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THOMAS KUHN, ROCKS AND THE LAWS OF PHYSICS

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The death last June of Thomas S. Kuhn, the most influential philosopher to write in English since World War II, produced many long, respectful, obituaries. Most of these obituaries referred to him as an historian of science rather than as a philosopher. Kuhn would not have objected to that description, but it is misleading nonetheless.

If I had written an obituary, I should have made a point of calling Kuhn a great philosopher, for two reasons. First I think that 'philosopher' is the most appropriate description for somebody who remaps culture - who suggests an new and promising way for us to think about the relation between several large areas of human activity. Kuhn's great contribution was to offer such a suggestion, one which has changed the self-images of many different disciplines.

My second reason is resentment over the fact that Kuhn was constantly being treated, by my fellow professors of philosophy, as at best a second-rate citizen of the philosophical community, and sometimes even as an intruder who had no business attempting to contribute to a discipline in which he was untrained. I do not think too much should be made of the fuzzy philosopher/nonphilosopher distinction, and I should hate to try to sharpen it up. But I found it annoying that people who used 'real philosopher' as an honorific when speaking of themselves and their friends should feel entitled to withold it from Kuhn.

Kuhn was one of my idols, because reading his <u>The Structure of Scientific Revolutions</u> (1962) had given me the sense of scales falling from my eyes. The fact that he came to philosophical issues sideways, so to speak - having taken a Ph.D. in physics and then becoming a self-taught historian of 17th century science-seemed to me a very bad reason to try to exclude him from our ranks.

The main reason Kuhn was kept at arm's length by the philosophy professors is that anglophone philosophy is dominated by the so-called 'analytic' tradition - a tradition which has prided itself on having made philosophy more like science and less like literature or politics. The last thing philosophers in this tradition want is to have the distinctiveness of science impugned - to be told, as Kuhn told them, that the success of science are not due to the application of a special 'scientific method', and that the replacement of one scientific theory by another is not a matter of hard, cold, logic, but comes about in the same way as does the replacement of one political institution by another.

Kuhn's major contribution to remapping culture was to help us see that the natural scientists do not have a special access to reality or to truth. He helped dismantle the traditional hierarchy of disciplines, a hierarchy which dates back to Plato's image of the divided line, a line stretching from the material up into an immaterial world. In the hierarchy Plato proposed, mathematics (which uses pure logic, and no rhetoric at all) is up at the top and literary criticism and political persuasion (which use mostly rhetoric, and practically no logic at all) is down at the bottom. Kuhn fuzzed up the distinction between logic and rhetoric by showing that revolutionary theory-change is not a matter of following out inferences, but of changing the terminology in which truth-candidates were formulated, and thereby changing criteria of relevance. He helped break down the idea that there are 'canons of scientific reasoning' which Aristotle had not obeyed and Galileo had.

He thereby helped make the question 'how can we set our discipline on the secure path of a science?' obsolete. This was the question which Kant had posed about philosophy, and to which Husserl and Russell had offered competing answers. It was the question which B. F. Skinner answered by asking psychologists to confine themselves to a vocabulary dominated by notions like 'stimulus,' 'response,' 'conditioning' and 'reinforcement' It was the question Northrop Frye answered by suggesting a taxonomy of myths, a set of pigeonholes which future literary critics could occupy themselves with filling up.

Kuhn could not, of course, have made this question obsolete all by himself. He was abetted by the self-criticisms of analytic philosophy offered by the later Wittgenstein, Quine, Sellars, Goodman, and others - self-criticisms which were the main topics of discussion within analytic philosophy in the period (1955-1965) during which <u>Structure of Scientific Revolutions</u> first appeared. All these self-critical analytic philosophers had, in their youth, bought in on Russell's suggestion that 'logic is the essence of philosophy' and on his vision of philosophy as a matter of analysing complexes into simples. But then they became sceptical both about the notion that there was something called 'logic' which would guide such analysis, and about the idea that there were any simples into which to analyse non-simples.

Russell's candidates for such simples – what is given in a sensory experience, the clear and distinct ideas of 'and' and 'not' and 'if...then' which are the referents of the vocabulary of elementary symbolic logic – no longer seemed satisfactory. Goodman pointed out that simplicity itself is relevant to a choice of description. Sellars, like Kuhn, pointed out that there is no non-ad-hoc way to divide sensory experience up into what is 'given to the mind' and what is 'added by the mind' Wittgenstein asked *'Why did we think that logic was something sublime?'*. Quine and Goodman, taking a leaf from Skinner pointed out that it might be better to view logic as a pattern of human behaviour rather than as an immaterial force shaping such behaviour.

Nobody suggested that these internal critics of what Quine called 'dogmas of empiricism' - doctrines which Russell and Carnap had taken as self-evident - were 'not really philosophers'. For they did not endanger the professional self-esteem, the habit of self-congratulation, which made even the most self-critical analytic philosophers rejoice in having been born at the right time - a time in which philosophy had become clear, rigorous, and scientific. Kuhn did endanger this self-esteem, because reading his book made analytic philosophers wonder if the notion of „scientific clarity and rigor" was as, clear, rigorous, and scientific as they had assumed.

I made myself unpopular among analytic philosophers by drawing some of the morals which seemed to me implicit in Kuhn's new map of culture. Drawing these morals was a way of overcoming my own earlier training. Carnap and others had persuaded me, in my early twenties, that philosophers should indeed try to become more 'scientific' and 'rigorous'. I was even briefly persuaded that learning symbolic logic was probably a good way of achieving this end. Having been forced to learn the proofs of some of Goedel's results in order to pass my Ph.D. examinations, I became loftily condescending toward philosophers whose training left them unable to juggle logical symbols. But by the time I had reached thirty (just about the time of the publication of Kuhn's Structure) I had begun to doubt whether the best analytic philosophers (e.g., Quine and Sellars) were using anything like an 'analytic method'. It seemed to me that they were just being brilliant, in idiosyncratic and free-wheeling ways.

I also had doubts about whether symbolic logic added more than a stylistic elegance to analytic philosophers' prose, and about whether the famous clarity and rigor on which my colleagues prided themselves (as I too had, for a time) amounted to more than a preference for answering certain sorts of questions and for ignoring others. As far as I could see, what made us 'analytic' had nothing to do with applying a method called 'conceptual analysis' or 'investigation of logical form'. All that united us was that we took certain doctrines advanced by Carnap and Russell seriously enough to want to refute them.

Kuhn's notion of the history of science as a history of what he called 'disciplinary matrices' was a great help to me in formulating this view of analytic philosophy. So was his notion of paradigm. After reading Structure I began to think of analytic philosophy as one way of doing philosophy among others, rather than as the discovery of how to set philosophy, once and for all, on the secure path of a science. This led to a certain edginess in my relations with my colleagues, most of whom thought that Kuhn had shown, at most, that Carnap's picture of 'the logic of science' needed a few minor qualifications. These colleagues did not think that Kuhn's work had any metaphilosophical implications.

Carnap and Russell, I came to think, had suggested something new for philosophy to be, just as had, successively, Aristotle, Locke, and Kant. Each of the latter had created a disciplinary matrix, and thereby a philosophical tradition - a tradition made up of the people who took the founders' terminology and arguments seriously. In this Kuhnian view, analytic philosophy was a matter of testing the utility of the new model which Carnap and Russell had suggested. The model might prove fruitful, or it might prove to be just one more way of rejuvenating tired old philosophical controversies by phrasing them in a new jargon. Only time could tell. But there was no a priori reason to think that either symbolic logic or the famous 'rigor and clarity' on which the analytic philosophers kept pluming themselves, would pay off. There was no reason to think of Carnap's and Russell's model for philosophy as 'more scientific' or even more rigorous, than Hegel's, Husserl's or Heidegger's.

This is not to say that Kuhn showed the notion of 'being scientific' to be empty. Like other vague and inspiring ideas, this one can be filled in, and made concrete, in various ways. One way is to ask whether a discipline can offer accurate predictions, and can therefore be helpful for engineering, or medicine, or other practical purposes. Galilean mechanics was good at this, Aristotelian physics not very good at all. Medicine before Harvey offered fewer confirmed predictions than after Harvey. But Kuhn helped us realise that it is pointless to try to explain greater predictive success, in these cases, by saying that Galileo and Harvey were 'more scientific' than Aristotle and Galen. Rather, by showing that we can predict more than we had thought we could, these two men helped change the meaning of 'science' in such a way that 'able to make useful predictions' became a more important criterion for 'being an able scientist' than it had been previously.

But of course this way of firming up the notion of scientificity is of no use when it comes to philosophy. Philosophers have never predicted anything successfully, and do not try to do so. So for metaphilosophical purposes the criterion of scientificity has to be the ability to get agreement among informed inquirers. The main reason admirers of physics distrust literary critics is that no consensus ever seems to form about the right interpretation of a text: there is little convergence of opinion. At the opposite extreme, mathematicians are usually unanimous about whether or not a theorem has been proved. Physicists are closer to the mathematics end of the spectrum, and politicians and social scientists closer to the literary criticism end.

The trouble is that intersubjective agreement about who has succeeded and who has failed is easy to get if you can lay down criteria of success in advance. If you all you want is fast relief, your choice of analgesic is clear (though the winning drug may have unfortunate, belated, side effects). If you know that all you want out of science is accurate prediction, you have a fast way to decide between competing theories (though this criterion by itself would, at one time, have led you to favour Ptolemaic over Copernican astronomy). If you know that all you want is rigorous demonstration, you can check out mathematicians' proofs of theorems and then award the prize to the one who has proved the most (although the award will then always go to a hack, whose theorems are of no interest.) But intersubjective agreement is harder to get when the criteria of success begin to proliferate, and even harder when those criteria themselves are up for grabs.

Reading Kuhn led me, and many others, to think that instead of mapping culture onto a epistemico-ontological hierarchy topped by the logical, objective, and scientific, and bottoming out in the rhetorical, subjective, and unscientific, we should instead map culture onto a sociological spectrum ranging from the chaotic left, where criteria are constantly changing, to the smug right, where they are, at least for the moment, fixed.

Thinking in terms of such a spectrum makes it possible to see a single discipline moving leftward in revolutionary periods and rightward in stable, dull, periods - the sort of periods where you get what Kuhn called 'normal science'. In the fifteenth century, when most philosophy was scholastic and almost all physics contentedly Aristotelian, both physics and philosophy were pretty far to the right. In the seventeenth, both were pretty far to the left, but literary criticism was much further to the right than it was to become after the Romantic Movement. In the nineteenth, physics had settled down and moved right, and philosophy was desperately trying to do so as well. But philosophy had to settle for splitting itself up into separate traditions, each of which claimed to be 'doing real philosophy', and each of which had fairly clear internal criteria of professional success. In this respect - lack of international consensus about who is doing worthwhile work - it remains much more like contemporary literary criticism than like any of the contemporary natural sciences.

This new, Kuhnian, sociological, view of the relation of the disciplines to one another has made people in many disciplines more relaxed about the question of whether they have a rigorous research method, or whether their work produces knowledge rather than mere opinion. Since sociologists began reading Kuhn, for example, it has become easier for them to grant that Weber and Durkheim were great sociologists, even though neither was acquainted with the powerful methods of statistical analysis in which sociologists are now trained. This permits them to concede that contemporary sociologists who abstain from statistics (David Riesman and Richard Sennett, for example) might be perfectly respectable members of the profession. To take another example: since psychologists began reading him, the question of whether Freudian depth psychology is as 'scientifically reputable' as Skinner's work with pigeons has seemed less pressing. Adolf Gruenbaum is one of the relatively few philosophers of science to care whether Freud produced testable generalisations.

All of the social sciences, and all of the learned professions, have by now gone through a process of Kuhnianisation, marked by an increased willingness to admit that there is no single model for good work in a given cultural area, and that the criteria for good work have changed throughout the course of history, and will continue to change. Though philosophy has been something of a holdout, even there there has been an increased willingness to historicize: to grant that there is no point in dividing the history of philosophy into sense and nonsense, and to admit that even Hegel and Heidegger might have done useful philosophical work.

These post-Kuhnian attempts to substitute a spectrum ranging from the controversial to the non-controversial for the traditional Platonic hierarchy are, however, still staunchly resisted by two sorts of people. One is the kind of analytic philosopher who prides himself on being a 'realist' and who sees what he calls 'relativism' as a clear and present danger to our culture. (John Searle, who once honoured me by bracketing me with Kuhn and Derrida as one of the more dangerous relativists, is perhaps the most conspicuous example).[1] The other is the natural scientist who enjoys his inherited position at the top of an epistemico-ontological hierarchy, and has no intention of being toppled. Such scientists will tell you that 'no real scientist' takes Kuhn seriously.

Scientists of this sort think that they know all they need to know about philosophy of science simply by being scientists. They see no need to reflect on the questions which philosophers of science debate, and about which 'realist' philosophers disagree with Davidson, Putnam, and with Kuhn disciples like myself. They seem to think that philosophers of science should test their views about the nature of science simply by asking native informants - asking their physicist friends, for example, whether they have finally managed to get physics right. Steven Weinberg, a Nobel laureate in physics, is a good example of this way of thinking. Weinberg spoils a recent judicious and sensible article in *The New York Review of Books*[2] about the 'Sokal hoax' (a spoof article offering a defence of so-called 'postmodernist' views on the basis of recent developments in physics) by concluding it with the usual scientists' exorcism of Kuhn: None of us who are really at home in the field take Kuhn seriously.

Here is a sample of Weinberg's attempt to do philosophy of science:

> "What I mean when I say that the laws of physics are real is that they are real in pretty much the same sense (whatever that is) as the rocks in the fields, and not in the same sense (as implied by [Stanley] Fish) as the rules of baseball. We did not create the laws of physics or the rocks in the field, and we sometimes un happily find that we have been wrong about them, as when we stub our toe on an unnoticed rock, or when we find we have made a mistake (as most physicists have) about some scientific law. But the languages in which we describe rocks or in which we state physical laws are certainly created socially, so I am making an implicit assumption (which in everyday life we all make it about rocks) that the statements about the laws of physics are in a one-to-one correspondence with aspects of objective reality. To put it another way, if we ever discover intelligent creatures on some distant planet and translate their scientific works, we will find that we and they have discovered the same laws."

> "The objective nature of scientific knowledge has been denied by Andrew Ross and Bruno Latour and (as I understand them) by the influential philosophers Richard Rorty and the late Thomas Kuhn, but it is taken for granted by most natural scientists."

> "I have come to think that the laws of physics are , real because my experience with the laws of physics does not seem to me very different in any fundamental way with my experience with rocks. For those who have not lived with the laws of physics, I can offer the obvious argument that the laws of physics work, and there is no other known way of looking at nature that works in anything like the same sense."

I imagine that Weinberg thinks he is being as sensible and judicious in this concluding portion of his article as in its earlier portions. But he is not. He is just blowing smoke, abominating beyond his competence. He is throwing around terms ('objectively real', 'one-to-one correspondence', etc.) which have been the subject of endless philosophical reflection and controversy as if he and the common reader knew perfectly well what they meant, and could afford to ignore the pseudosophistication of the people who have spent their lives trying to figure out what sense, if any, might be given to them. He treats Kuhn as a mere paradox-mongered, and feels entitled to do so for no better reason that that, as a physicist, he is the ultimate court of appeal for any philosophical claim about the epistemologico-ontological status of physical laws. The possibility that Kuhn might have rendered the whole idea of epistemological and ontological status obsolete, and with it the distinction between objective reality and some other kind of reality, does not cross his mind.

Earlier in his article Weinberg sensibly remarks that some distinguished scientists draw absolutely fabulous philosophical consequences from what might seem rather limited empirical results. (He mentions Heisenberg and Prigogine; he might also have mentioned Piaget and Eccles.) He rightly rebukes such people for exceeding their briefs. But he does not realise that he is doing much the same sort of thing. He is assuming that he does not have to learn anything about the context of the discussion to which he thinks he is contributing: he can just charge in and straighten everybody out. He thinks that a physicist, just by virtue of being a physicist, knows all that is necessary about the relation of physics to the rest of culture, and therefore can adjudicate philosophical disputes about its relation to other human activities. He does not see that knowing a great deal about X is quite compatible with being rather dim about how X is related to Y, Z, etc. 'What do they know of England,' Browning quite rightly asked, 'who only England know?' As with England, so with the laws of physics.

Compare Weinberg's testimony to his experience with the laws of physics with a good old-fashioned moral theologian's testimony to his experience with the Will of God. This Will, he tells us, is much more like a great big rock than like the rules of baseball. We did not create the prohibitions against usury and sodomy, though of course we can misinterpret them - an experience which, he assures us, is much like stubbing one's toe against a rock. Having lived with the moral law for a long time, and dealt with it on intimate terms, he is prepared to assure us that there is the same sort of one-to-one correspondence with objective reality in morals as there is in geology. The paradox-mongering speculations of atheistic relativists, he explains, are not taken seriously by anybody who is really at home in the field.

Weinberg tells us that all of us, in everyday life, recognize that there is a 'one-to-one correspondence' between what we say about rocks and 'aspects of objective reality'. But ask yourself, common reader, in your capacity as everyday speaker about rocks, whether you recognise anything of the sort. If you do, we philosophers would be grateful for some details. Do both the subject and the predicate of your sentences about rocks ('This rock is hard to move', say) stand in such a relation of correspondence? Are you sure that hard-to-moveness is really an aspect of objective reality? It's not hard for some of your neighbours to move, after all. Doesn't that make it an aspect of only subjective reality?

Or is it that the whole sentence stands in one to one correspondence to a single aspect of objective reality? Which aspect is that? The rock? Or the rock in its context, as obstacle to your gardening endeavours? What is an 'aspect' anyway? The way something looks in a certain context? Aren't some contexts more objective than others? Maybe it is only the rock as viewed by the particle physicist that is an aspect of objective reality (a view favoured by many eminent 'realist' philosophers)? Maybe the rock under other descriptions than the physicists' gets increasingly non-objective as sentences about it get fancier? Or perhaps all descriptions of the rock are on an epistemico-ontological par (a view favoured by many of us 'relativist' philosophers).

And do, while you are at it, tell us more about correspondence, a notion which has given us philosophers a great deal of trouble. Is the relation of correspondence a matter of properly-educated humans' ability to utter non-controversial statements about rocks at a single glance? Is this desirable relation absent in the case of their ability to utter noncontoversial statements about the batter's hits and strikes?

Or is the relevant sort of correspondence a causal, physical, matter (as Saul Kripke has suggested)? Or is the notion of correspondence so hopeless that it, along with that of 'accurate representation of reality', should be discarded from philosophy altogether (as Donald Davidson has suggested)?

I can come up with conundrums like this for as long as you like, and I am willing to bet that Weinberg would not see the point of my raising any of them. The difference between us is that I am in the philosophy business and he is not. I concoct conundrums like that for a living. So did Kuhn. If you don't wish to discuss such conundrums – if you don't want to reflect on what you mean by and 'objective' and 'corresponds' and 'works' and 'not made by us', and if you imagine that you can explicate 'real' by saying 'you know, like rocks' - you had better not think that you understand the epistemico-ontological status of physical laws better than Kuhn did (even if you happen to have discovered a few of those laws yourself). Kuhn and I may be quite wrong to abandon the traditional Platonic hierarchy of disciplines, but you will not in a position to know whether we are or not until you are have engaged in this sort of reflection.

Weinberg's attachment to the traditional Platonic hierarchy is clearest in a passage where he says what Herbert Butterfield called the Whig interpretation of history is legitimate in the history of science in a way that it is not in the history of politics or culture, because science is cumulative, and permits definite judgements of success or failure.

Does Weinberg really want to abstain from definite judgements of the success or failure of, say, the constitutional changes brought about by the Reconstruction Amendments and by the New Deal's use of the interstate commerce clause? Does he really want to disagree with those who think that poets and artists stand on the shoulders of their predecessors, and accumulate knowledge about how to write poems and paint pictures? Does he really think that when you write the history of parliamentary democracy or of the novel that you should not, Whigglishly, tell a story of cumulating? Can he suggest what a non-Whiggish, legitimate, history of these areas of culture would look like?

I doubt that Weinberg has any clearer idea what he means by 'legitimate' and 'definite' and 'cumulative' than of what he means by 'one-to-one correspondence'. But his intent is clear: it is to keep natural science at the top of the cultural pecking order.

I hope it is clear that I do not want to assign science a lower position on this pecking order. Some of my fellow philosophers - the more far-out postmodernists, for example - do seem to want to do that. They seem to think that we philosophers still get to prescribe such orders, and that they have promulgated a new one, which lowers the position of natural science. By contrast, what I want to do is get rid of the whole idea of using terms like 'real' and 'objective' to construct such an order, and to substitute questions about the utility of disciplines for questions about their status. It seems to me as silly to try to establish a hierarchy among disciplines, or cultural activities, as to establish one among the tools in a toolbox, or among the flowers in a garden.

For my anti-hierarchical purposes, I find it helpful to say, with Kuhn, that 'whether or not individual practitioners are aware of it, they are trained to and rewarded for solving intricate puzzles - be they instrumental, theoretical, logical or mathematical - at the interface between their phenomenal world and their community's beliefs about it.'[3] I would interpret this remark of Kuhn's as applying to all practitioners of all disciplines: physics as much as jurisprudence, philosophy as much as medicine, psychology as much as architecture. As I read him, Kuhn gave us a way of seeing the history of physics, of philosophy, of the novel, and of parliamentary government, in the same terms: human beings trying to improve on their ancestors' solution to old problems in such a way as to solve some new, recently arisen, problems as well. Kuhn suggested that in all these areas we could drop the notion of 'getting closer to the way things really are' or 'more fully realising the essence of....' or 'finding out how it really should be done'. For all these, we can substitute the notion of capitalising on past successes while at the same time coping with present problems.

Kuhn aimed, he once said, to 'deny all meaning to claims that successive scientific beliefs become more and more probable or better and better approximations to the truth and simultaneously to suggest that the subject of truth claims cannot be a relation between beliefs and a putatively mind-independent or 'external' world.'[4] This suggestion is, admittedly, a shock to common sense, not to mention to the self-esteem of those accustomed to being at the top of the cognitive hierarchy. But it is the sort of healthy shock which all great philosophers have administered to the common sense of their time. Philosophy is not a field in which one acheives greatness by ratifying the community's previous intuitions.

So much for my protest against Weinberg's attempt to dismiss Kuhn as somebody who lacked sufficiently intimate contact with the laws of physics. But I should end by making an embarrassing admission: Kuhn would have been embarrassed by my defence of him.

Kuhn thought physicists were wonderful, and was dubious about philosophers like me (the only marginally 'analytical', kind - the kind with a lot of literary interests, a fondness for metaphor, and other symptoms of intellectual squishiness). Not only were most of his heroes Nobel laureates in physics, but the more 'clear and rigorous' a philosopher was (the more he sounded like Carnap, roughly speaking) the better Kuhn liked him. As one of his obituaries accurately noted, Kuhn usually preferred his critics to his fans.

In interviews Kuhn took pains to distance himself from 'Rorty's relativism', and from the writings of various other fans who had tried to weave Kuhnian doctrines into the fabric of philosophical positions which Kuhn found unattractive. But, even though we were colleagues for some fifteen years, I never got straight why Kuhn thought I was more 'relativistic' than he was, or where exactly he thought I went off the rails. I always hoped that when he published the book on which he was working in the last decade of his life - a return to the controversies raised by Structure - I would be able to cite chapter and verse to show him that we had been preaching pretty much the same doctrine.

I tend to explain away the fact that Kuhn found my enthusiasm for his work embarrassing by the thought that he sometimes confused criticism of the purportedly exalted epistemico-ontological status of physics with criticism of its aesthetic and moral grandeur. I am at one with him in acknowledging this grandeur. I can enthusiastically agree with C. P. Snow that modern physics is one of the most beautiful achievements of the human mind. I am happy, and unsurprised, to be told by Weinberg that his is still a field in which unknown young people are making the big contributions - a field in which the author of a single paper can acquire an instant international reputation, a reputation which has nothing to do with academic politics, but is simply the prompt and proper reward of sheer brilliance.

I think that Kuhn was so impressed by this moral and aesthetic grandeur that he thought that any attempt to dismantle the old Platonic hierarchy should be accompanied by appropriate gestures of respect toward natural science - traditional gestures which I sometimes did not bother to make. He may have had a point. But I would still insist that getting rid of the old pecking-order, and creating an intellectual environment in which eminent scientists will no longer be tempted by Weinberg's rocky rhetoric, is a very useful project. Kuhn was one of the most influential philosophers of our century because he did as much as anyone else - even Wittgenstein - to get this useful work done.

1) See my *Does Academic Freedom Have Philosophical Presuppositions?* forthcoming in a collection of essays on academic freedom edited by Louis Menand, to be published by the University of Chicago Press. This is a reply to Searle's 'Realism and Relativism: *What Difference Does it Make?*, published in Dadedelus for 1991. ***2)*** Steven Weisberg, *Sokal's Hoax*, New York Review of Books, August 8, 1996, pp. 11-15. ***3)*** Thomas Kuhn, *Afterwords* in Horwich, Paul, ed., World Changes: *Thomas Kuhn and the Nature of Science* (Cambridge, Mass.. MIT Press, 1993), p. 338. ***4)*** Ibid., p. 330