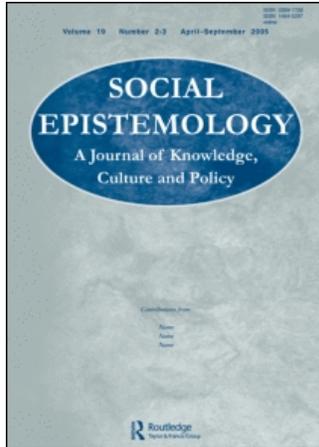


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Social Epistemology

A Journal of Knowledge, Culture and Policy

Publication details, including instructions for authors and subscription information:
<http://www.informaworld.com/smpp/title~content=t713765921>

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Online Publication Date: 01 October 2007

To cite this Article: Gingras, Yves (2007) "'Please, Don't Let Me Be Misunderstood': The Role of Argumentation in a Sociology of Academic Misunderstandings", *Social Epistemology*, 21:4, 369 - 389

To link to this article: DOI: 10.1080/02691720701746607

URL: <http://dx.doi.org/10.1080/02691720701746607>

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“Please, Don’t Let Me Be Misunderstood”: The Role of Argumentation in a Sociology of Academic Misunderstandings

Yves Gingras

Academic debates are so frequent and omnipresent in most disciplines, particularly the social sciences and humanities, it seems obvious that disagreements are bound to occur. The aim of this paper is to show that whereas the agent who perceives his/her contribution as being misunderstood locates the origin of the communication problem on the side of the receiver who “misinterprets” the text, the emitter is in fact also contributing to the possibility of this misunderstanding through the very manner in which his/her text is written. In other words, I propose a symmetric approach to understanding misunderstandings: taking simultaneously into account the position of the reader in the scientific field and the structure of the texts of the writers. The paper thus proposes to complement the sociological analysis of controversies in a scientific field with the close reading of texts, a practice usually found in studies of argumentation, in order to explain the occurrence of misunderstandings. The debate surrounding the charge of “relativism” among sociologists of scientific knowledge provides us with a case study to analyse in detail the argumentative context of misunderstanding.

Keywords: Strong program; Argumentation; Controversies; Academic Misunderstanding; Sociology of Scientific Knowledge (SSK)

Academic debates, either in print or in formal or informal discussions, are so frequent and omnipresent in most disciplines, particularly the social sciences and humanities, it seems obvious that disagreements are bound to occur. When not resolved by some clarification or concession, they are most of the times spontaneously explained in

Yves Gingras is Canada Research Chair in history and sociology of sciences at CIRST UQAM. Correspondence to: Yves Gingras, Professeur, Département d’histoire, Chaire de recherche du Canada en histoire et sociologie des sciences, CIRST, UQAM, C.P. 8888 Suc Centre-Ville, Montréal, Québec, Canada H3C 3P8. Email: gingras.yves@uqam.ca

terms of conflicting methodologies, paradigms or ideological commitments, taking for granted the transparency of the argumentative structure of the texts. Although the literature on scientific controversies is huge,¹ scarce attention seems to have been devoted to the fact that the agents involved in debates frequently invoke being “misunderstood” by their critics. The existing literature on this question is essentially limited to linguistic analysis of oral communication between two agents of the same (or different) culture and language, and few papers address in any detail misunderstandings based on written communication. In the oral context, misunderstandings can be generated by a variety of mechanisms related to phonology, syntax, semantics or pragmatics (Blum-Kulka and Weizman 1988; Zaefferer 1977). Also, these treatments usually focus on the receiver (hearer) and scant attention is given to the producer (emitter) of discourse.² In the oral context, cultural and linguistic habits can also explain many misunderstandings of an otherwise well-formed sentence (Jung 2006). As for written communication, it is more constrained than the oral one and if we leave aside typographical errors—which are usually taken care of by a charitable interpretation of the immediate context of the sentence—the syntax and, above all, the semantics are the main locus of possible misunderstandings. At the pragmatic level, various reasons may be invoked to explain misunderstandings, such as being ignorant of the topic discussed or not familiar with the technical vocabulary used by the opponent. This type of explanation is more likely to be put forward when the participants are from different fields or even subfields and take for granted different meanings of some of the terms involved in the debate. Here the pragmatic context is related to the sociology of the interaction, and it is this connection that I want to make in this paper. What interests me here is to look at a case where the complaints about being misunderstood come from agents that are part of the same subfield and even the same paradigm.³ Having such a homogeneous group permits one to leave aside the usual obvious reasons of misunderstanding and concentrate on an aspect that is rarely analysed in sociology of science, although it is, I think, central to scholarly communication: the role played by the structure of the texts themselves—that is, their argumentative structure—in contributing to misunderstandings. Using Pierre Bourdieu’s concept of a scientific field (Bourdieu 2004) having its specific rules and legitimate forms of argumentations we can connect two aspects usually separated in the literature on rhetoric and that on scientific controversies: whereas the former concentrates on the linguistic aspects and takes the sociological context as given, the latter focuses on the social aspects leaving aside the formal properties of the texts. In the large domain of rhetorical analysis, I follow the tradition that is more interested in the structure of the *argumentation* itself than in the identification of the different *tropes* used in the texts, although of course both aspects are present in most texts (Eemeren and Grootendorst 2004; Perelman 1997; Perelman and Olbrechts-Tyteca 1988). Moreover, I will also usually stick to the word “argumentation” instead of “rhetoric” in order to avoid the negative connotation attached to the latter and to insist on the dialogical and intersubjective nature of communication.⁴ Although attentive to the internal structure of the texts being debated by the agents, the overall approach remains sociological in that it locates the texts and their authors in the structure of the scientific field.

My aim in this paper is to show that whereas the agent who perceives his or her contribution as being misunderstood locates the origin of the communication problem on the side of the *receiver* who “misinterprets” the text, the *emitter* is in fact also contributing to the possibility of this misunderstanding through the very manner in which his text is written. I thus propose to complement the sociological analysis of controversies in a scientific field with the close reading of texts, an approach usually found in studies of argumentation, in order to explain the occurrence of misunderstandings. For only by taking simultaneously into account the context of production and the context of reception can we get a complete understanding of successful as well as failed communication.⁵ The debate surrounding the charge of “relativism” among sociologists of scientific knowledge (SSK) provides us with a case study to analyse in detail the argumentative context of misunderstanding. As a general rule, I will restrict my analysis to criticisms formulated by agents within the field of SSK and omit those put forward by scientists and philosophers; not because of their lack of relevance, but because they are more complex to analyse given their heterogeneous origins. A general analysis of all the heterogeneous agents involved in this debate would have to take into account the larger intellectual field and would divert us from our primary purpose.⁶ Moreover, my interest being in understanding misunderstandings, it is much easier to explain (away) those of outsiders than insiders. In the former case, answering “they just don’t understand” could be considered an adequate rebuttal to counter their criticisms as coming from agents outside the field and thus with limited, superficial or even inexistent knowledge of *sociological* practice.⁷ As David Bloor himself noted about some misinterpretations of the strong programme in SSK, “such errors might be expected from ill informed critics from outside the field” (Bloor 1999, 111). One could also, as suggested by Steven Shapin, explain these misunderstandings by the different “disciplinary intensions” of scientists and social scientists (Shapin 2001, 102). In any case, the acrimonious tone of many exchanges with scientists confirms Aristotle’s advice in *The Topics*: “Do not argue with every one, nor practice upon the man in the street: for there are some people with whom any argument is bound to degenerate. [...] Therefore the best rule is, not lightly to engage with the man in the street, or bad argument is sure to result. For you see how in practicing together people cannot refrain from contentious argument” (Aristotle 1984, 277).

So, whereas “external” attacks can often be ignored with good reasons, the situation is quite different when the same charges of “relativism” or “idealism” come from colleagues who, until recently at any rate, seemed to belong to the same “constructivist camp”. In this case, one cannot invoke disciplinary distance and, short of concluding that the strong programme is inherently incomprehensible or that the critical readers are incompetent or disingenuous, it seems to me that part of the answer must be sought in the narrative structure of the texts themselves. Whereas, as we shall see below, Bloor attempted to explain the misunderstandings of his views by invoking Bartlett’s psychological law of “conventionalisation”, thus locating their source entirely on the side of the *receiver*, I wish to sketch a complementary explanation that puts part of the possibility of misreading on the side of the *transmitters*, thus taking seriously the argumentative structure of their texts. In other words, I propose a symmetric approach

to understanding misunderstandings: taking simultaneously into account the position of the *reader* in the scientific field (here SSK) and the structure of the texts of the *writers*. In that sense, the proposed explanation is not opposed to that of Bloor but complementary, as it brings a dimension of communication that is taken for granted as transparent and unproblematic whereas in fact it often plays a crucial role in generating misunderstandings and should be analysed as such.

Before proposing this analysis of the argumentative structure of some classic texts and debates that takes into account the phenomenology of reading, I will first recall the key criticisms voiced by major players in the SSK subfield who accused some of their colleagues of not taking “reality” into account in their analysis of science. This will be followed by a presentation of David Bloor’s proposed explanation of the “misunderstanding” of the Strong Program based on the views of the psychologist Frederick Bartlett. I will then proceed to a close reading of exemplary texts to show how the structure of the sentences contributed to the misunderstandings, thus formulating a brief phenomenology of reading.

Constructivist Self-criticism

After a decade of intense intellectual activity, running roughly from 1975 to 1985, followed by a period of diversification (1986–1992), SSK entered a period of conceptual disorientation and intense internal and external debates.⁸

Thus, after a first decade of internal consensus over the “social construction of science”, fairly strong criticisms started issuing from the “actor-network” movement,⁹ first set in motion by Michel Callon, Bruno Latour and John Law. Indeed, as early as 1987, in an implicit reference to the strong programme, Law wrote: “to try to reduce an explanation of the Portuguese system to a limited number of social categories would be to fail to explain the specificity of the *volta*, the caravel or the *Regimento*” (Law 1987, 131). These boats are material objects that are, Law assures us, necessary to *explain* Portugal’s maritime expansion. In a similar way and that same year, Latour wrote that “since the settlement of a controversy is the cause of Society’s stability, we cannot use Society to explain how and why a controversy has been settled” (Latour 1987, 144). Pretending to “generalize” the principle of symmetry that Bloor had introduced to permit *sociological* explanations of the content of science, Latour had thus proposed that “we should consider symmetrically the efforts to enrol humans and non-human resources” (Latour 1987, 144). Similar criticisms also appeared in a text published by Michel Callon (1986).

Following a different route, Andy Pickering also came to impute to the strong programme a social relativism that completely failed to consider the material dimension of science. Before joining the strong programme’s critics, Pickering had published in 1984 an exemplary application of Barnes’ sociology of interests to the case of elementary particles physics (Pickering 1984).¹⁰ In Pickering’s narrative, the constraints or resistances of reality played no *explanatory* role. Only variables related to social interests were employed to explain the existence or the non-existence of phenomena such as neutral currents or the fractional electric charges of quarks, thus characterising them

as merely the *effects* of a consensus that was obtained within the scientific community.¹¹ In 1993, in an article entitled “The Mangle of Practice”, which also served as the title of his 1995 book, Pickering “confessed” the error of his youth and denounced the strong programme for having eliminated the *material* dimension from the study of scientific practice. Indeed, according to Pickering, “the problem is that SSK makes it impossible to take material agency seriously. The other side of SSK’s focus on human agency is precisely the invisibility of material agency” (Pickering 1995, 10).¹²

Whereas these skirmishes were limited essentially to the subfields of SSK and philosophy of science, the publication in 1994 of Paul R. Gross’ and Norman Levitt’s polemical book *Higher Superstition* moved the debate into the wider intellectual field and associated media (Gross and Levitt 1994). These external attacks on constructivist sociology of science denounced its “post-modern” orientation (whatever that term may mean), and launched what came to be known as the “science wars”. Beginning in the spring of 1996, the “Sokal affair” exacerbated the controversy over the so-called “relativism” of constructivist sociology by popularising the issue in daily newspapers, thus oversimplifying it beyond what had already been achieved by *Higher Superstition*, a book that had inspired Sokal.¹³ Building on the momentum provided by the Sokal affair, Gross and Levitt published that same year the collective work *The Flight from Science and Reason*, which continued their attack, indiscriminately associating SSK with the North American *cultural studies* movement (Gross, Levitt and Lewis 1996).¹⁴

Much attention has already been paid to the “realism–relativism” debate, which mostly pitted philosophers and scientists against the “relativistic” analyses of constructivist sociologists. I have no intention here of contributing to this debate, nor even to discuss the strong programme’s epistemological and methodological foundations.¹⁵ Rather, I wish to analyse an internal debate in which central figures of SSK have accused their peers of the very same “relativism”, and even “idealism”, that scientists and philosophers had so often directed at them over the past 25 years. For this debate raises an intriguing question: How can we explain the fact that the founders of the “strong programme”, Barry Barnes and David Bloor, are now saying that they have never denied the influence of reality in the production of scientific knowledge and that they have in fact been grossly misunderstood? The question is all the more interesting since Bloor has gone so far as to charge his colleague Bruno Latour with interpreting his work in ways that are utterly “incomprehensible” coming from such a “central” figure of the field (Bloor 1999, 111). Similarly, but without giving names, Barnes believes that a certain form of “misinformation” has been “actively encouraged by cynical academics hostile to [the Edinburgh] approach” (Barnes 1998, 637). But before trying to understand how such a misunderstanding can occur among a relatively homogeneous group, let us look at Bloor’s response to these “attacks”.

Bloor Strikes Back

Although the criticisms of “relativism” and “idealism” had been circulating for over a decade, it was only in 1999 that Bloor decided to respond energetically to his detractors/colleagues. Until then, his defence of the strong programme had been essentially

targeted at philosophers' criticisms. He now concentrated his attack on Latour, who, in his eyes, represents the best example of someone having completely misunderstood the nature of the strong programme. In fact, Bloor ends up concluding that Latour "belongs to the ranks of those like the recent contributors to *The Flight From Science and Reason* (Gross et al., 1996) who have only to see the label 'sociology of knowledge' to conclude that its doctrine must imply the absurdity that knowledge is 'purely social'" (Bloor 1999, 110). The reference to this book strongly suggests a link between the "science wars" and this new phase of SSK's internal debates. It is indeed difficult to avoid the impression that Bloor's decision to finally enter the debate¹⁶ is in large part dictated by these external events and appears as a reaction to attacks by scientists and other intellectuals, which had taken on considerable proportions with the "science war" and had also had, in the United States at least, concrete institutional repercussions.¹⁷

Whatever the exact role of the "science wars" played in the origin of this dispute, it is interesting to note that in their book *Scientific Knowledge: A Sociological Analysis*, Barnes, Bloor, and Henry (1996) also directed their attack at another major actor within the SSK field: Harry Collins. Indeed, they accuse Collins of "methodological idealism" and of adopting a "relativistic" position that denies the existence of natural causes in the closure of scientific debates. Recall that, for Collins, the sociologist must study science *as if* the natural world played no role in the production of knowledge. Although clearly polemical, this methodological thesis was of course not new in 1996. It had first appeared as far back as 1981 in a programmatic text in which Collins proclaimed that "the natural world *in no way* constrains what is believed to be" (Collins 1981a, p. 54; emphasis added). A slightly different (and more ambiguous) formulation was given in a short introduction to the collection of papers in which the preceding formulation appears: "the natural world has a *small or non-existent* role in the construction of scientific knowledge" (Collins 1981b, p. 3; emphasis added). Two years later he used the formula: "the natural world must be treated *as though it did not affect our perception of it*" (Collins 1983, p. 88; emphasis added), which has still another meaning. Reflecting on these expressions, Collins now sees no reason to change anything "except for a few rhetorical flourishes" (Collins 2004, 797).¹⁸ But from the point of view of *argumentation* adopted here, what Collins seems to consider "rhetorical flourishes" were of course read as serious *arguments* (or at least *statements*) by most readers (including scientists and philosophers) who usually take it for granted that writers know their grammar and do mean what they say and what they write. As I suggested elsewhere (Gingras 2007), one can even suggest that without these "flourishes" there would hardly have ever been any "science war"; no "unpleasant war of words between a group of self-appointed spokespersons for science and the social scientists", to use Collins' words (Collins 2004, 793).

Coming back to Bloor and his colleagues, it is intriguing that they waited 15 years before denouncing Collins as an "idealist". And it is worth noting that, from a rhetorical point of view, castigating him as "idealist" implicitly suggests that the strong programme is, by contrast, "materialist" and that it consequently takes into consideration the existence of the natural world in its sociological explanations of how

knowledge is produced. For Bloor and his colleagues insist that: “Pace Collins and his interesting idealist arguments, sociologists should be willing to acknowledge the existence and the *causal relevance* of the physical environment when they study the growth of knowledge” (Barnes, Bloor and Henry 1996, 76; emphasis added).

Moving from principles to concrete applications, the authors criticise Collins’ and Pinch’s analysis of a controversy over the existence of extra-sensory perception (ESP). There, Collins and Pinch had claimed to be able to explain the absence of consensus in the field in a perfectly symmetrical way, noting that the “believers” interpreted the negative experimental results as examples of real phenomena that simply failed to occur, while the “unbelievers” considered these negative results as proof that such paranormal phenomena simply do not exist. Contrary to Collins and Pinch, who say nothing about the possible “reality” of the contested phenomena, Bloor and his colleagues suggest that “it was probably because of *the state of the material world* in the context of the experiment that ESP was considered absent” (Barnes, Bloor and Henry 1996, 76). Thus, Collins and Pinch propose a *social* cause precisely where Bloor and his collaborators advance a *material* one. As my goal is not to defend Collins and Pinch, nor to discuss the validity of the symmetry principle, I simply wish to point out that the alternative explanation proposed is somewhat surprising from an SSK perspective, since it is founded upon the *absence* of a result, which, as Collins and Pinch noted, is an entirely circumstantial outcome from the point of view of the actors involved. To invoke “the state of the material world” is to abandon the actors’ point of view and to violate the symmetry principle by adopting in fact the position of an “unbeliever”. As we shall later see, it is the very *absence* of an empirical signal (as in the case of gravitational waves) that ensures the plausibility (and possibility) of the symmetrical analysis of ESP proposed by Collins and Pinch. Conversely, it is the intervention of a material cause through an unambiguous *positive* signal (as in the case of the Transverse Electric Atmospheric (TEA) laser, discussed below) that allows one to escape from the experimenter’s regress, thus breaking the symmetry. But these logical points are not perceived by Bloor and his colleagues.

Bloor’s analysis tends to remain at the level of general principles and rarely addresses the narrative detail of the case in question. But it is important to first clearly distinguish general statements of principle that abstractly affirm the importance (or not) of “reality” in the construction of knowledge from the actual empirical studies and their narrative structure. For, if I am correct in believing that the misunderstandings are partly explained by the specific formulations offered to the reader, it is only by carefully analysing the argumentative structure of existing case studies that we will be in a position to determine whether the authors are relativists or not *in the explanatory narratives* of their texts. In other words: instead of asking a metaphysical question about “reality” or trying to know whether Bloor or Collins really believe in the external world, I propose to search for the exact *role* any element of “reality” plays in the narratives they offer of the phenomena they analyse. From this point of view, as we shall now see for the case of the successful construction of a TEA-CO₂ laser, Collins is sometimes less “idealistic” than he seems in his more programmatic statements (quoted above).

In his study of the final development of the TEA-CO₂ laser, Collins’ narrative explicitly invoked the material world as a factor in forging the consensus surrounding the

laser's success. He writes: "the experimenter's regress did not make itself apparent [...] *because* in the case of the TEA-laser, the circle was readily broken. The *ability of the laser to vaporise concrete*, or whatever, comprised a universally agreed [upon] criterion of experiment quality" (Collins 1985, 84; emphasis added). In this brief sentence on which Collins does not insist, the laser's physical properties are presented as the *cause* for believing that the laser works. This example, unique to Collins' work, suffices to illustrate the potential difference between methodological or programmatic statements and the causal structure of a particular narrative account. What clearly stops the discussion on the working or not of the laser is not any social interest but the very fact that coherent electromagnetic waves intrudes in a spectacular way by evaporating any material on its path. By contrast, what makes credible relativist-looking narratives in the case of ESP or gravitational waves is the fact that *they have not yet been detected in any comparable spectacular way*. In other words, it has rarely been noted that it is only these *open* and undecided cases that allow Collins to adhere to the "relativistic thesis within which consideration of the 'actual existence' of a phenomenon is redundant" (Collins and Pinch 1979, 262–263). "Reality" can indeed appear to be superfluous (in a contingent way and up to now) for phenomena such as ESP and gravitational waves but not for the TEA laser.

Another manner to implicitly suggest a relativist posture is exemplified by Trevor Pinch when he disputes Martin Rudwick's assertion that empirical data played a decisive role in closing the controversy over the nature of the Devonian period in the history of mid-19th-century British geology. Although he recognises that Rudwick is receptive to the constructivist approach, Pinch nonetheless sees him attributing a much more important role to nature than SSK proponents normally do. He thus tries to show that it is precisely for this reason that Rudwick's conclusions are unconvincing. Again, my goal here is not to take side in this controversy, but to draw attention to the fact that, by concentrating his attack on Rudwick's "realism", Pinch ends up suggesting implicitly that a convincing SSK analysis *cannot* grant a causal role to empirical data (Pinch 1986, 711).¹⁹

Bloor on Bartlett's Law

In view of these accusations of "relativism" and "idealism" coming from some of the pillars of social constructivism, it seems hardly necessary to recall the numerous "denunciations" of relativism or even of "obscurantism" from scientists or rationalist philosophers.²⁰ Let us instead return to our opening question: How are we to explain such "misunderstandings" between presumably well-informed parties? Despite the many papers devoted to the "realist-relativist" debate, most are contributions to the debate itself and few try to offer a distanced sociological analysis, taking as it were the very debate as an object instead of participating in it. An exception is David Bloor who instead of just adding fuel to the debate took some distance to propose an explanation based on Bartlett's second law, named after the British psychologist Sir Frederic Charles Bartlett (1886–1969), author of an important study on memory (Bartlett 1932). This law, also referred to as "conventionalisation", describes how

agents have a tendency to simplify the stories they have heard (or read) so as to render them decreasingly dependent on the culture of origin and increasingly compatible with the agent's own culture. Moreover, over the course of recalling and retelling, the story takes on new characteristics that are specific to the agent's own culture and absent from the original version. In short, the original formulation tends to be reinterpreted in the context and in accord with the culture of the receiving agent (Bloor 1997, 375). Thus, when Stephen Cole (1998) reduced Bloor's position to that of a neo-Marxist—even though the targeted article was entitled “Durkheim and Mauss revisited” (not Marx ...)—and oversimplified his analysis of how Boyle and Newton came to believe in the passivity of matter, the American sociologist was succumbing, for polemical reasons, to Bartlett's law of conventionalisation (Bloor 1999, 376).²¹

One can hardly deny the existence of this process of oversimplification and schematisation, often accentuated by polemical posturing, in the debates between supporters and opponents of constructivism.²² I believe, however, there are other processes at work that can better explain why some authors seem to be so misunderstood. Indeed, while Bartlett's law locates the process on the side of the *receiver*, I think that the basis for the misunderstanding also lies on the side of the *transmitter* of the message, or more precisely in the narrative and explanatory structures of the texts themselves. Bazzanella and Damiano identify different types of “triggers” that generate misunderstanding, and ambiguity is one of the most important of them (Bazzanella and Damiano 1999, 818), cultural differences being another one. Thus, one should expect that, in written texts, the misunderstandings will in large part be triggered by: (1) the ambiguous formulation (conscious or not) of some statements of principle that allow for at least two readings, one benign (if not trivial) and the other radical; (2) the confusion, during polemical exchanges, between statements of principle and the actual content of case studies, as if they were equivalent; and, finally, (3) the gap between theoretical intentions (formulated in the general principles) and the effective narrative structure of case studies based on these principles, some agents moving from one to the other without indicating it.

Adopting here the neutral posture of the observer, I will eschew the moral tone expressed in the use of terms such as “cynical” or “misinformation” and try to propose instead cognitive and sociological reasons that take into account the argumentative structure of the texts. We thus leave open the question as to whether or not some of the actors involved in the debate are “cynical”, for the simple reason that it is very difficult to test such imputations of motives. By limiting our analysis to a homogeneous group of actors in a scientific field, we take for granted that they obey the rules of the field and do not wilfully act in a cynical manner.²³ If the latter were the case, they would simply breach the implicit communication contract, which, as Marcelo Dascal noted, “rests on mutual trust between responsible individuals” (Dascal 1999, 757). In that sense, this strong implicit contract based on the presumptions of trust and responsibility is such that it would be legitimate “to view those use of language that deliberately violate [these presumptions] as morally—not just technically—wrong since they involve a breach of trust”. Although one could study “the moral implications of manipulative practices that evade communicative responsibility” (Dascal 1999, 757), I will not do that here

and limit myself to making visible the argumentative strategies that may trigger misunderstandings, whether these uses are conscious or not. It is up to the reader to impute motivations if he/she so wishes, but that does not affect the analysis proposed here.

Radicality Effects

Before focusing more narrowly on the narrative and explanatory structure of certain key texts, let us briefly recall some of the textual features associated with general statements of principle—usually found in introductions and conclusions—that contribute to explain the “exaggerated” reactions of certain readers and their “profoundly incomprehensible” reading of SSK texts. I have noted elsewhere (Gingras 1995) the tendency to play on the meaning of words and to confound, for example, a *relation* with an *identity*, thus allowing the reduction of “science” to “politics”. Following Olga Amsterdam-ska (1990), Bloor has also noted Latour’s tendency to play on the meaning of words, moving from “nature” to the “representation of nature” as if these formulations were synonymous. Bloor points out that SSK acknowledges the existence of nature and is only interested in the representations verbalised by scientists; that is, in their beliefs regarding nature. It is because they failed to see this distinction between “reality” and “knowledge of reality”, he argues, that critics perceived SSK as a form of idealism, whereas the programme in fact embraces both materialism and naturalism (Bloor 1999, 87).

Brief programmatic statements that are sufficiently ambiguous to enable the writer to charge any would-be opponents with misunderstanding them often provoke the anger of scientists and rationalist philosophers. Take Collins’ three different statements already cited. The first, which affirms that “the natural world *in no way* constrains what is believed to be” (Collins 1981a, 54; emphasis added), is indeed radical (“*in no way* constrains”) and was even considered (and denounced) as “absurd” by some sociologists (Cole 1998, 275), not to mention physicists.²⁴ The second version, written the same year, suggesting that “the natural world *has a small or non-existent role* in the construction of scientific knowledge” (Collins 1981b, 3; emphasis added), is already opening the door to a *role* (albeit small) for reality. Finally, the third, which suggests that “the natural world *must be treated as though it did not affect our perception of it*” (Collins 1983, 88; emphasis added), reflects more clearly the *methodological* nature of the statement. By contrast, the first statement having no qualifying clause such as “*must be treated as though*” can only be read as an *ontological* statement (“*nature in no way constrains*”); the only way to interpret it otherwise would be to invoke external factors like “I know the guy and what he *means* is methodological”. The internal logic of the first two statements does not give this information (as the third statement did), and any charitable reinterpretation must invoke *ad hoc* assumptions (like “careless writing”) or “strategic manoeuvring” in order to justify *not* taking them at face value.

Now, let us see how a sociologist of science trained in the SSK paradigm reads an ambiguous statement about the role of reality in knowledge construction. In his critique of SSK, Pickering’s picks up a short statement by Barnes: “Reality will tolerate alternative descriptions without protest. We may say what we will of it, and it will not

disagree. Sociologists of knowledge rightly reject realist epistemologies that empower reality” (Barnes 1994, 331).²⁵ This statement is (consciously or not) *intrinsically* ambiguous. In a strict but trivial sense, it is true: reality cannot protest against what *we* say of it for the simple reason that *it* cannot talk. In a figurative sense, however—which is most of the time the way such metaphors are used and interpreted in ordinary language²⁶—such a statement suggests that a scientific discourse (theory) need not be related to reality, which seems to be how Pickering understood it since he denounced it as being a relativistic thesis. Again, the point is not to agree or disagree with Barnes (or any other sociologist for that matter) but to note that the very structure of the sentence provides the material basis that makes possible Pickering’s interpretation, which cannot simply be dismissed as a “mistaken” reading.

We have a similar situation in David Mermin’s review of the book written by Barnes, Bloor and Henry. Commenting on the proposition that even a discipline as rigorous as mathematics can in fact conceal flagrant contradictions that go unnoticed by students, these authors write: “People have apparently found little problem in accepting that a circle is a line, notwithstanding it has a finite area” (Barnes, Bloor and Henry 1996, 64). In his review, Mermin, a physicist, noted that the use of such ambiguous formulations give a radical tone to statements that would otherwise be totally banal. He writes: “this sounds less paradoxical and considerably less interesting if you say instead: ‘people find little problem in accepting the use of a single term to describe either a line or the region of finite area bounded by that line, because the intended meaning is almost invariably clear in the context’” (Mermin 1998, 609).²⁷ As before, it seems clear that the very structure of the sentence does contain the possibility of suggesting both interpretations to the reader.

Another striking example of what we might call *radicality effects* is at work in declarations that *objects* have a history, that they are born and often die, as Lorraine Daston writes in her introduction to a book on *Biographies of Scientific Objects*: “This is a book about [...] how whole domains of phenomena [...] come into being and pass away *as objects of scientific inquiry*” (Daston 2000, 1–2; emphasis added). In the following paragraph we learn that “this way of understanding *scientific objects* [...] is at odds with the sense entrenched in the etymology of the very word ‘object’ in several major European languages”, which suggest that these objects “are neither subtle nor evanescent nor hidden [...] They do not need to be discovered or investigated [...]”. Finally, we are told that “an ontology that is true to *objects* that are at once real and historical has yet to come into being, but it is already clear that it will be an ontology in motion”. Beyond the poetic style, the shift in meaning is pretty obvious: the author moves from the banal observation that *objects of scientific inquiry* change over time to the idea that it is *objects themselves* that emerge and disappear. Daston could of course allege that she distinguishes “objects” in some technical sense from “things” in the everyday sense, but we are still left with an intrinsic ambiguity that permits to move from one interpretation to the other depending on the audience or the critics received. This oscillation between “object” and “object of scientific inquiry” or “scientific objects”, which are at once real and historical, thus astutely combining (at least in the realm of words) the advantages of “realism” and “constructivism”, is in fact similar to Latour’s

shifts between “reality” and the “representation of reality” noted by Bloor, which produce a radicality effect while conserving the possibility of returning to the “banal” sense when needed. One could perhaps offer a more charitable reading, but as Alban Bouvier observes, although the transgression of elementary rules of communication is not incompatible with the comprehension of a text, it nonetheless “requires a cognitive (and temporal) cost that not all readers, even the well intentioned, are necessarily ready to pay” (Bouvier 1996, 56).

Moreover, while a better control of style would eliminate such radicality effects (when they were not intended), it would not completely exclude the kind of biased readings that Bloor denounces. Thus, although Collins’ and Pinch’s (1979) text on parapsychology was essentially ethnographic and neutral with regard to the actors’ epistemological positions, it could not prevent Gerald Eberlein, blinded by a “normative epistemology”, from denouncing SSK as a “new irrationalism” (Eberlein 1994, 131). This mutual incomprehension is essentially the result of a *category mistake* whereby the author mistakenly applies a *normative* reading to what was essentially a *descriptive* ethnographic text.

As the responses of rationalist philosophers to remarks by constructivist sociologists illustrate, it would be foolish to believe in any easily obtained ideal consensus. But it is by acknowledging that the audience (targeted or constructed) is an intrinsic part of the process of argumentation that we may begin to explain the reception of texts beyond the simple psychological law invoked by Bloor (Perelman and Olbrechts-Tyteca 1988, 22–46). It is obvious that many of the strong programme’s remarks are directed more at philosophers than sociologists, and that many of the latter would no doubt find them curious if not plainly outrageous. It is sufficiently clear, for example, that Sokal’s and Bricmont’s critiques were responding more to the strong programme’s *philosophical* theses than to its sociological methodology. The mertonian sociologist Stephen Cole even believes that the tendency of constructivists to use a philosophical language could have contributed to the fact that, unlike their counterparts in philosophy, few sociologists have actually confronted the constructivists. Cole also thinks that many constructivists, “though not all”, display an “abysmal” ignorance of the major sociological issues (Cole 1998, 277). For Cole, it is evident that the SSK movement was created at the margin of a sociological field, whose members are generally unmoved by relativistic theses, as opposed to the rationalist philosophers who are quick to man the barricades to defend any perceived attack on “reason” or “reality”. Cole’s reaction can also be understood in sociological terms. The strong whiff of resentment emerging from his text can be understood as the reaction of a sociologist who identifies himself more strongly with the general field of sociology than with the SSK subfield and who sees the constructivist “revolution” as superseding and marginalising (thus devaluating) his own work. In point of fact, the very success of the strong programme (at the time of Cole’s writing) completely eclipsed the mertonian approach, which was the dominant research programme up to the mid-1970s. Likewise, the rising criticisms purporting to show that the Strong programme is now *dépassé* in the field of SSK may have forced Bloor to temporarily abandon the debate with philosophers and to take issue with “former friends” in order to defend a threatened position in the SSK field. Thus, the

position of the agent in the field must be taken into account while interpreting a text (Bourdieu 1988). In this way, what is often glossed as individual psychological reactions can be seen instead as the *effect* on a given agent of the changing sociological configuration of the scientific field.

Even the propensity to endorse trendy arguments depends on the position of the agent in his discipline (sociology, philosophy, etc.). Thus, new entrants in the field, for example, have a tendency to prefer to the already established (and occupied) positions those that seem “radical” and “new”. They may then show less concern for the detailed internal coherence of the arguments and be more prone to use them as a weapon to “renew” their field and attract attention, and thus create new positions for themselves in the field. By contrast, well-established agents who feel under attack have every reason to carefully analyse the arguments of their opponent and make visible their weaknesses in order to defend their position. A polysemic text that is explicitly aimed at different audiences (in this case sociologists and philosophers) is thus bound to produce different discursive effects by virtue of its structure and the diversity of its potential readers, who interpret the text in relation to their respective environments, backgrounds and positions in the field (Ceccarelli 2001). As for the transmitter, even the place of publication can influence how his or her text will be read, as journals always presuppose a certain kind of contribution, which the readers then expect to find in the papers appearing in those journals. Yet, all these things being equal, one must not overlook the importance of the argumentative structure of the text itself, although most debates have paid little attention to that aspect.

Interestingly, actors often activate the dispositions necessary to make such close readings when they are under attack, although they do not so readily apply these same dispositions when writing their own texts. For example, in their answer to the critical comments by Cassandra Pinnick on their book *Leviathan and the Air Pump*, Shapin and Shaffer are very attentive to the effects of her text on the potential readers. Noting that she quotes one-half of a sentence from their book and then cites “exactly the same two passages from Hobbes that are found on that page” of the book, they add: “she thus *makes it appear* that [our book] misses the agreement between Boyle and Hobbes [...] and *makes it appear*, too, that [our book] knows nothing of these specific passages from Hobbes” (Shapin and Schaffer 1999, 250–251; emphasis added). Here the authors clearly show how a given sentence can effectively suggest an interpretation to the reader through its very structure, without having to state it explicitly. And if such an analysis of the effects (intended or not) of the texts can be applied to opponents, then it can, and indeed should, also be applied to our own texts in order to better control them, since these effects follow from the phenomenology of reading.

A Brief Phenomenology of Reading

The classic works of W. B. Gallie (1964) and of Arthur C. Danto (1965) on the nature of narrative explanation clearly identified the two major dimensions of a narrative that make it possible to follow a story.²⁸ All narratives propose an order and a sequence, and the act of understanding a narrative is accomplished in a dialectical relation between its

episodic (or chronological) and configurational (or global) dimensions. To follow a story, and therefore to *understand* it, is to reconstitute a whole by combining, while reading a text (listening a story or watching a movie), these two dimensions that convey the general meaning of the narrative. This overall understanding is informed by and consistent with the narrative details contained within the story. To use Louis O. Mink's terms, to follow and understand a story consists of forming a synoptic judgement that draws upon a configurational understanding that takes into account the whole content of the text in a circular relation between the local and the global (Mink 1987). In other words, historical understanding (and sociological understanding we may add) is a combination of local (episodic) and global (configurational) dimensions. From this perspective, the text itself contains elements that *orient* the very process of "conventionalisation" that Bloor invokes to explain why SSK is so often misunderstood.

Take, for instance, Barry Barnes' foundational work, *Scientific Knowledge and Sociological Theory*. The way in which he speaks of "reality" in the body of the book gives the overall impression that it is not an explanatory resource upon which the sociologist may draw, and yet by addressing the question of the role of reality at the beginning of the book he seems to imply the opposite. Indeed, in the very first page of the preface, Barnes notes that: "Occasionally, existing work leaves the feeling that reality has nothing to do with what is socially constructed or negotiated to count as natural knowledge [...] We may safely assume that this impression is an accidental by-product of over-enthusiastic sociological analysis, and that sociologists as a whole acknowledge that the world *in some way* constrains what it is believed to be. *The question however remains: what is the nature of this constraint and how much is it*" (Barnes 1974, vii; emphasis added).

Having encountered this question in the introduction of the book, the reader is thus *alerted* to this problem and can reasonably *expect* to find it addressed again in the body of the work or at least in the conclusion. But it never comes up again, and the question of determining "the nature" of the constraint or its intensity ("how much is it?") is left open as the account instead focuses on the social variables that help shape a consensus concerning reality. This kind of argumentative structure tends to suggest an implicit answer to the reader: if reality does not explicitly intervene as an explanatory factor in the arguments developed in the body of the text, it is probably because its role is nil, or at the very least negligible. This interpretation is even reinforced by stylistic details. Indeed, each time Barnes affirms the importance of external reality in scientific practice, he immediately tempers his account with a caveat. Sentences take the form "of course, but", which tends to emphasise the second half of the sentence where the role played by "reality" (or "material objects") in scientific practice affirmed in the first part of the sentence is relativised. Barnes thus reminds us in a footnote: "Experimentation has, *of course*, played a very important role in the development of science, its importance is *nonetheless overrated, even* within the experimental sciences" (Barnes 1974, 165; emphasis added). Further on he adds: "the result of a piece of creative science [...] must, *of course*, accord with experimental findings *to some extent* if they are to be accepted. *But* the constraint of experiment is *easy to over-estimate*, and its role in securing acceptance of work is *never more than partial*" (Barnes 1974, 88; emphasis added).

Pickering also used sentences of this type, in his study of quarks. After describing experiments performed by many groups to confirm the existence of free quarks on particles of tungsten that produced systematically negative results, Pickering notes that: “As a result of these findings, the tungsten hypothesis was abandoned” (Pickering 1981, 232). And since, in such a formulation, the *cause* for abandoning the hypothesis would clearly be attributed to the experimental results—an “answer” from “reality” as it were, which would be a fairly classic explanation for a realist—Pickering adds, five sentences later: “Nonetheless, *through custom and routine use* [the tungsten experiments] are deemed sufficiently closed by the physics community and therefore evidence from them might be deemed conclusive” (Pickering 1981, 232). Thus, in a few sentences, Pickering transformed what first looked as a material cause (experimental results) into a social one (tradition and habit). Let us repeat here that the point is not to criticise such formulations but to note how they make possible the “relativist” readings on the part of readers and provide the semantic basis of their conclusion that reality (through experimental results) has no *causal role in the narrative*. Other examples would show that the “conventionalisation” tendency noted by Bloor is rooted in the narrative structure of the texts themselves. Taken as a whole, these rare *explicit* formulations cannot help but leave the reader with the impression that reality plays an insignificant role since the sentences are constructed so as to minimise its importance, an impression reinforced by a way of accounting that emphasises social variables as causal factors.

As linguists have shown, in forms “P but Q” the connector “but” creates an opposition that tends to minimise or even negate the role of P (Carel 1999; Ducrot 1980; Lakoff 1971; Ponchon 1993). In the case at hand, it is the *combination* of the uses of “but” and the absence of a causal role given to natural objects in the narrative that orient readers to the conclusion that reality plays no part in explanation of science provided in typical SSK narratives. The “but” *could* be interpreted as simply counterbalancing the tendency to maximise the role of reality in realist narrative *only if* it were followed by examples where the resistance of objects were *also* mentioned positively as constraining possible interpretations, which is not the case in most of the examples given above. This is confirmed by Bruno Latour, who, in his reply to Bloor, notes the absence of objects in constructivist accounts: “It is not enough to say that objects play the role of anchor in our beliefs about the world. [...] The question is to know *which role* objects play. [...] If you make a list of all the roles that things or sensory inputs play in SSK’s narratives, you will be struck by the fact that they don’t do very much” (Latour 1999, 1117). Latour here reveals a major source of the misunderstanding: the question is not to determine whether Bloor (or anybody else) is idealist or not, but to correctly identify the precise role of objects in the causal explanations offered in SSK narratives. Thus, if material causes are to be present as such, they should intervene in the very narration of case studies. If the narrative *focuses the reader’s attention* on social variables, then the reader will conclude (and schematise) the explanation as being *uniquely* social. And just as objects do rarely explicitly *cause* conversions in case studies of the SSK tradition, which insists on interpretative flexibility and contingency, they are conversely at the heart of the explanatory process in the actor-network approach, a presence that in turn explains why the latter is perceived as a return to “realism”.

Conclusion

Taking into account the fine textual structure of narratives provides a way to a more complete explanation of misunderstandings between agents than the approaches that concentrate solely on the *receiver* end and his or her tendency to simplify an account. For in addition to the social variables of the position of the agents in the field, their cognitive interests and the like, one has to take into account the “logic of reading” that is guided by the very structure of the texts themselves. So, in addition to looking at the receiver of texts, one has also to concentrate on the *transmitter* of those texts and more precisely on the linguistic structure of the texts. After all, meaning is conveyed through sentences and, as we have seen, the presence (or absence) of a single word (or even punctuation) can transform the possible interpretations of the text. My analysis in terms of narrative structure suggests that in analysing debates among agents (in the field of SSK or any other one) one should be sensitive to the fine explanatory structure of accounts rather than concentrate on general meta-discourses that serve only to reiterate intentions that are not always put into practice. Hence, by concentrating on general principles (which are materialistic), Barnes and Bloor could claim to having been “misunderstood” by their adversaries whose attention was in fact directed toward the actual content and implicit explanatory structure of their case studies.

In short, in a truly reflexive practice, the art of reading must be combined with a mastery of writing and its *effects*. And whatever the intentions of authors, a poor mastery of the art of writing—or a conscious play on words in order to generate polysemic readings to attract more readers—can only lead to misunderstandings. Once the *form* is brought properly under control, authors and their critics may finally be in a better position to seriously address the *content* of these texts and engage in meaningful intellectual exchanges that avoid unproductive misunderstandings. They might then finally be able to at least agree on exactly *what* they disagree about and *why*.

Acknowledgements

The author wishes to thank Peter Keating, Claude Rosental, Wiktor Stoczkowski and Simon Schaffer for their comments on an earlier draft of this paper. The author also wishes to thank David Bloor for his comments on an earlier draft, although he does not agree with the presented analysis. Thanks also to Steve Fuller for his encouragement, and the reviewers of *Social Epistemology* for their useful comments. This paper further develops an analysis first presented in Gingras (2000).

Notes

- [1] For an entry into this literature, see Engelhardt and Caplan (1987) or browse issues of this journal or *Social Studies of Science*.
- [2] For a recent survey of the linguistic analysis of misunderstandings, see Weigand (1999).
- [3] I am not interested here in the hypothetical and metaphysical case imagined by Steve Fuller of “misunderstandings” that are not detected as such and could thus “persist for long periods because the parties are using much the same language, yet to mean systematically different

- things" (Fuller 1988, 129). I am interested in empirical cases where the misunderstanding is explicitly perceived and stated by one of the parties involved in the discussion.
- [4] By doing this, we are conscious that we lose the symbolic profit of radicality associated with talks of "rhetoric of science"; the expression "argumentation in science" looking more traditional and rationalistic. It is significant that the title of the French version of Perelman and Olbrechts-Tyteca (1988) is "Treatise of argumentation" with the subtitle "The New Rhetoric", whereas the English translation used the subtitle as the main title, thus reversing the original choice of the authors by putting the emphasis on *rhetoric* instead of *argumentation*. For a survey of the literature on "rhetoric of science", see Gross (1996). For a recent survey of the vast literature on argumentation theory, see Tindale (1999) and Doury and Moirand (2005).
 - [5] For an exemplary case linking production of texts and their reception, see Ceccarelli (2001).
 - [6] For example, it is striking that in their introduction to the book *The One Culture? A Conversation About Science* (Labinger and Collins 2001), the editors Jay A. Labinger and Harry Collins say nothing about the "culture wars" that raged in the USA at the beginning of the 1990s and that clearly paved the way for the "science wars". None of the other contributors seem to be aware of that context, and most make a rather artificial link with the old 1950s "two cultures" debate of C. P. Snow, which has hardly anything in common with the culture and science wars. But this is not the place to develop this analysis, since a convincing one would have to take into account the specificity of each country: for example, the "Sokal Affair" in the USA has a totally different dynamic and meaning than its namesake in France. On the culture wars that preceded the science wars, see Linenthal and Engelhardt (1996) and Nash, Crabtree and Dunn (1997).
 - [7] For an example of academic misunderstandings based in part on gender, see Tickner (1997).
 - [8] Traces of these debates can be found in this journal and also in *Social Studies of Science*. The *locus classicus* of a first wave of debate is the collective book published under the direction of Andy Pickering (1992).
 - [9] More often referred to as ANT for "actor-network theory". Although I cannot develop that point of view here, I think that this approach is not a *theory* in any standard epistemological sense, but simply a descriptive *language*. It would thus be more precise to call it "actor-network language" or, to use an acronym, ANL.
 - [10] His methodological resources were Barnes (1974, 1977).
 - [11] For a detailed analysis of Pickering's book, see Gingras and Schweber (1986).
 - [12] The confession is in note 15 at the bottom of the page. For a detailed analysis of the book, see Gingras (1997), and the exchanges that followed in Pickering (1999) and Gingras (1999).
 - [13] For preliminary analyses of the Sokal Affair written in the heat of the debate, see Hilgartner (1997), Lynch (1997), Dubois (1998), Feldman (1999) and Jeanneret (1998).
 - [14] See also Koertege (1998). For a series of exchanges between some of the authors of this book and those they critique, see *Social Studies of Science*, volume 29, number 2 (April 1999). It would be illuminating to study more precisely how *cultural studies* appropriated the work of SSK. All signs point to a largely one-way "phagocytic" relationship of the former with the latter. From a strictly bibliometric perspective, one can readily see that the canonical cultural studies journals have just about never been cited in the leading SSK journals (*Social Studies of Science*, and *Science Technology and Human Values*), while authors connected to the sociology of science movement are widely cited by cultural studies partisans, who are often located in American University departments of languages and literature. Thus a journal like *Social Text*, which Sokal qualified as "prestigious", was in fact rather obscure at the time and has been made "famous" by Sokal's very hoax. It does not figure among the network of journals cited by *Social Studies of Science*. As shown in the *Journal Citation Reports* published by Thomson Scientific, this network includes journals such as *Scientometrics*, *Science Technology and Human Values*, *Isis*, *Research Policy* and other similar journals closely associated with science and technology studies. For analyses of the American context of cultural studies, see Wacquant (1997) and Piessens (1988). On the ties between cultural studies and sociology, see McLennan (1998).

- [15] For a recent attempt of this genre, see Tosh (2006).
- [16] I refer here to the debate *inside* SSK as opposed to the usual debates with philosophers.
- [17] See, for example, the overview of the debates in *Nature*, volume 387 (22 May 1997, 331–335) and in *The Economist* (13 December 1997, 79).
- [18] For another “autobiographical” explanation by Collins of that oft-quoted remark, see Labinger and Collins (2001, 184).
- [19] For a critique of the *validity* of Pinch’s arguments against Rudwick, see Gingras (1995).
- [20] For a sample of these criticisms, see Gross and Levitt (1994), Gross, Levitt and Lewis (1996), Koertege (1998), Boudon and Clavelin (1994).
- [21] The reference to “voodoo” in the title of Cole’s article clearly flags its polemical character.
- [22] The tendency to reduce choices to a dichotomy where there is in fact a continuum of positions is analysed in Boudon (1994).
- [23] For a model of argumentation in a scientific field, see Godin and Gingras (2002).
- [24] For Collins recent “autobiographical” explanation of that oft-quoted remark, see Labinger and Collins (2001, 184, note 1).
- [25] Cited in Pickering (1995, 10, note 16). In a more recent article, Barnes (1999, 55) takes up essentially the same formulation: “reality itself, needless to say, is indifferent to how it is oriented to or described, and never voices any dissatisfaction with whatever specific construction is put upon it”.
- [26] Here is an example: talking about the interpretation of new astrophysical data, a scientist told a journalist: “It’s always possible the universe *is trying to tell us* something”. It is obvious that readers of *The New Scientist* (2 July 2005, 35; emphasis added), in which this interview appeared, spontaneously understood that he meant that the data could suggest a new interpretation, not that the universe was *really* trying to *tell us* something or that the scientist was a kind of mystic who *really* thought the universe was indeed *talking* to him.
- [27] Note that this review was prepared at the invitation of the editor, David Edge, as a gesture to facilitate direct exchange between scientists and SSK scholars in the aftermath of the “science wars”.
- [28] For a more recent discussion, see Ricœur (1984).

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