratiocination than someone anticipating an everyday discussion.

These two factors conspire to explain scientists’ tendency to engage in productive ratiocination: they face high costs if they produce weak arguments, and they can relatively easily anticipate other scientists’ counter-arguments, thereby increasing the quality of their own arguments. Crucially, both factors are social: the costs take the form of a loss of reputation if too many weak arguments are produced, and the ease with which counter-arguments can be anticipated depends on the degree of shared beliefs in the community. However, another interpretation of the efficacy of scientists’ ratiocination could be that scientists have a different reasoning ability, that they can spontaneously exert a higher control over their own arguments, or that their subject matter naturally lends itself to superior ratiocination. At least two elements militate against this individualistic interpretation.

First, Dunbar’s ethnographic observations point to a learning curve in the ability to anticipate other scientists’ counter-arguments. Junior scientists get their rationalizations shot down in lab meetings. Senior scientists go straight to the stage of generating alternative hypotheses. Presumably, they do so because they have learned to anticipate their colleague’s counter-arguments. Second, when the pressure to produce strong argument eases, even the best reasoners lower their standards. The most dramatic case might be Newton’s. Before submitting the *Principia* to his peers’ evaluation, which he knew would be intense, he made sure all the arguments were sound. By contrast, Newton didn’t intend his alchemical writings to be publicized, and the quality of the arguments drops dramatically, from mathematical demonstrations in the *Principia* to vague allegories in the alchemical notes (Hall 1996; Principe 2004).

Even if scientists are able, to some extent, to critically evaluate their own arguments, the most cost-effective solution remains to let others do it for them: to make the best of argumentative discussions, as presently suggested.

## 5 Evaluating Other People’s Reasons

So far, we have focused on the production of argument. People do evaluate—often minimally, sometimes more thoroughly—their own arguments in order to weed off the poorest ones, but the argumentative theory of reasoning suggests that evaluation should be more spontaneous, more thorough, and also more objective when it bears on other people’s arguments. One function of reasoning, indeed, is to evaluate other people’s arguments in order to change one’s mind when it is warranted. Accordingly, people should be able to reject weak arguments and accept strong ones.

In the last section, we reviewed evidence showing that people are, on the whole, not very exigent when it comes to their own arguments. Crucially, there is also substantial evidence that people are good at evaluating other people’s arguments. Not only do people reject fallacious arguments, but they are convinced when strong reasons are presented (e.g. Hahn and Oaksford 2007; Petty and Wegener 1998).

What of scientists? Are they also able to reject weak arguments and accept good ones? It seems to us that the former charge is rarely seriously leveled against scientists: they rarely become convinced of a new claim when it is poorly supported; especially if their current beliefs make the truth of the claim improbable. By contrast, scientists have often been charged with pigheadedness, with a refusal to accept new theories, however well supported. The most famous of these charge was made by Max Planck, who contended that “A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.” (Planck 1968, pp. 33–34). According to this view, scientists never change their minds, revolutionary ideas have to wait for a new generation to be accepted. Such a dire assessment would be difficult to reconcile with the argumentative theory of reasoning. The goal of this section is to explore the ways in which claims about scientists’ pigheadedness can be tested and to suggest that the existing evidence supports the more optimistic view, that of the argumentative theory.

People’s ability to evaluate arguments and to change their mind when faced with good arguments can be measured indirectly, by looking at the outcome of bouts of argumentation. In the course of argumentation, people alternate turns of argument production and argument evaluation. If the latter functions properly, the person who is right should convince her interlocutor more often than the other way around. Many experiments have demonstrated as much; however, only a few are relevant to the case of scientists. Okada and Simon (1997) have demonstrated that discussion in pairs greatly improves the way laypeople construct and test hypotheses when faced with a



scientific problem. Dunbar's ethnographic studies of lab meetings show junior scientists being convinced to reject their hasty rationalizations and forced to develop new hypotheses instead (Dunbar 1995).

However, people who follow Planck in deploring the pace at which new scientific theories are accepted have a different scale in mind: the scientific community as a whole, not small scale discussions. Several factors other than the properties of reasoning could account for the pace in the adoption of a new theory by the scientific community. Slow adoption rates could be perfectly rational: maybe the revolutionary theory is only tentatively supported, with the weight of the evidence still falling behind the current paradigm. For instance, it has been argued that initial rejection of continental drift was quite rational (Thagard and Nowak 1990). Even when pigheadedness is not normatively defensible, it might not be attributable to reasoning. A scientist might privately appreciate the value of a new theory while holding on to the existing paradigm for strategic reasons: to protect her career, her reputation. Such an outcome would not be an indictment of scientists' reasoning, but of the place of reasoning in science. Conversely, fast adoption rates might be driven by social factors—following some more prestigious peers maybe—rather than by a thorough assessment of the arguments supporting a new theory. Although it is difficult to disentangle these explanations, we argue presently that the historical evidence supports the idea that good arguments allow new scientific theories to promptly spread through the community.

Most quantitative studies of the diffusion of scientific theories have addressed a particular thesis shared by Planck and Kuhn: that older scientists are more resistant to change than their younger peers (see Wray 2011). By examining the historical record, these studies have repeatedly failed to find support for this thesis: younger scientists are barely more likely to take new theories in stride (Hull et al. 1978; Levin et al. 1995). Furthermore, the scientists under study nearly always ended up accepting the new theory, sometimes within a relatively short time period. For instance, although the revolutionary theory of plate tectonic was only developed in the mid-1960s, it gained such rapid acceptance that by the early 1970s it had made its way into textbooks (Oreskes 1988). By the end of the 1970s, close to 90 % of geologists—at least in the American sample of Nitecki et al. (1978)—thought the theory established.

Although Kuhn is often remembered for explaining resistance to, rather than acceptance of, new scientific theories, he did not share Planck's severe pronouncement; for Kuhn "[b]ecause scientists are reasonable men, one or another argument will ultimately persuade many of them" (Kuhn 1962, p. 157). The available historical evidence

suggests that Kuhn was, if anything, underestimating scientists' ability to accept new theories.

Even if scientists accept relatively promptly new theories, one could still question the role of argument evaluation in this process. Instead of being convinced by cogent arguments, scientists could follow prestigious colleagues at first, and then the majority of their peers. However, scientists don't simply have to follow the right theory as if it was a mere fashion. They have to work with it, to understand its details and implications. One hardly sees how that would be possible if they did not appreciate the bulk of the arguments supporting the new theory. Accordingly, when scientists have left traces of the process that led them to change their minds, they often make references to being 'converted' by the arguments offered in support of the new theory (Cohen 1985).

If adoption of new scientific theories is driven by successful argumentation, it should be possible to make more specific prediction about the speed at which these theories are adopted. Theories that clash more strongly with the existing paradigm should take longer to accept—a point that hardly needs to be belabored here. Two other factors, more specific to argumentation, are worth exploring: the degree of interactivity of scientific communication, and the degree of shared beliefs in a scientific community.

As discussed above, more interactive contexts make for more efficient argumentation, as they allow counter-arguments to be addressed and better arguments to emerge. Scientific communication comes in more or less interactive forms, with the informal discussion at one end, the published article at the other, and the conference presentation somewhere in between. While the less interactive formats can reach more people, they should be less convincing, *ceteris paribus*, as they make it harder to address the audience's reservations. This decrease in convincingness predicts specific spatial patterns in the diffusion of scientific research and the formation of intellectual schools based on geography—who can talk to whom.

Another factor critical to successful argumentation is shared beliefs between arguer and audience. If two people disagree about everything, they have no lever to convince each other with. Scientific communities differ in the degree of shared beliefs between their members. At one end we find communities of mathematicians, in which at any given time members agree about whether a given proposition is a theorem a conjecture. Mathematicians also seem to share their judgment of new proofs, allowing them to reach consensus promptly once new proofs are offered (Azzouni 2007). Strikingly, this is true even when the proof demolishes a research program central to the field. A few years after Gödel had submitted his first incompleteness theorem, most major mathematicians had accepted it (Mancosu 1999). At the other end of the spectrum we find human

sciences, which often harbor at the same time schools in fundamental disagreement with each other. Accordingly, in such divided fields new theories take longer to settle, if they ever do.

The degree of shared beliefs also mitigates the need for interaction in argumentation. Convincing an audience with very different beliefs is difficult, as each audience member is likely to reject the speaker's arguments for different reasons. Even if it were possible to anticipate all of these reasons, their inclusion in an argument would make most of the argument irrelevant to each audience member. By contrast, an audience with mostly shared beliefs will produce less idiosyncratic counter-arguments, making the counter-arguments easier to address. For instance, a mathematician is in a good position to find most of the potential counter-arguments to her proof with the help of a few colleagues. These counter-arguments can be addressed in the final proof, making it more convincing not only to these few colleagues, but probably to the community as a whole. As a result, in a community with many shared beliefs, the temporal gap between convincing one's close colleagues and convincing the whole community can be very short.

It might be worth here dispelling what might seem like a contradiction between the conclusion of this section—on the whole, argument evaluation works well in science—and the arguments used to demonstrate myside bias among scientists in Sect. 3.2. In Sect. 3.2, we claimed that reviewers were biased in evaluating articles. In the current section, we defend the relative impartiality of argument evaluation. To reconcile these claims, we must stress the importance of dialogue for the success of argumentation. When a reviewer evaluates the arguments in an article, and unless they are perfectly persuasive, she is bound to start producing counter-arguments, as she would in a discussion. However, by contrast with a discussion, these counter-arguments will not be immediately addressed. Instead, they will influence the reviewer's overall evaluation of the article. What should be essentially an evaluative task is in fact at least as much a production task and, as a result, it shares the biases of argument production. By contrast, in the current section we looked either at small-scale discussions or at the large-scale diffusion of scientific ideas—which presumably involves a great many discussions. In both cases, the discussions allowed counter-arguments to be examined and, when the new theory is sound, dismissed.

## 6 Conclusion

Reasoning can be defined as a cognitive mechanism that produces and evaluates reasons. It is a very specific mechanism that we identified by its output: arguments and

epistemic evaluation of arguments. This mechanism is generally understood as having an individualist aim: helping the lone reasoner improve her beliefs and decisions. However, many observations—in particular failures of reasoning documented by experimental psychologists—are difficult to reconcile with this view. An alternative is to consider that reasoning has a social function. This is the route followed by the argumentative theory of reasoning, which suggests that the function of reasoning is to find reasons to convince others and to evaluate others' reasons so as to be convinced when, and only when, appropriate.

The argumentative theory of reasoning clashes with several common beliefs about scientists' reasoning. On the one hand, scientists are often seen as lonely geniuses, working out grand theories in isolation. This is hard to reconcile with the biases that affect solitary reasoning: the myside bias and the difficulty of thoroughly evaluating one's own arguments. Several research strategies are available in view of this apparent contradiction. First, assert that scientific reasoning is essentially different from laypeople's reasoning. There is some truth to that claim since scientific reasoning is regulated by institutions and historically developed epistemic beliefs that specify what counts as a good argument. However, when studying the evolved cognitive basis of scientific reasoning, we focus on an ability that is shared by all humans. Our approach is a contribution to the naturalistic program in science studies, which questions how scientists manage to do what they do given that they are evolved social animals. The second possible strategy consists in asserting that the argumentative theory of reasoning is refuted by scientists' epistemic achievements. Such a claim would, however, be misguided, because the central claim of the argumentative theory of reasoning is about the biologically evolved function of reasoning. Scientists' cognitive practices are not so representative of the normal effect of a capacity that would have favored its biological evolution. At this stage of research, therefore, it is better to consider scientists' cognitive achievements as a normal problem for the theory rather than as refutation. There nonetheless remains a challenge: how to explain the scientific achievements in spite of constitutive bias in reasoning. We recognize that this challenge must be met for the argumentative theory of reasoning to remain plausible. This is why we dedicated this paper to providing some elements of answer. These elements are of two kinds. First, it appears that scientists are also subjects to the biases predicted by the argumentative theory of reasoning; second, the specific argumentative context of science explains why scientists can, to some extent, limit the influence of these biases. We hope to have provided, on the basis of the argumentative theory of reasoning, explanatory elements for some of the observed cognitive biases and achievements of science.

Scientists are also often believed to be resistant to new theories, even if the theories are well supported. Such an observation would also be difficult to reconcile with the argumentative theory, since this theory predicts that reasoning should allow people to change their mind when they are faced with good arguments. Again, the evidence supports the prediction of the argumentative theory rather than the common perception. On the whole scientists, junior or senior, are apt to accept new theories, even if the theories challenge central assumptions of their field.

If scientists reason like everyone else, are prone to the same mistakes, how can science be so successful? The argumentative theory, and the broader framework it is a part of, can also suggest answers to this question. Here we have only hinted at these answers.

First, science might be an epistemically successful institution because it makes the best of reasoning as an argumentative skill. The dynamic of argumentation, we pointed out, is more likely to lead to true beliefs than lonely ratiocination, and the traditions and institutions of science foster and empower argumentation. In particular, scientists are encouraged to exert a sound skepticism: when it comes to evaluating the interpretation of results, experts are expected to rely on the arguments presented rather than on who presents them. This, together with other epistemic practices, allows efficient filtering of poorly supported ideas. This filtering is efficient because it results from a discursive dynamic where scientists judge others' ideas and arguments—an enterprise where the evolved function of reasoning is directly serving epistemic goals—rather than just their own.

Second, and crucially, science has developed ways to work around reasoning's limitations. In particular, it has evolved complementary means to resolve disagreements: experiments and other forms of systematic data gathering together with pre-established agreement on the role they can take in argumentation. These mechanisms supplement argumentation and provide an even finer way to tell right from wrong.

## References

- Azzouni J (2007) How and why mathematics is unique as a social practice. In: Perspectives on mathematical practices. Springer, pp 3–23. Retrieved from [http://link.springer.com/chapter/10.1007/1-4020-5034-8\\_1](http://link.springer.com/chapter/10.1007/1-4020-5034-8_1)
- Barnes B, Bloor D, Henry J (1996) Scientific knowledge: a sociological analysis. University of Chicago Press, Chicago
- Bloor D (1976) Knowledge and social imagery. University of Chicago Press, Chicago
- Bloor D (1997) Remember the strong program? *Sci Technol Hum Values* 22(3):373–385
- Boudon R (1995) *Le juste et le vrai*. Fayard, Paris
- Cohen IB (1985) *Revolution in science*. Harvard University Press, Cambridge
- Cole S (1995) *Voodoo sociology*. *Ann N Y Acad Sci* 775(1):274–287
- Collins H, Pinch T (2005) *Dr. Golem: how to think about medicine*. University of Chicago Press, Chicago
- Dunbar K (1995) How scientists really reason: scientific reasoning in real-world laboratories. In: Sternberg RJ, Davidson JE (eds) *The nature of insight*. MIT Press, Cambridge, pp 365–395
- Dunbar K (1997) How scientists think: online creativity and conceptual change in science. In: Ward TB, Smith SM, Vaid S (eds) *Conceptual structures and processes: emergence discovery and change*. American Psychological Association, Washington, DC, pp 461–493
- Evans JSBT (2003) In two minds: dual-process accounts of reasoning. *Trends Cogn Sci* 7(10):454–459
- Fugelsang JA, Stein CB, Green AE, Dunbar KN (2004) Theory and data interactions of the scientific mind: evidence from the molecular and the cognitive laboratory. *Can J Exp Psychol (Revue canadienne de psychologie expérimentale)* 58(2):86
- Giere RN (2002) Scientific cognition as distributed cognition. In: Carruthers P, Stich S, Siegal M (eds) *Cognitive basis of science*. Cambridge University Press, Cambridge, pp 1–14
- Hahn U, Oaksford M (2007) The rationality of informal argumentation: a bayesian approach to reasoning fallacies. *Psychol Rev* 114(3):704–732
- Hall AR (1996) *Isaac Newton: adventurer in thought*. Cambridge University Press, Cambridge
- Heintz C (2013) Scaffolding on core cognition. In: Caporalet L, Wimsatt WC, Griesemer J (eds) *Developing scaffolds in evolution, culture, and cognition*. MIT Press, Cambridge, pp 209–228
- Holton GJ (1978) *The scientific imagination: case studies*. CUP Archive
- Horwich P (1982) *Probability and evidence*. CUP Archive
- Howson C, Urbach P (2005) *Scientific reasoning: the Bayesian approach*. Open Court, Peru, ILL
- Hull DL, Tessner PD, Diamond AM (1978) Planck's principle. *Science* 202(4369):717–723
- Kahneman D (2003) A perspective on judgment and choice: mapping bounded rationality. *Am Psychol* 58(9):697–720
- Klayman J, Ha Y (1987) Confirmation, disconfirmation, and information in hypothesis testing. *Psychol Rev* 94:211–228
- Knorr Cetina K (1991) Epistemic cultures: forms of reason in science. *Hist Polit Econ* 23(1):105–122
- Koehler JJ (1993) The influence of prior beliefs on scientific judgments of evidence quality. *Organ Behav Hum Decis Process* 56(1):28–55
- Kuhn T (1962) *The structure of scientific revolutions*, 50th anniversary edn. Chicago University Press, Chicago
- Kuhn D (1991) *The skills of arguments*. Cambridge University Press, Cambridge
- Kuhn D, Crowell A (2011) Dialogic argumentation as a vehicle for developing young adolescents' thinking. *Psychol Sci* 22(4):545
- Lakatos I (1970). History of science and its rational reconstructions. PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, 1970, 91–136. Retrieved from <http://www.jstor.org/stable/495757>
- Latour B (1993) *The pasteurization of France*. Harvard University Press, Cambridge
- Levin SG, Stephan PE, Walker MB (1995) Planck's principle revisited: a note. *Soc Stud Sci* 25(2):275–283
- Magnani L, Nersessian NJ (2002) *Model-based reasoning: science, technology, values*. Springer, New York
- Mahoney MJ (1977) Publication prejudices: an experimental study of confirmatory bias in the peer review system. *Cogn Therapy Res* 1(2):161–175

- Mahoney MJ, DeMonbreun BG (1977) Psychology of the scientist: an analysis of problem-solving bias. *Cogn Therapy Res* 1(3):229–238
- Mancosu P (1999) Between Vienna and Berlin: the immediate reception of Godel's incompleteness theorems. *Hist Philos Log* 20(1):33–45
- Mercier H (2013) Using evolutionary thinking to cut across disciplines: the example of the argumentative theory of reasoning. In: Zentall T, Crowley P (eds) *Comparative decision making*. Oxford University Press, New York
- Mercier H, Heintz C (2013) The place of evolved cognition in scientific thinking. *Religion Brain Behav* 3(2):128–134
- Mercier H, Sperber D (2011a) Why do humans reason? Arguments for an argumentative theory. *Behav Brain Sci* 34(2):57–74
- Mercier H, Sperber D (2011b) Argumentation: its adaptiveness and efficacy. *Behav Brain Sci* 34(2):94–111
- Nersessian N (2008) *Creating scientific concepts*. MIT Press, Cambridge, MA
- Nickerson RS (1998) Confirmation bias: a ubiquitous phenomena in many guises. *Rev Gen Psychol* 2:175–220
- Nitecki MH, Lemke JL, Pullman HW, Johnson ME (1978) Acceptance of plate tectonic theory by geologists. *Geology* 6(11):661–664
- Okada T, Simon HA (1997) Collaboration discovery in a scientific domain. *Cogn Sci* 21(2):109–146
- Oreskes N (1988) The rejection of continental drift. *Hist Stud Phys Biol Sci* 18(2):311–348
- Osbeck LM, Nersessian NJ, Malone KR, Newstetter WC (2013) *Science as psychology: sense-making and identity in science practice*. Cambridge University Press, Cambridge
- Pauling L, Herman ZS (1989) Criteria for the validity of clinical trials of treatments of cohorts of cancer patients based on the Hardin Jones principle. *Proc Natl Acad Sci* 86(18):6835–6837
- Perkins DN (1985) Postprimary education has little impact on informal reasoning. *J Educ Psychol* 77:562–571
- Petty RE, Wegener DT (1998) Attitude change: multiple roles for persuasion variables. In: Gilbert D, Fiske S, Lindzey G (eds) *The handbook of social psychology*. McGraw-Hill, Boston, pp 323–390
- Pickering A (1980) The role of interests in high-energy physics. In: Knorr K, Krohn R, Whitley R (eds) *The social process of scientific investigation*. Springer, Dordrecht
- Planck M (1968) *Scientific autobiography and other papers*. Citadel Press, New York
- Principe L (2004) Reflections on Newton's Alchemy in light of the new historiography of Alchemy. In: Force JE, Hutton S (eds) *Newton and Newtonianism: new studies*. Kluwer, Dordrecht, pp 205–219
- Resnick LB, Salmon M, Zeitz CM, Wathen SH, Holowchak M (1993) Reasoning in conversation. *Cognit Instr* 11(3/4):347–364
- Shapin S (1991) "The mind is its own place": science and solitude in seventeenth-century England. *Sci Context* 4(01):191–218
- Sloman SA (1996) The empirical case for two systems of reasoning. *Psychol Bull* 119(1):3–22
- Sperber D (1997) Intuitive and reflective beliefs. *Mind Lang* 12(1):67–83
- Sperber D (2001) An evolutionary perspective on testimony and argumentation. *Philos Top* 29:401–413
- Sperber D, Mercier H (2012) Reasoning as a social competence. In: Landemore H, Elster J (eds) *Collective wisdom*. Cambridge University Press, Cambridge
- Stanovich KE (2004) *The Robot's rebellion*. Chicago University Press, Chicago
- Thagard P, Nowak G (1990) The conceptual structure of the geological revolution. In: Shrager J, Langley P (eds) *Computational models of scientific discovery and theory formation*. Morgan Kaufman, San Mateo, pp 27–72
- Wray KB (2011) *Kuhn's evolutionary social epistemology*. Cambridge University Press, New York