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SCIENCE, RHETORIC, AND THE SOCIOLOGY OF KNOWLEDGE: A CRITIQUE OF DASCAL'S VIEW OF SCIENTIFIC CONTROVERSIES

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Abstract: *Dascal's position on scientific controversies is submitted to a critical examination. It is pointed out that his distinction between knowledge and understanding, between 'hard rationality' and 'soft rationality' is unlikely to survive sustained critical probing. What is egregiously missing in his approach is a recognition of the role of so-called 'sociology of knowledge' in the way scientific controversies play out. It is argued that, insofar as they constitute pragmatic events, scientific controversies cannot be studied properly without taking into account their inalienable sociological dimension.*

Key-words: *science; rhetoric; sociology of knowledge; coutroversies; rationality; speech acts.*

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> *Natural selection is Darwin's solvent of metaphysics. It dissolved Aristotle's final cause, teleology, into efficient cause, and now Leibniz's pre-established harmony as well.* (Willard Van Orman Quine)

1. SCIENCE AS AN ARGUMENTATIVE ENTERPRISE

Epistemological issues concerning the study of language have played a central part in the long and highly productive academic career of Marcelo Dascal. Dascal's untiring interest in these questions has led him to investigate the history of ideas in such disparate areas as linguistics, philosophy, psychology, semiotics, and cognitive science – to name just a handful. More recently, he has riveted his eyes on controversies as they spring up every once in a while in the world of academics. For Dascal such controversies are far from being phenomena marginal to the history of science. Nor should they be regarded as random incidents that take place – as many innocently think they do – in stark contrast with what might otherwise be considered the long, steady, smooth, and uninterrupted march of science in pursuit of the ultimate truth about the world or whatever. No doubt, this latter view of science has a tremendous appeal to the man in the street who would rather think of the scientist as a person wholeheartedly dedicated to the progress of science and who is willing to sacrifice her material comforts for the sole purpose of co-operating with her peers in a genuinely collective and totally disinterested enterprise called scientific research. In his more recent work, Dascal has shown that the layman's view of science is highly romanticised and bears little semblance to what goes on really behind the curtains in academia. He is currently engaged in what he himself characterises as "a more ambitious project that places controversies at the centre of an account of the history of ideas, in science and elsewhere" (Dascal and Cremaschi (1999), p. 1130).

Now, to regard science as an enterprise marked by controversies and constant clashes of opinion amongst researchers with conflicting and competing views is to admit that scientific research is over and above everything else a *human* affair. The product of scientific enquiry may outlast the lives of the individual scientists involved in it and may even aspire to universal validity, far beyond the contingencies of the particular set of circumstances in which the scientists toil; but the activity itself of the scientists – the way they toil and come up with their findings – is one that is shot through with the multifarious implications of the crucial fact that it is conducted by human beings in the real world. In other words, it is pointless and misleading to look at the work of the scientist as anything but a form of human labour. Controversies arise in science because scientists are humans; and, insofar as they are humans, they are not exempt from such human feelings as the desire for recognition and career advancement, power to influence others and have a greater say in the various decision-making bodies, greater material comfort, and so on and so forth. Furthermore, scientists are part of the very same society as non-scientists and are subject to the very same aspirations and anxieties that other members of the society go through in their day-to-day lives. As Restivo ((1988), p. 5) put it very nicely:

> Even when I carry out scientific work – an activity which I can seldom conduct in direct association with other men – I perform a social, because *human*, act. It is not only the material of my activity – like the language itself which the thinker uses – which is given to me as a social product. My own existence itself is a social activity.

My aim in this paper is to consider science as an argumentative endeavour and look at some of the important implications of holding such a view of science. By way of anticipating myself, I shall contend that one useful way of taking stock of the argumentative character of science

is to regard scientific discourse from a pragmatic perspective – a claim also explicitly made by Dascal. However, appearances aside, it will soon become clear that there are some important differences between us as to the role of pragmatics in scientific controversies which ultimately boil down to crucial differences as to what Pragmatics is all about. Furthermore, I shall contend that the theory of speech acts may provide a useful theoretical framework for comprehending what goes on in science, although speech act theory itself – as indeed it goes without saying – is a product of the very same activity it seeks to theorise and distance itself from. Once again, my understanding of the way speech acts work will turn out to be significantly different from what the proponents of the speech act theory in its received version would have us believe (cf. Rajagopalan (1984), (1993), (1998a), (1998d), (1998e), (2000 a), (2000b)).

2. TWO WAYS OF INTERPRETING THE ARGUMENTATIVE NATURE OF SCIENCE

Although a growing number of scholars today subscribe to the view that arguments and controversies constitute a substantial part of the history of a science, it is not at all clear that everyone understands the term 'argument' in the same manner. Broadly speaking, one may distinguish between two positions in regard to the role of arguments in the advancement of knowledge.

According to one widely-held view, it is thanks to the fact that science is constitutively argumentative that scientists find themselves having to incessantly look for better and more accurate explanations for the problems they isolate and decide to zero in on. On this view, controversies are salutary in that they force scientists to hone their theoretical convictions against the whetstone of criticisms from their peers. Controversies and debates provide the ideal set of conditions that

prevent scientific hypotheses from degenerating into dogmas before they have been properly subjected to close scrutiny by other members of the scientific community and can be reasonably regarded as a plausible theoretical explanations. Controversies and debates are our guarantee that growth of knowledge is achieved through steps that are well chosen and that each new moment of approximation to the ultimate truth of scientific reality is checked and double-checked for its correctness and reliability. In other words, growth of scientific knowledge takes place through the exercise of scientific rationality and one way scientists make sure that they are on the path of rationality is by having their peers keep an ever-vigilant eye on each and every one of their steps. Arguments arise whenever a scientist suspects that one of her fellow-scientists has swerved from the path of scientific rationality.

According to an alternative view, arguments are primarily exercises in rhetoric where rhetoric is understood, not as a means to an end, but an end itself. Truth is what gets established as such. And the so-called ultimate truth is ultimate only to the extent that no one has yet been successful in showing that the story does not quite end where it has been claimed to end. This means, the notion of an ultimate truth or any other transcendental concept cannot be invoked as a point of reference in relation to which one may proceed to adjudge the relative merits and demerits of the contending factions in a controversy. Rather, controversies, including scientific controversies, are verbal battles; and just as it happens in all other battles, truth (i.e. whatever part of the old notion of truth one might nostalgically wish to harbour) is the first casualty in scientific battles as well. Scientific battles are invariably struggles over which of the contending parties is entitled to the truth of the matter; so truth is all a matter of which side has the upper hand in a controversy. The truth is the trophy of the victor in a scientific controversy. The truth of a scientific claim is to be decided solely in

terms of its success in the number of controversies through which it has managed to emerge unscathed. Truth is that which has not yet been knocked down and replaced by a contending view. To paraphrase Popper (1963) in a way that may not be entirely to his liking, truth is merely that which has not been falsified as yet; or, to put the point at its strongest, truth is that which is in principle falsifiable.

3. DASCAL ON ARGUMENTS, SCIENTIFIC CONTROVERSIES, AND RATIONALITY

Where does Dascal stand in relation to the two contending views about scientific argument adumbrated in the foregoing paragraph? In the paper by Dascal and Cremaschi (1999) referred to earlier on, the authors are careful to distance themselves from both the contending views, at least in their radical versions. After undertaking a minute analysis of the exchanges between Ricardo and Malthus in what is recorded in the annals of the history of science as a major *cause célèbre* and pointing out several instances of the use of argumentative stratagems and rhetorical trickery designed to sidestep argument rather than face it head-on, Dascal and Cremaschi ask:

> If this is how the exercise of criticism is supposed to lead to the growth of knowledge, one may well ask whether such a growth is indeed 'rational'. Perhaps, instead, Feyerabend, Foucault, or McCloskey are – each or all of them – right. Perhaps science 'progresses' not through a rational process, but rather through 'anarchy', power struggles, or the rhetorical manipulation of emotions. *Unless one can show that this conclusion is unwarranted.* (Dascal and Cremaschi, 1999, p. 1160; italics mine)

Yet, referring specifically to the Malthus-Ricardo controversy, they also feel inclined not to embrace the diametrically opposite claim that it did contribute to the growth of knowledge in a straightforward and unproblematic way. Thus they say:

> We have analyzed in detail one cycle in the Malthus-Ricardo correspondence. Can we say that this cycle has contributed to the 'growth of knowledge'? Can we say, for example, that it led to the discovery of new facts, to the establishment of new scientific laws? Can we say that it solved the problem of (the economic effect of the Corn Laws) it set out to discuss? The answer is certainly 'No' (Dascal and Cremaschi (1999), p. 1161)

In plucking the two excerpts above out of their immediate contexts and juxtaposing them, my aim is to persuade the reader that, on the face of it, the two passages seem to be on a collision course. But the authors seem to want to have it both ways. For, either science progresses in a straight line or it doesn't. If the Malthus-Ricardo exchange produced plenty of heat but no light, if the debate between the two stalwarts revealed clear signs of " 'anarchy', power struggles, [and] the rhetorical manipulation of emotions" but did not lead "to the discovery of new facts, to the establishment of new scientific laws", then, surely, something is seriously amiss in Dascal and Cremaschi's reluctance to concede that Feyerabend, Foucault, and McCloskey may, after all, have a point when they claim that the notion of transcendent truth is not what is at issue in scientific controversies. How do Dascal and Cremaschi avoid a possible stalemate in their reasoning? The answer is that they opt for a 'third way'. And the third way they advocate is paved with plenty of generosity when it comes to interpreting such key terms as 'rationality' and 'knowledge'. Immediately following the passage just cited, the authors hasten to observe:

> If 'growth of knowledge' is taken to mean more data, solved problems, and agreement on the acceptance of a theory at the expense of another, our cycle did not contribute to it. If, however, this notion is reasonably broadened to include, say, the 'growth of understanding', then the answer is definitely 'Yes'. (Dascal and Cremaschi (1999), p. 1161)

As regards what the authors identify as the Feyerabend-Foucault-McCloskey orthodoxy, here is what they have to say:

> We think Feyerabend, Foucault, and McCloskey are right insofar as they have called attention to important aspects of the scientific enterprise that have been systematically overlooked by the idealized normative image of science that can be found in the writings of philosophers of science like Popper. Methodological opportunism, political struggle, and the temptation to achieve short-lived victory through persuasive tricks are indeed ever present in science, and can be observed at work precisely by looking at scientific controversies, which are not governed only – or even mainly – by lofty logical standards. What this means, however, is not that there is no 'rationality' at work in the workings of science; it only means that a particular kind of rationality, which equates reason with logic, is not found there. (Dascal and Cremaschi (1999), p. 1160)

And the new form of rationality that emerges from the ensuing discussion is baptised 'soft' rationality.

4. 'SOFT RATIONALITY' VS. STRICT LOGIC IN SCIENTIFIC CONTROVERSIES

What exactly is the nature of 'soft rationality' is not fully made explicit by Dascal and Cremaschi, except for the claim that it is not subject to the strict laws of logic. Our first clue as to what the difference might be is made available when the authors equate the two types of rationality with Pragmatics and Semantics respectively. "Pragmatics is ruled by norms which ensure the intelligibility of communicative acts; such norms in contrast to semantic rules, are not algorithmic in nature, but rather heuristic" (Dascal and Cremaschi (1999), p. 1164). For Dascal and Cremaschi then, scientific controversies are first and foremost communicative events and must be analysed with the aid of pragmatics rather than semantics. Semantics is strictly truth-conditional, but since

there is much more in scientific discourse than semantics, it is foolhardy to expect that scientific controversies will be fully subject to the rules of logic. As Dascal put it in his 1983 book:

> When we present an argument, we count on the listener's ability to supply the missing logical links including the obvious missing premisses. When we use deictic expressions, we count on the context to supply their referents. When we concatenate our discourse in terms of given-new alternations, we count on the listener's ability to find the appropriate 'bridge assumptions' in case we fail to provide them explicitly. (Dascal (1983), p. 131)

The above remark could be interpreted to mean that what makes actual scientific discourse and scientific controversies different from logical reasoning is that a lot is left unstated by the participants who require that their interlocutors (or, in the case of controversies, their adversaries) supply the missing elements by appealing to the context. In other words, under *ideal* conditions, scientific controversies should lead to perfect communication between the parties. Dascal's reason for holding that in actual practice scientific controversies fall short of that ideal is presumably that it is conducted by humans and humans, after all, are, alas, too human. "The understanding we have been discussing is not some idyllic union of minds and hearts that poets (and perhaps also thinkers such as Martin Buber) are after. What we are talking about is understanding achieved through regular communication, and whatever rationality it possesses is at least the kind of rationality involved in communicative interaction" (Dascal and Cremaschi (1999), p. 1163).

In other words, scientific controversies occur in conditions that are far from ideal. And the price we pay for this unfortunate state of affairs is that they rarely result in any appreciable growth of knowledge. The best that we can hope for is a growth of understanding – which is, let us not forget, no mean achievement.

> To be sure, communicative understanding is also an impressive achievement, given the ubiquity of misunderstanding, misrepresentation, and other forms of (alleged or real) communicative failure [....]. If natural languages were 'transparent' , i.e., if their semantic rules were sufficient to account for the meanings conveyed through their use, then communicative failures could not occur, and any allegations such a failure should be ascribed to *mauvaise foi* or illiteracy. But the use of natural languages is not transparent. More often than not we convey only implicitly and indirectly what we mean. Understanding thus always requires a complex inferential process whereby the speaker's actual meaning is determined. The theory that seeks to account for such a process is pragmatics. (Dascal and Cremaschi (1999), p. 1163)

5. SOFT RATIONALITY AND THE UNDERLYING VIEW OF LANGUAGE

What is the view of natural language that underwrites the view entertained in the foregoing passage? Before we attempt an answer to this question, let us note that the word *transparent* is somewhat inappropriately used in the passage just cited. When the authors write "If natural languages were 'transparent' " what they presumably mean to say is 'fully explicit', because what is alleged to stand between natural languages and the presumed ideal of perfect communication is the fact that meanings are often conveyed *implicitly* or *indirectly*. Transparency is opposed to opacity, not implicitness or indirectness.

However, I want to insist that Dascal and Cremaschi are indeed lamenting that natural languages are – alas! – unfortunately opaque, albeit in a sense altogether different from the one they are willing to admit. For the whole argument being advanced by the authors is premised on the key assumption that ordinary everyday language (as opposed to the formal language of mathematical logic) disguises thought. If the opacity is due to distortion and if distortion in turn turns out to be one of the design features of ordinary language, then it is not the usual apparatus of

Gricean maxims and inferential rules that will do the job. For the job is no longer one of retrieving lost or missing information; it is one of ferreting out meanings that are often deliberately being withheld as well as meanings that unfold under appropriate prodding and, that too, in ways unbeknownst to the speakers and in spite of their best efforts to repress them.

That language disguises thought is not in itself an outrageously outlandish idea. Among the respectable names with which one finds such a view of language associated is the early Wittgenstein. As a matter of fact, the idea that natural languages are opaque may, for all we know, turn out to be the most durable of our culture's fundamental assumptions about language and one which is well ensconced in a time-honoured metaphysics whose echoes are to be heard in such apparently unconnected things as the Biblical tale of the Tower of Babel and Leibniz's belief in the absolute perfection of the language of mathematics and his certainty that God Almighty must have at His disposal such a perfect language. In a perfect language, one will be able to express one's thoughts without the need to first convert mental concepts into physical signs. A perfect language will therefore be *qualitatively* different from natural human languages in that it need not have any material realisation whatsoever. A perfect language is a language whose existence is wholly virtual.

The idea of opacity arises from the fundamental assumption that language acts as a *tertium quid* between the human mind and the world at large. Since language stands between the world of reality and the sentient mind, it is potentially capable of distorting it. For die-hard idealists like Berkeley and Hume that is a permanent possibility and against which there is no safeguard whatsoever, so that scepticism is what awaits us at the end of the road. For realists and others who are less pessimistically inclined, one can always beat language at its own game, provided one has

the necessary will to do it and has done one's homework properly, with plenty of exercises in Gricean pragmatics and all the rest. If only to complete the picture, there are also the Rortyan pragmatists who would rather have us forget about the whole thing because, in their view, the idea that language acts as an intermediary – a filter, if you like – between the mind and the world has only made us waste our precious time in such idle concerns as mainstream philosophy.

6. SOFT RATIONALITY AND THE PROBLEM OF DISTINGUISHING IT FROM HARD RATIONALITY

For Dacal, thus, what makes scientific controversies all too often inconclusive and not conducive to the growth of knowledge in the rigorous sense is that they are – alas!! – conducted in ordinary language as opposed to a logically perfect language where not only would there be no implicitness or indirectness, there would be no opacity whatsoever, either (For, as already seen, if it were simply a matter of implicitness or indirectness, a pragmatic theory adequately bolstered by insights from Grice would resolve all our woes).

Soft rationality is thus *qualitatively* opposed to hard rationality, or so it would seem. Such a conclusion would seem to be warranted by the claim that, unlike the exercise of hard rationality that results in the growth of knowledge, the exercise of soft rationality leads to the advancement of understanding. And understanding in turn is claimed be a collective rather than individual achievement (cf. Dascal (1985), (1987), (1989)). Furthermore, it does not consist in the deepening of awareness of what is already there in place. The object of understanding – call it consensus or a common meeting point – is actually the outcome of a process rather than a pre-condition for it. This is how Dascal and Cremaschi spell out their claim:

> *Mutual* understanding [...] does not consist in the discovery of an initially hidden meaning, but rather in the joint – even though conflictual – construction of such a meaning. It is an achievement of the contenders, which can only be understood – in fact 'reconstructed' – by an external observer if the polemic context where it develops is taken into account. (Dascal and Cremaschi (1999), p. 1162)

Or, as Dascal put it elsewhere:

> Controversies can in fact terminate without agreement, without thereby becoming disputes. What is interesting is that even in such cases they are perceived by the participants and analysts alike as 'productive'. That is, although they do not permit even a tilting of the balance in favor of one of the positions in conflict, they make a cognitive or epistemic contribution: they clarify the problem, they permit one to identify conceptual or methodological divergencies or difficulties, they reorient the research effort, or simply they produce 'understanding' (Dascal, Ms:9)

But Dascal is not always consistent on this question. For there are other moments when he seems to want to say that soft rationality is not necessarily in a relationship of mutual exclusion with its hard counterpart, but in a relation of partial overlapping. This latter reading suggests itself when Dascal and Cremaschi equate the knowledge/understanding distinction on the one hand and the distinction between soft and hard rationalities on the other with the distinction between semantics and pragmatics – or rather, one way of characterising the distinction.

Before examining the parallel claimed by the authors amongst the three binary sets, let us recall that scholars have long been known to hold widely different opinions concerning the semantics/ pragmatics divide. The gamut of proposals include the plea for an integrated pragmatics (Ducrot (1972), Vogt (1977)), 'garden variety semantics' (Lakoff (1972)),

a semantics/pragmatics divide with "open and permeable borders" (Mittwoch (1976)) – to name just a few. Dascal's own approach to the problem is best understood in light of the following statement:

> Language and other semiotic systems are not only instruments of communication but also instruments of our own thought. Linguistic and non-linguistic signs play an essential role in our mental processes, particularly in higher level cognitive processes and states. (Dascal (1983), p. 44)

Assuming for the sake of the argument that scientific controversies involve some 'higher level cognitive processes and states,' Dascal could be seen as claiming that pragmatics should be taken to cover not just the communicative side of language use but also that aspect of language use that is strictly non-communicative and fundamentally introspective and involves a series of mental operations. Accordingly, Dascal pleads for two distinct subdomains of pragmatics which he calls 'psychopragmatics' and 'sociopragmatics'.

> If pragmatics is a theory of the use of language, the investigation of the calculative, as well as other mental uses of language, belongs *de jure* to its domain. I propose to call this subdomain of pragmatics 'psychopragmatics', to distinguish it from its other main subdomain, namely 'sociopragmatics', which is concerned with the communicative use of language. (Dascal (1983), p. 45)

To go back to the semantics/pragmatics distinction and its relation to the distinction between knowledge and understanding on the one hand and between hard rationality and soft rationality on the other, Dascal and Cremaschi claim:

> Th[e] content [of the speaker's linguistic behaviour] is not forever fixed by the semantics rules of the language. It must be inferentially construed on the basis of the semantics of the utterance *cum* contextual and co-

> textual information. *Pragmatics, therefore, does not replace semantics but rather cohabits with it. Pragmatics permits one to use that system in a flexible way, occasionally violating its rules, without thereby sacrificing intelligibility.* (italics mine) (Dascal and Cremaschi (1999), p. 1163)

To be sure, such a way of putting things changes the picture significantly. Contrary to our earlier impression that hard and soft rationalities have mutually exclusive domains, it now turns out either that their domains somewhat overlap or that they may both be at work in certain discursive domains including, notably, scientific controversies. The upshot of this is the prospect that controversies may produce not just 'understanding' but 'knowledge' because the parties involved may, unbeknownst to themselves, hit the very heart of their bone of contention namely its semantics and, with it, its hard rationality. Because all that is claimed is that the pragmatics of communicative understanding may *occasionally* bend the semantics of each other's arguments and hence equally *occasionally* depart from the hard rationality of robust syllogistic reasoning – which, being algorithmic through and through, admits of nothing but direct hits: there being no room for humming and hawing, nor haphazard or slovenly reasoning. The problem now is that even if genuine knowledge is attained – mind you, by happestance rather than design – neither side would be any the wiser for that!!!

7. SOFT RATIONALITY VS. THE SOFT UNDERBELLY OF RATIONALITY

What the discussion so far has revealed is that there are two conflicting views about soft rationality and its relation with hard rationality which contribute to obfuscating rather than clarifying matters. According to one view, explicitly advocated by Dascal and Cremaschi, the two types of rationality are wide apart, one leading to genuine knowledge and the other paving the way for understanding. Knowledge

consists in a deeper awareness of what is already known to exist, it is added on to what is already agreed to be available to one and all. Understanding is what gets constructed dialogically; the more we talk – even if we shout at each other in order to give vent to our differences – the chances are that we might begin to understand each other better. Understanding does not produce knowledge but – as one might suppose – provides the ideal intellectual 'climate' for the generation of knowledge. According to the alternative view, also discernible from Dascal and Cremaschi's paper, the parties involved in a scientific controversy may occasionally hit upon genuine knowledge but they will have no way of knowing for sure that part of what they think they have created or constructed through their willingness to talk is in point of fact precious knowledge, a deepening of what was already there in place and which – alas!! – they are prevented from ever coming to identify as such. If knowledge, as one fashionable definition would have it, is demonstrably true belief, what the parties in dispute accidentally hit upon can, given the terms of Dascal and Cremaschi's theoretical rigmarole, never be *genuine* knowledge because, although it may turn out to be true, it will not have been demonstrated to be true or acknowledged as such by either side. When all is said and done, what is conspicuously missing in Dascal's analysis of how scientific controversies work and have the positive consequences that they are claimed to have is a clear account of how it is (if it is possible at all) that one can hope to move from understanding to knowledge, from soft rationality to hard rationality.

What the discussion so far seems to lead up to is the conclusion that, rather than positing a softer form of rationality and trying to figure out how one may move from there to the world of pure rationality, uncontaminated by the vagaries of natural languages, one might do better to look more closely at rationality itself and wonder if it has a

highly vulnerable soft underbelly – that for whatever reason has customarily escaped our gaze.

8. INTERROGATING THE RATIONALITY OF REASON AND THE REASON BEHIND RATIONALITY

8. 1. Rationaliy as a Given

The major problem with the proposal by Dascal and Cremaschi has to do with what they take as given, rather than what they propose as part of their explanation as to how scientific controversies are important even whey do not terminate with tangible results. For Dascal and Cremaschi seem to presuppose that the only form of reasoning that can incontrovertibly yield concrete results and with it genuine knowledge is logical reasoning. I am not saying that Dascal is committed to the thesis that logic is reasoning at its most rigorous and that, furthermore, all other forms of reasoning, including the sort of reasoning carried out in scientific controversies, must be defined in relation to it, or rather, in terms of their degree of departure from it. But he certainly talks as if he is perfectly happy with such a view of logic and certainly finds no reason for calling it into question.

In the remaining part of my paper, I wish to sustain the claim that an interesting avenue we may try by way of rectifying the impasse identified earlier on is to begin by questioning the veracity of the very presupposition just identified. Now, to be fair to Dascal, he has consistently shown clear awareness of the fact that, in an all-out controversy, there need not be anything at all that the adversaries agree upon.

> To look at controversies (and at the criticism therein practiced) as 'valuable' just in case they lead to a clearcut decision as to which of the opposing theories should be 'accepted' and which 'rejected' by the

> Supreme Court of Reason (embodied in the absolutely impartial judgment of an ideal Scientific Community) is to overlook the fact that controversies [very often] call into question, among other things, the very 'laws' and 'procedures' according to which such a Court is supposed to judge. (Dascal and Cremaschi (1999), pp. 1160-1)

But then, no sooner have the authors taken such a bold step than they beat a tactical retreat – or so one should think – presumably upon being alarmed by the prospect that they may be poised to skid down the slippery slope of Feyerabend-Foucault-McCloskey brand of scepticism. Thus, the bold statement in the foregoing passage is immediately followed by the following words of self-restraint:

> That is to say, it is to overlook the possibly *radical* character of the differences between the contenders, i.e. strict logic. Such a view implies that, whenever such radical differences occur – as they do in fact occur – the controversy lies beyond the jurisdiction of reason, i.e., it is not properly viewed as a 'scientific' controversy, but rather an ideological one, where rationality has little or no role whatsoever (cf. Popper...). Adopting such a view, therefore, would be to play straightforwardly into the hands of the Feyerabend-Foucault-McCloskey, and also of the early Kuhn, in so far as it would amount to admitting that the *actual* process of scientific 'progress' – where controversies play a decisive role – is the result of factors alien to the concept of rationality. (Dascal and Cremaschi (1999), p. 1161)

In other words, the farthest Dascal is willing to go in characterising the radical nature of the differences in a scientific controversy is the point when a step beyond would have (in his view) the disastrous consequence of letting the unholy triumvirate of irrationality – the Feyerabend-Foucault-McCloskey trinity – get away with the final laugh. In other words, Dascal is prepared to negotiate even the proverbial claim of infallibility of 'strict logic' – thought by many to be the paradigm of all reasoning – provided the idea of rationality itself is conceded to be sacrosanct and unnegotiable, no matter how radical the differences

between the contending parties in a scientific controversy. For Dascal this is imperative, lest a controversy should be allowed to degenerate into a dispute (Dascal, Ms-2: 6). Or, as he puts it elsewhere (no doubt, in tune with a highly prestigious line of thought in mainstream pragmatic research),

> Assumptions of rationality play a key role in most current accounts of the nature of ordinary communication. Communication, on these accounts, is a goal-oriented, cooperative process. At the very least, in order to be able to achieve their communicative (and other) goals in such a process, interlocutors must share some notion of the effective available means of achieving such goals, and must presume that each other abides, on the whole, by criteria of choice that optimize effectiveness (Dascal and Wróblewski (1991), p. 421)

When all is said and done, Dascal's claim that scientific controversies do contribute to the advancement of understanding (though not necessarily – and for that matter *knowably*) is based on his conviction that if the heated exchanges did not take place within the confines dictated by rationality they would not be scientific to begin with. In the final analysis, then, the whole argument is a semantic one. It says that science is rational, so a discussion amongst scientists that does not obey the dictates of reason cannot be considered scientific – or in terms of a three-way terminological distinction proposed in Dascal (Ms-2), such a discussion will not qualify as a controversy but will degenerate into a dispute which is "rooted in differences of attitude, feelings, or preferences" (Dascal, Ms-2: 6). What started off as an epistemological issue has suddenly metamorphosed into a semantic one. The answer to the question "How do you know for sure that a scientific controversy will never overstep the limits of rationality? " will simply be "I speak English, damn it, and *scientific* is an English word and I know when it is being correctly used and when it isn't. If a controversy goes beyond the

limits of rationality, it will no longer be scientific; it is as simple as that". The incredulous interlocutor who persists and points out "But, hold on a minute, I know one famous instance where ..." is most certain to be silenced by what the logicians refer to as a 'No true Scotsman argument' of the sort: "But, well, what you are calling a scientific controversy is not quite scientific in the *relevant* sense of the word"

8.2 The Crisis of Rationality

Can such unconditional allegiance to rationality itself be justified rationally? The answer is an emphatic 'No'. But this has hardly disheartened the die-hard advocates of rationality. The only hitch is that this is exactly the point where the dividing line between reason and faith, between science and ideology, between truth and dogma becomes extremely tenuous. For, unwillingness to discuss the putative unquestionableness of rationality or insistence to the effect that rationality itself cannot be rationally justified amounts to saying that rationality is a matter of faith, that science is itself one form ideology, and that the thesis of the unassailability of truth is at bottom nothing but a dogma. It is, much against Dascal's caution not to play into the hands of Feyerabend and Co., to invite the pithy and ironic remark from Feyerabend to the effect: "If you are keen on waging a war against ideology, by all means, go ahead. But make sure you target *all ideologies, science included*" (cf. Feyerabend (1981); Rajagopalan (1999b)).

As it happens, recalcitrant voices that increasingly call into question the absolute primacy of reason are no longer neither few nor far between. In the words of Calvin Schrag,

> Reason itself, particularly in our time, has become a problem. We can no longer proceed with an untroublesome concept of reason as the ground for philosophical and scientific knowledge. We must submit reason itself to a radical critique (Schrag (1980), p. 103).

Schrag for one is thus willing to push the limits of radical criticism farther than Dascal is willing to go. Or, as Liberman puts it,

> In logic and mathematics (since Wittgenstein and Gödel), in physics (since Heisenberg), in history and sociology (since Gadamer and Garfinkel) and in art and literature (since Picasso and Joyce), the Aristotelian *logos* of Western man – that of a rationality certain of itself investigating the truth of a world presumed to exist just in the way it is being interrogated – has been unravelling. The term of popularity is "deconstruction", but its discovery reached its philosophical age of maturity before that fashionable term with the investigations of phenomenologists, investigations which discovered (as much as demonstrated) that we can no longer presume as an inheritance that there is a secured ground for all reasoned judging. (Liberman (1986), p. 113)

8.3 Struggle for Different Rationalities

I shall wrap up my critical foray into Dascal's views on scientific controversies by examining a *cause célèbre* among recent controversies in the philosophy of language – the one between the French philosopher Jacques Derrida and his North-American counterpart, John Searle. This case is especially interesting in that opinions are sharply divided as to who got the better of the other or, for that matter, if there was any fruitful discussion at all. Predictably thus, for Dascal, the Derrida-Searle encounter is primarily a classificatory problem: that is, one of deciding, first of all, to which of the three categories – discussion, dispute, and controversy – the episode in question must be assigned.

To get matters clearer, let us briefly look at the grounds for the three-way distinction. In Dascal's terminology, a discussion is a polemical exchange and has as its object a "well-circumscribed topic or problem" (Dascal, Ms-2: 6) and usually ends up with the contenders co-operatively arriving at a mutually satisfactory solution. A dispute too is a polemical

exchange but its object is a "well-defined divergence" and, as already seen, "rooted in differences of attitude, feelings, or preferences". A controversy is a polemical exchange as well – in fact, being a polemical exchange is the one common denominator that makes all three comparable – which "begin[s] with a specific problem, but [...] spreads quickly to other problems and reveals profound differences". Dascal hastens to note that controversies do not get "reduced to mere unsolvable conflicts of preferences" .

How do we know that a given conflict has been adequately resolved? Dascal's answer is: "Since 'dissolution' is a form of closure that, ultimately, remains 'external' both to the topic under dispute and to the participants' beliefs and attitudes [...]", the external authority is the scientific community at large. It is the members of the scientific community who will decide who has won. At no moment does Dascal pause to wonder how it is that the community goes about deciding who has won. Or, for that matter, what happens if the community cannot come to a decision as to who has won. Much less still, what are the mechanisms and stratagems that are brought to bear on the issue in order to, say, force the community decision one way or another.

I suggest that an investigation into these questions – that properly speaking belong to the realm of 'sociology of knowledge' rather than 'philosophy of science' or 'epistemology of scientific investigation' – may reveal precious insights into how scientific controversies evolve and are adjudged *post hoc*. Now, Dascal is absolutely right in his observation that, in the Derrida-Searle confrontation, it is simply impossible to say, in terms of his own three-way distinction, whether the exchange was a discussion, controversy, or a dispute.

> From the outset it is clear that this topic should engender a controversy rather than a discussion, since the contenders are well aware that they belong to radically different philosophical paradigms, which question

> each other's most basic assumptions and methods. Searle, however, opts to treat the debate as a discussion, by attempting to show that Derrida's reading of Austin is simply mistaken, and therefore refutable. He assumes thereby that the interpretation of a (philosophical) text is a decidable matter, and that his own decision-procedure is the one that must be applied in order to settle the issue. Derrida's response consists in questioning the assumptions underlying Searle's decision-procedure, which – according to him – need to be set aside in order to allow for a deeper understanding of Austin. At this point Derrida, although ostensively relying upon the 'obvious' controversial character of the opposition at stake, may be simply trying to impose upon his opponent the 'rules' of his own (Derrida's) 'language game'. In other words, he too may be treating the debate, at this stage, as a discussion. When, finally, both contenders' attempts to transform the debate into a discussion abiding by each one's rules fail, both seem to shift to its perception as rather a hopeless dispute. (Dascal, Ms-2: 7)

Three are some crucial moves in the reasoning above that deserve a closer look. What makes it the case that what should have engendered a controversy was hijacked by one of the contenders and transformed into a discussion? From whose perspective are we making such claims? When Dascal claims that, in his response to Searle, Derrida too was probably treating the whole affair as a debate – albeit, in his own sense, different from that of Searle – isn't he also conceding that the word debate may, after all, not have a universal application – i.e., what is a debate in the eyes of someone may be a dispute in the eyes of another? In other words, the fact (assuming that it is one) that Derrida too sees the exchange as a debate does not mean that the two are in a position to come to an understanding in Dascal's terms, i.e., by the exercise of a commonly agreed upon 'soft' rationality – simply because what is at stake is precisely a question as to what sort of rationality can be brought to bear on issues such as the one at hand.

The upshot of these considerations is that the whole attempt to describe the whole episode from a neutral or non-committal perspective

is hopeless from the very start. For, as Henderson ((1994), p. 627) put it, "Epistemology is properly a *normative* discipline" (emphasis added). And because it is normative and not descriptive, any claim made as to what the correct description of a given verbal exchange between scientists should be will necessarily be value-laden (Rajagopalan (1984), (1993), (1998c), (1998d)). Any attempt to adjudge who the winner is or what the precise characterisation of the exchange should be is bound to be itself a gesture of intervening in the exchange and taking sides one way or the other. What this amounts to is that, strictly speaking, one cannot separate the history of a science from science itself (Rajagopalan (1989)). Or, as Nerlich and Clarke ((1994), p. 439) put it more recently: "In a scientific discipline, there is no clear distinction between the history of the discipline and the discipline itself."

Which is not – it is important to stress – the same thing as saying that scientific controversies are ultimately undecidable. They are undecidable only so long as one persists in approaching the whole issue armed with the tools of the trade available in conventional epistemology. That is to say, the issue becomes hopelessly muddled only if we insist on asking which of the contending parties is getting us closer to the truth or at what precise point can we conclude that the exchange is generating some understanding – i.e., not just plenty of heat but some amount of light as well, no matter how dim. These are questions that properly belong to the sphere of sociology of knowledge rather than epistemology. For, the question as to who has won in a scientific battle of wits is ultimately decided by the scientific community at large and the members of the scientific community decide such issues on the basis of the balance of power that is perceived to hold once the dust has settled on the battlefield.

It is for this reason that scientific controversies frequently take on political connotations. The political fallout of the Derrida-Searle exchange is no longer a secret. In the words of Payne:

> What began as a serious philosophical debate – in part over a reading of Austin – finally spilled over into a running scandal in the pages of *The New York Review of Books* and in an attempt by the president of the American Philosophical Association to convince the French government to veto the unanimous election of Derrida as Director of the International College of Philosophy on the charge of *obscurantisme terroriste*. (Payne (1995), p. 6)

In the effort to knock down the adversary, no weapon is discarded. Branding your opponent as a non-philosopher or non-scientist is one that the philosophical establishment has time and again used with great effectiveness against powerful adversaries that include Nietzsche and the later Wittgenstein (cf. Rajagopalan (1998a), (1998e), (2000a)) – and also Derrida (cf. Rajagopalan (1993)). In the aftermath of the debate with Derrida, Searle has frequently been asking the rhetorical question as to why it is that Derridean deconstruction has won over sympathisers only in the English departments in the English-speaking world – meaning, as its unmistakable sub-text, that Derrida, like Nietzsche before him, can at best hope to impress people are who have a literary bent and who, by definition (given the notorious stand-off between philosophy and literature), cannot be philosophers.

9. CONCLUDING REMARKS

Scientific controversies are part of academic discourse whose reigning metaphor is that of war (Lakoff and Johnson (1980)). There is no more reason to think that scientific controversies will be conducted in any way other than the way wars are waged than there is to believe that

academic discourse will be any different from what it is and has always been, at least within the span of living memory. For all we know, the spirit of belligerency may be, as Lyotard has insisted, of the very essence in so far as the human faculty of speech is concerned. In Lyotard's formulation, "To speak is to fight [...] and speech acts fall within the domain of a general agonistics" (Lyotard (1979), p. 10)

Lyotard is alluding to one way of interpreting the Austinian notion of speech acts differently from the way Searle went on to transform it into a workable theory. Searle's interpretation of Austin's thoughts consisted primarily in realigning them with a long tradition of thinking about language according to which the primary function of language was that of stating facts. Unlike Austin who showed clear signs of wanting to break with that tradition, Searle was all too happy reintroducing into the theory the notion of proposition (time-honoured truth-value bearers) and thus make the notion of truth a central component of the theory (Rajagopalan (1990), (2000a)). Lyotard and others (amongst whom one may list McCloskey) who follow a non-Searlesque approach to speech acts are saying that a speech act of assertion is not necessarily (or in any principled way) anchored to the truth of the proposition asserted therein. A speech act of assertion is not thereby an assertion of truth; it is only a *claim* to the effect that the speaker believes he is asserting the truth (This is one way to handle Moore's paradox). Likewise, the one who denies the truth-claim made by one's interlocutor is making an alternative claim for the same truth. Now, *ceteris paribus*, one of the interlocutors must have hit upon the real truth (assuming that the other presuppositions are satisfied in the relevant sense) but neither side can possibly have any guarantee of it over and above a personal conviction of having done it – and a public endorsement of their having done it by the community at large. In other words, there is no logical or otherwise certain move from the speech act

of assertion to the truth of the asserted proposition (Rajagopalan (1982)). If the community at large does come to accept the truth claimed by means of a speech act of assertion, it will not be thanks to the logical force of the argument advanced by the speaker but by the fact that possible dissent to the claim was simply muted by the overwhelming prestige of the speaker or someone else whose authority the speaker successfully invoked (*argumentum ad vericundiam*), the sheer rhetorical power of the argument itself or a covert menace implied therein (*argumentum ad baculum*), the fact that the argument in question involved the hearers at a personal level (*argumentum ad hominem*), and so on. In McCloskey's words,

> Scientific assertions are speech-acts made in a scene of scientific tradition by the scientist-agent, through the agency of the usual tropes, for purposes of describing nature or mankind better than the next fellow [...] The error is to think that one is engaged in the propositional act, which is a matter of formal logic, when in fact one is engaged – all day, most days – in illocutionary acts, which are rhetorical (McCloskey (1984), pp. 105-6)

The winner takes it all. To the winner goes the spoils of war and with it the right to write the history of the war. Understanding is nothing but a euphemism for the state of *détente* that emerges after that the contending parties have already been through a series of bouts and decided that that is just as far as they can go. Understanding is the state of uneasy truce that results from the general perception that neither side is in a position to win outright or score a decisive victory over the other. Dascal's difficulty in showing how understanding can ultimately lead to the growth of knowledge in a principled way has to do with the fact that there is no such thing as a smooth passage from sociology of knowledge to classical epistemology.

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