

Interview with David Bloor*

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Could you begin by providing us with some historical background on what motivated the creation of the Strong Programme, and how does it relate to the creation of the Science Studies Unit at the University of Edinburgh?

The Science Studies Unit pre-dated the Strong Programme. As I understand the origin of the Unit, it arose from a suggestion by the biologist C. H. Waddington that scientists should receive teaching in ‘Science and Society’ courses to broaden their education. The 1960s was a period where governments in this country, knowing that science was of ever-increasing significance and that scientists were therefore becoming increasingly influential, held it to be important that science education was appropriately broad, rather than overly specialised. Waddington persuaded Edinburgh University to create a Unit or Department that was to teach scientists ‘Science and Society’ courses. The University appointed David Edge¹, who had moved from radio astronomy at Cambridge into the BBC, where he was doing some science broadcasting. David Edge was acquainted with people such as Thomas Kuhn, Mary Hesse, Imre Lakatos. He knew the philosophers as well as the scientists.

Three appointments were made early on to the Unit, and although there was a bit of coming and going in the early years, roughly speaking, I was the philosopher of science, whilst Barry Barnes was the sociologist of science and Steve Shapin was the historian of science. I was the first appointment to the Unit, the others followed quickly on afterwards. Though there was not an existing syllabus or understanding of what the activity of teaching a ‘Science and Society’ course was, we were given the go-ahead. We were about twenty-four years-old, roughly, it was our first

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¹ David Edge (1932-2003) co-founded *Social Studies of Science*, which has become a central journal in the field of science studies ; D. Bloor, “David Owen Edge: Obituary,” *Social Studies of Science*, 33: 171–176, 2003.

job in all three cases – and our remit was to think up courses and teach them. This we did. We would also talk a great deal to one another: in the early days, the teaching load was not heavy, we would lecture to courses with a dozen people in, although of course we were new, and so were young and were writing our lectures for the first time.

The thing that tends to be called the Strong Programme (Barry Barnes does not use the word very much and I do not think Steve Shapin uses it very much either; it is a label that I introduced¹, so I tend to use it more, although we do agree over fundamentals) emerged from combining philosophy, sociology and history. We also had a shared underlying orientation that just happened to arise because the three of us instinctively thought in similar ways. We had scientific backgrounds and tended to have the habits of mind that arose from scientific training. What we brought together was unified with a common underlying set of habits of thinking, common to the sciences themselves. Barry had previously been a research chemist, and had also done some research on nuclear spin resonance. Steve Shapin was a biologist and had previously done research within genetics and biology. There is in fact a species of mountain moss named after Steve Shapin that he discovered. My first degree was a joint honours degree in mathematics and philosophy, treated as two quite separate subjects. I then went on to do postgraduate philosophy of science work with Mary Hesse at Cambridge, but I decided that what I wanted to study was experimental psychology, which I did. I was very impressed by Cambridge experimental psychology, it was very hard-nosed, it was not Freud and emotion, it was motor skills, perception and learning. I picked up an orientation towards the understanding and analysis of cognition from Cambridge psychology, and that was what I mixed in along with philosophy into the common mixture that I mentioned previously.

That is fairly important to emphasize because, of course, you could get a historian, a sociologist and a philosopher, put them together and they would conceivably hit upon some way of combining their thinking that would be totally different to what happened in this particular case. It would be very different if, for example, the people involved were anti-scientific, or if they were humanistic and had a humanistic orientation which focused on meaning, interpretation and hermeneutics rather than causality. David Edge was a radio astronomer by training, he thought therefore, in some rough sense of the word, scientifically. He knew what he was doing when he was making his appointments. He probably filtered out those he wanted and made sure we had this shared orientation, and the result was effectively the sociology of knowledge done with an explanatory causal orientation.

¹ D. Bloor, *Knowledge and Social Imagery*, 2nd ed., Chicago, University of Chicago Press, 1991 [1976], ch. 1.

Moving to the Strong Programme itself, could you explain to a lay reader the type of relativism that characterises your sociological analysis of knowledge? What theories of relativism do you reject?

I would say the essential feature of every type of relativism must be a rejection of absolutism. To be a relativist acknowledges that the knowledge claims of science do not and cannot live up to the title of absolute knowledge. If knowledge is going to be absolute then it has to be true without any qualification, it has to be known with certainty, to be completely stable and eternal truth: those are the sorts of connotations that give meaning to the word 'absolute'. And it is precisely all of these connotations that the relativist rejects.

So the relativist can say: All knowledge is conjectural, all knowledge is partial, all knowledge is open to revision, scientific theories always have some point at which they break down, scientific theories nearly always – possibly always, but certainly nearly always – get some things right and some things wrong. They get things right to a certain degree of approximation, and some they get right at a better degree of approximation. All of those things mean that you cannot meaningfully attach the word 'absolute' to the knowledge claim, and I would say, from which it follows, that you necessarily must therefore accept or embrace some form of relativism.

Now, yes, there are different types of relativism, although every type must be a rejection of absolutism in order to be a form of relativism. But of course, there are silly ways of rejecting absolutism. For example, somebody might simply say: 'Oh, we do not know anything, do we?' or they might say: 'Yeah, well it's all a matter of opinion isn't it?' and then produce appalling propositions like 'Well, that might be true for you but it's not true for me.' So there can of course be all sorts of sloppy and silly ways of denying that knowledge is absolute, but simply to deny that knowledge is absolute doesn't commit you to any of these silly opinions. You can deny that knowledge is absolute without lapsing into subjectivism, the irresponsible invoking of mere opinion, and things of that kind. The lay reader needs to understand that, in order to be a relativist, you do not need to think that 'anything goes'. That is a form of relativism, but it is a very silly form of relativism, and nobody who is a relativist needs to be that sort of relativist. I can assure you that the relativism associated with the Sciences Studies Unit is not of that kind. It is a much more carefully formulated denial of the absolute character of any knowledge. I reject 'anything goes' relativism, I reject subjectivist relativism, I reject anti-scientific relativism, I reject a relativism based on a highly individualistic conception of knowledge.

Since the Strong Programme has been accused of supporting some of the theses that you just mentioned, I would like to turn to one of them in particular, the 'equal validity' thesis. Anti-relativists consider that relativism acknowledges the existence of several 'equally valid' ways of knowing the world, science being just one of them.¹ Do you have any idea how this objection first came into being?

It came into being as a fantasy construction in the minds of the critics of the Edinburgh school. It did not come from us. It is imputed to us, and it is imputed incorrectly. What I think happens is that equal validity is a misreading for saying that we should adopt equal curiosity, as it were, about things that are true and false. If one thinks about it, the symmetry proposition rests on the presupposition that beliefs are to be divided into true and false. In fact, the very formulation of the symmetry postulate implies the denial of equal validity, at least in the way I articulate it.

You could of course have a form of symmetry which is equal validity, but symmetry as such does not imply equal validity. It is the same sort of mistake that is made about relativism: You can have subjectivist forms of relativism; all subjectivism implies relativism; but not all relativism implies subjectivism. Equal validity may imply a form of symmetry, but symmetry does not imply equal validity, as you can adopt symmetry without adopting equal validity.

I actually do not know anybody who supports the 'equal validity' thesis. I have seen some casual formulations of it, usually by people who are non-specialists in the field. You can find people who appear to be saying something like the equal validity thesis, or things a bit like that, although they are not Edinburgh sociologists of knowledge. No doubt it is currently alive somewhere, but not at the Science Studies Unit. It never has been. I presume it has not been definitely wiped out, but that has nothing to do with us so to speak.

On the other side of the relativist spectrum, Latour has proposed to 'go beyond' the principle of symmetry embodied in the Strong Programme, by considering additional symmetries such as the one between humans and non-humans.² How would you say Latour's 'even stronger' programme has affected the original Strong Programme?

¹ In *Fear of Knowledge. Against Relativism and Constructivism* (Oxford, Oxford University Press, 2006, p. 1–3), Paul Boghossian describes 'equal validity' as the key tenet of a doctrine he identifies as "postmodern relativism," a doctrine which, according to him, "has achieved the status of orthodoxy" in "vast stretches of the humanities and social sciences" (p. 2). 'Equal validity' is attributed by Boghossian to, *inter alia*, the authors of the Strong Programme and to Paul Feyerabend (*Against Method*, Princeton, Princeton University Press, 1985). For an essay in defence of Feyerabend's position, cf. D. Russell, "Anything Goes," *Social Studies of Science* 13: 437–464, 1983.

² B. Latour, *La science en action. Introduction à la sociologie des sciences*, Paris, La Découverte, 2005 [1989].

I think I can answer in one word: adversely. What Bruno did was to conflate Strong Programme relativism with idealism. As one of the many critics who think that Strong Programme relativism amounts to a denial of the material world, he treats the Strong Programme as if it presented a picture of knowledge as a sort of projection of our fantasies onto some blank screen. Now, it is fairly clear, I think, where he got this from. He did not get it from us, he got it from Harry Collins. What Bruno does is to describe some version, some strands of Harry Collins' work, and take that to be equivalent to the Strong Programme. Many people do this, the collective characterisation is a standard manoeuvre. I do not know why. It is quite extraordinary that in the academic world, people cannot distinguish between the positions of different writers, but somehow... I, therefore, do not believe that Bruno's position is, as it were, a 'stronger programme' than the Strong Programme. It looks to me very similar to the Strong Programme in practice, but with those similarities ultimately very obscured.

Could you come back to the debate with Harry Collins?

The debate with Harry is actually the debate about idealism. In other words, this is roughly the extent to which you do or do not accept the existence of an independent material world in one's overall picture of science, and what sort of role you attribute to the external world, as the philosophers call it, in writing one's analysis of science. I think the idealism debate is an important debate in its own right, as it is orthogonal, strictly, to the relativism debate, it is another dimension of debate. Harry thinks that on methodological grounds, one should (he uses this amazing metaphor himself) think of science as like seeing patterns in flames when you look into the fire, like you see pictures in the fire, like a sort of projection process.¹

Collins' methodological idealism is explicitly discussed in the Barnes, Bloor and Henry book². One thing that is done very carefully in the book is to insist that Harry is indeed a methodological idealist rather than the simple straightforward metaphysical idealist. Even though we tend to differ about methodological idealism and possibly metaphysical idealism, Harry's work is not to be criticised in the way I was criticising Bruno's work. Harry is a very concrete, straightforward and bold writer, which does not mean that he is necessarily right or always right, but it is a

¹ Harry Collins has indeed stated that "our perceptions of the world" should be treated as "pictures in the fire" and that these perceptions "should play no more role than the fire in which the pictures are seen" (*Changing Order: Replication and Induction in Scientific Practice*, London, Sage, p. 16).

² B. Barnes, D. Bloor, J. Henry, *Scientific Knowledge: A Sociological Analysis*, London, Athlone, p. 14.

different, and I believe a much more healthy idiom in which to proceed and in which to have to articulate one's differences and disagreements.

The controversy over relativism was a central aspect of the 'Science Wars' launched by Alan Sokal and Jean Bricmont¹. The debate seems to have shifted targets in the very last months. Sokal and his followers now argue, in substance, that the enemy of science is not the 'postmodern academic Left' anymore, but the Bush administration.²

Do they? Well, I am glad they finally made that switch. The 'Science Wars', as I understand them, was a movement by vocal scientists against what they saw as an anti-scientific trend in the academic world, and they identified that trend with something called 'postmodernism'. But one of the funny things about the 'Science Wars' debate and indeed some of the earlier debates is that one finds oneself classified in ways that seem rather strange. The Strong Programme is sometimes identified as a species of postmodernism, but I would be very surprised if that was an accurate perception of the Strong Programme. I cannot speak for the 'postmodern Left', I do not know what the 'postmodern Left' is. The reason I do not know what it is because I am allergic to some of the styles of philosophising and philosophical styles of writing which have come into prominence and which I think belong to postmodernism. I have kept away from them, and so I do not quite know what to think when I am sometimes classified along with postmodernism. I cannot prove that it is false, but I doubt it is true.

With regard to postmodernism, The 'Science Wars' included among the features of that trend something they called 'relativism', and understood Latour, Collins, Barnes, Bloor as amongst the offending parties, with minimal distinction between them. In criticising the relativism of the sociologists of science, they also very typically conflated relativism with idealism; they took idealism to be part of what they called 'relativism'. They understood the sociologists of science to

¹ The expression "Science Wars" refers to the storm of reactions that followed the publication of *Intellectual Impostures*, by physicist Alan Sokal and philosopher Jean Bricmont (London, Profile Books, 1998; published in the United States under the title *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science*, Picador, 1998). The book expands on Sokal's hoax, which consisted in publishing a nonsensical article in the *Social Text* journal in 1996. The authors' argument intends to challenge all forms of relativism, notwithstanding the theoretical divergence between the theories under attack, which results in Jacques Derrida, Roland Barthes and Gilles Deleuze being considered alongside Paul Feyerabend, Bruno Latour and the Strong Programme. Most of the arguments produced during the "Science Wars" appear in N. Koertge, *A House Built on Sand. Exposing Postmodern Myths About Science* (Oxford University Press, 1999) as well as in Alan Sokal's personal archive, "Articles on the 'Social Text' Affair," available online at <http://www.physics.nyu.edu/faculty/sokal/>.

² C. Mooney, A. Sokal, "Can Washington get smart about science?," *Los Angeles Times*, 4 February 2007.

be saying that the material world had little or nothing to do with scientific opinion, or that scientific evidence did not count when scientists were drawing their conclusions. They thought that the sociology of science meant: 'Scientists are socially determined, and are not influenced by the world or by evidence.' This is, conceivably, an acceptable reading of certain isolated things that Collins said, or at least I think it may be a legitimate reading. It is a completely indefensible reading of the Strong Programme.

In your view, has anything useful to the Strong Programme emerged from the Science Wars?

I believe the answer is no. I think it has been a scandalous waste of everybody's time, and the people who are to blame for this are the people who launched the pre-emptive strike of the 'Science Wars'. I think it was foolish and misguided. They were attacking their allies and defining them as enemies, when the real enemies lay elsewhere. The real enemies are absolutists, not relativists, as they might now belatedly be realising.¹

My lecture notes from the 'Relativism' session of your Sociology of Scientific Knowledge course indicate that relativism has raised a lot of emotion and debate, 'some of it civil in its terms, some of it verging on the uncivil in its terms.' Am I right to understand that relativism is still met with fierce controversy and a high degree of controversy, even inside science studies?

I think relativism is still met with fierce controversy and that the debate still continues. I am not sure that there is a high degree of controversy inside science studies, although there can be some conflict within science studies. To take an immediately local instance, my arguing with Latour is a conflict inside science studies². There are people whose position I think is wrong and unfortunate, and these positions have been taken as representative of the sociology of scientific knowledge. Of course it makes me particularly irritated to find what I think of the weaker and the less effective, indeed, indefensible, versions of relativism being singled out as representative, and one cannot wholly blame the critics for that. I think that the 'Science Wars' as a phenomenon was an absurdity, though, and I tried to keep out of it as much as possible. I did not think these people were worth engaging with, although at the same time I was irritated with people like Bruno for giving them their excuse.

¹ *Ibid.*

² D. Bloor, "Anti-Latour," *Studies in the History and Philosophy of Science* 30(1): 81–112, 1999.

I am inclined to think that some relativists genuinely deserve criticism for their idealism, but I do not think they deserve the sort of criticism that treats them with contempt. Witness the recent book published by Boghossian.¹ Although he himself maintains a fairly civilised tone, it is fundamentally dismissive and treats the relativist in a rather condescending way, from on high. The anti-relativist stance, the anti-relativist uproar is an intellectual disgrace. The philosophers go into battle, confident, arrogant, dismissive, contemptuous, when in fact they are completely muddle-headed. I like to think that in a few years' time this will become apparent.

But how would the debate revert? As of today, relativism is still considered to be the defensive side of the argument.

Oh absolutely, yes. I do not know... I can think of no sociological mechanism that is likely seriously to come out on our side. All the arguments are out there, it is not a case of putting more arguments, that would be futile. It has got to be some external thing that tips it. The only conceivable thing is, if religious absolutism becomes ever more oppressive and aggressive, then at some stage people might realise that a means of attacking absolutism might come in handy, and for that, a bit of relativism might be unavoidably necessary. They might start looking back and find that some people have been doing their homework for them. But I doubt it. I think absolutism will be met by a different absolutism. I think we are in for a war of absolute against absolute.

I am afraid that I am not very optimistic, but that does not mean that I am in a state of existential despair: I am not. The important thing to do is not to live in the hope that one's own opinions will triumph in the world. The important thing is to say: Can one find an intellectual and ecological niche where a few rational people can talk with one another in a civilised way, explore interesting questions with one another, and perhaps meet a string of bright and intelligent students and talk to them about these things. It is a little, not a grand aim, but I think it is both worthwhile and realisable, and I think the Unit has provided that. One is never going to change the world and one should not have fantasies of changing it. I certainly do not.

¹ P. Boghossian, *Fear of Knowledge*, *op. cit.* For a detailed critique, see D. Bloor, "Epistemic Grace. Antirelativism as Theology in Disguise," *Common Knowledge* 13(2-3): 250-280, 2007.

Going back almost twenty-five years, the book by Steven Lukes and Martin Hollis book was an attempt to generate a genuine, open exchange between relativists and anti-relativists.¹ Did it help the debate to progress at all?

It probably helped to make the position, the form of relativism, Barry and I were defending, more widely known, because the paper has been reprinted and cited quite a lot. In some sense, it must have helped the debate to progress, although on the other hand, the Lukes and Hollis book pre-dated the ‘Science Wars’: If you take what then happened as an indicator then perhaps it did not help much, although it was clearly a widely known book.

Just one little interesting background note to that. Steven Lukes and the late Martin Hollis were very kind and generous, because – I think I am right in remembering it – Barry and I had written that paper originally at the request of some other editors for some collection on relativism to be published in America. The paper was turned down on the grounds that it was... relativist. In other words, they were really only interested in hearing why relativism is all wrong, they did not really want to have anyone vigorously defending it. So when Lukes and Hollis approached us, we said ‘Well, we actually have got this paper which was turned down, would you be interested?’ They were very interested, and published it. I hope I am remembering that correctly.

Turning to the current state of existence of the Strong Programme thirty years after its creation², is today’s Strong Programme identical to the 1976 version?

I think it is identical, but I think it has been articulated in some quite useful new ways that were only hinted at the 1976 version; I am referring here to the theme of finitism³. The thought behind finitism can be identified in *Knowledge and Social Imagery* in the references to J.S. Mill,⁴ and of

¹ See S. Lukes, M. Hollis, *Rationality and Relativism*, Oxford, Blackwell, 1982 (although the only chapters in the book to fully abide by this guideline are the ones by Barnes and Bloor on the one hand, Lukes and Hollis on the other).

² See also S. Shapin, “History of Science and Its Sociological Reconstructions,” *History of Science* 20: 157–211; “Here and Everywhere: Sociology of Scientific Knowledge,” *Annual Review of Sociology* 21: 289–321.

³ Finitism, which refers back to Wittgenstein’s work on the foundations of mathematics, is covered in another interview of David Bloor conducted by Morgan Huang. See also D. Bloor, *Wittgenstein: A Social Theory of Knowledge*, London, Macmillan, 1983; D. Bloor, *Wittgenstein, Rules and Institutions*, London, Routledge, 1997. For a recent debate on finitism, see the views of Michael Lynch and David Bloor published in *Social Studies of Science* in 1992, and the exchange of views between Martin Kusch and David Bloor published by the same journal in 2004.

⁴ J. S. Mill, *A System of Logic: Ratiocinative and Inductive*, London, Longmans, 1848.

course from the influence of Hesse's work¹, but Barry in particular did a wonderful job in articulating the finitist idea.² That, I think, has added very strongly and effectively to the basic thrust of the programme, without in any way modifying its fundamental presuppositions. Quite the contrary, it articulates these presuppositions.

Another very good articulation of the programme, or a line of thinking that is wholly consistent with the programme, that can be connected to it, and that is a very good step forward, is Barry's development of the self-referential model, a performative model of social institution.³ The fundamental roots of this can be found elsewhere: the Humean interactional theory of convention is, as it were, a mini-version of that.⁴ Barry did a wonderful job in stating those ideas in a very clear and deep manner.

If one will allow those to be seen as steps in the same direction, then I would say that the Strong Programme has survived in its entirety, and is in a very healthy state. That is viewing it as it were from the inside, treating it as an intellectual resource, as a body of thinking. That is not to say anything about whether anybody take notice of it, uses or cites it. I do not know what has happened there. I am inclined to think that most people in our field see it as yesterday's idea, and they no doubt believe that they have gone far beyond it. I suspect they are wrong, and I suspect that if anything has happened, those who think they have gone beyond it have actually retreated from it.

Can you identify other approaches that share the relativist methodological feature of the Strong Programme?

Well, over the years, there certainly has been communication with anthropologists, in particular there has been quite a close link between the Unit and the work of Mary Douglas, which has been quite an inspiration.⁵ Again, that is not to say that there is total agreement on all points, but our relativisms intersect very well, although there are some quite subtle and interesting complications to the epicycles as it were, whose full implications I do not myself understand.

¹ M. Hesse, *The Structure of Scientific Inference*, London, Macmillan, 1974.

² B. Barnes, *T.S. Kuhn and Social Science* (London, Macmillan, 1982), *The Nature of Power* (Cambridge, Polity, 1988), *The Elements of Social Theory* (London, UCL Press, 1995).

³ B. Barnes, "Social Life as Bootstrapped Induction," *Sociology* 17(4): 524–545.

⁴ D. Hume, *A Treatise of Human Nature*, 2nd ed. (L. A. Selby-Bilge, P. H. Nidditch), Oxford, Clarendon Press, 1978 [1740], Book III.

⁵ M. Douglas, *Natural Symbols: Explorations in Cosmology*, 3rd ed., London, Routledge, 2003 [1970].

One very interesting thing about Mary Douglas is that she takes her own Catholicism very seriously. I am sure there are philosophical, theological and metaphysical ways of having effectively relativism at one level with a higher level of absolutism up here. I do not exactly know how she does negotiate it, but she is a very powerful intellect and enormously rich thinker, and it is an education to talk to her. I am sure there could be a lot more connexions with anthropology. The history of theology and the history of religion are also interesting. There is excellent work done by historians of religion, and I have mentioned some parallels between the Strong Programme and the Higher Criticism of the Tübingen school¹. The work that they were doing on religion was very like the work that is now being done on science by the Sociology of Scientific Knowledge, and you can almost call SSK the ‘Higher Criticism of science’ as it were, bearing in mind that criticism there is not just a negative word; ‘higher level of analysis and explanation’ might be better.

Another example that I have in mind, which I only just discovered, is some very interesting writing on relativism by a man called Philipp Frank. Philipp Frank was a professor of theoretical physics at the German-speaking university in Prague, in the twenties and thirties. He took over the chair of theoretical physics from Albert Einstein, who had occupied it for a while before going to Berlin. In the late thirties of course, Philipp Frank had to flee Prague and then went to Harvard, but there he did not teach theoretical physics. He taught philosophy of science, and his philosophy of science was a relativist one. His book on relativism² was published after the war, in 1951. It was written in the late 1940s. It is a defence of relativism. It is very simple, very straightforward, and absolutely spot on target. And it is a very effective description of what relativism is, of why relativism is a scientific view.

I had not realised that Frank had been so clear and forthright on the issue. For the most part, philosophers of science put him down as a positivist, which is perfectly correct, but stay silent about the fact he was a relativist³.

So you can definitely be a positivist and a relativist at the same time?

¹ See D. Bloor, *Knowledge and Social Imagery*, *op. cit.*, ch. 8.

² P. Frank, *Relativity: A Richer Truth*, London, Jonathan Cape, 1951. The book is prefaced by Frank’s colleague and friend Albert Einstein (see also Frank’s biography of Einstein, *Einstein: His Life and Times*, London, Jonathan Cape, 1949).

³ See, e.g., R. S. Cohen, M. W. Wartofsky (eds), “In Honour of Philipp Frank,” *Boston Studies in the Philosophy of Science*, vol. 2, 1965.

I think that if you are a positivist, you have to be a relativist, yes. If you are a relativist, you do not necessarily have to be a positivist, but I think all consistent positivism comes out with a form of relativism. Of course, as with any intellectual tradition, you can start to gloss it and turn it into something else, and you can very often turn things into their opposite. The way to turn positivism into its opposite is to start off by saying 'Knowledge comes through experience' – and the old hard line scientific positivists would treat experience as being a physiological and psychological process of a kind that could be studied in laboratories, like the psychological study of perception. You can then begin to turn it by saying 'All knowledge comes through experience,' but you start treating experience as if it were a sort of revelation of truth, and start saying: 'If you are careful about it and are looking at something in a good light, then you can be absolutely certain of the deliverances of your sense experience, which will give you certain knowledge.' And so you start 'absolutising' the given of experience. You can then start to smuggle absolutism in by having the given as not being itself a natural process, but a supernatural process. Thus you turn into a form of absolutism what used to be a down-to-earth, rather sceptical position, with no room for absolutes.

To a great extent, that is what happened in America with positivism. I think this fairly early stream of European scientific positivism could have been the basis of a very healthy relativist analysis. It got turned into something else and people had to fight their way out of it again.

Is it to say, if I rephrase a bit, that the debate has impoverished through time?

Oh, yes. Fifty years later, we are worse than we were then.

Interview conducted by François Briatte