

## Go Strong or Go Home: An Interview with David Bloor

Zheng-Feng Li · Ruey-Chyi Hwang ·  
Chih-Tung Huang

Received: 10 March 2010 / Accepted: 10 March 2010 / Published online: 2 October 2010  
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**Abstract** Widely read in many areas, Professor David Bloor is arguably the pre-eminent authority in the field of STS (Science, Technology and Society). Since his influential book, *Knowledge and Social Imagery*, was published in 1976, the Strong Programme and its four tenets have become widely known. Professor Bloor was a member of the so-called Edinburgh School who proposed that scientific knowledge could not, and should not, be exempted from sociological analysis. Inspired by Wittgenstein's philosophy, Professor Bloor and his colleagues introduced the concept of finitism to further explain why all knowledge claims are constructed in a specific social frame, to which sociologists have a lot to contribute. Noting the special intellectual position of finitism, this interview started from this topic. We then moved onto the achievements of the Strong Programme in the past 30 years. Professor Bloor also explained his recent work and the way STS beginners discover meaningful case studies. We ended this interview by asking his suggestions toward the burgeoning STS field in East Asia. This interview with Professor Bloor is a step toward a deeper understanding of the Edinburgh School.

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This interview has been translated into Chinese and published in *Taiwanese Journal for Studies of Science, Technology and Medicine*, Number 10 (April 2010): 237–264.

**Editors** Dong-Yuan Tai, Rong-Xuan Chu, Xiang Fang, Miao Hang

Z.-F. Li

Center for Science, Technology and Society,  
Tsinghua University, Beijing 100084, People's Republic of China  
e-mail: lizhf@tsinghua.edu.cn

R.-C. Hwang

Institute of European and American Studies, Academia Sinica, No. 128, Sec. 2, Academia Rd.,  
Nankang, Taipei 115, Taiwan, Republic of China  
e-mail: rchwang@sinica.edu.tw

C.-T. Huang (✉)

Center for General Education, National Chiao Tung University, Room 638, Assembly Building 1,  
1001 Ta-Hsueh Road, Hsinchu, Taiwan, Republic of China  
e-mail: moenhuang@hotmail.com

**Keywords** Interview · David Bloor · STS · The Strong Programme · The Edinburgh School

## 1 Introduction

Widely read in many areas, Professor David Bloor is arguably the pre-eminent authority in the field of STS (Science, Technology and Society). Since his influential book, *Knowledge and Social Imagery*, was published in 1976, the Strong Programme and its four tenets have become widely known. Since then, he has gone on to publish two books on Wittgenstein's philosophy, *Wittgenstein: A Social Theory of Knowledge* and *Wittgenstein, Rules and Institutions*. These publications have made Professor Bloor one of the leading thinkers in the rapidly growing field.

Professor Bloor was a member of the so-called Edinburgh School who proposed that scientific knowledge could not, and should not, be exempted from sociological analysis. Inspired by Wittgenstein's philosophy, Professor Bloor and his colleagues introduced the concept of finitism to further explain why all knowledge claims are constructed in a specific social frame, to which sociologists have a lot to contribute. Noting the special intellectual position of finitism, this interview started with some initial questions on this topic. We then moved onto the achievements of the Strong Programme in the past 30 years. Professor Bloor also explained his recent work and the way STS beginners discover meaningful case studies. We ended this interview by asking his suggestions toward the burgeoning STS field in East Asia.

Before his retirement, Professor Bloor was the Director of Science Studies Unit, the University of Edinburgh. He was trained in philosophy and mathematics at the University of Keele. He then carried out further work in philosophy and experimental psychology at the University of Cambridge. He holds an Edinburgh doctorate in psychology. After his retirement, Professor Bloor maintains an active researching career. He will soon publish a book on aerodynamics.

## 2 Interview

10/12/2008 at Professor Bloor's home

*Chih-Tung Huang (Huang): I'd like to ask about finitism. Could you explain it for our readers and please tell us why it is important.*

Professor David Bloor (Bloor): First of all let me say where the idea comes from. It goes back to some of the empiricist philosophers, the British empiricists, for example John Stuart Mill. He argued that inferences always go from particular to particular. And, in a way that is the central idea of finitism. There are, though, a number of sources. Wittgenstein's work on rule-following is important. He represented rule following as a step-by-step process rather than something that runs along as if it were on predetermined and pre-existing rails. Mary Hesse's (1974) work is another vital source; in particular her important 1974 book *The Structure of Scientific Inference*.

Another way to express the basic idea of finitism is to say that previous applications of a concept or a word do not themselves determine, or fully determine, the next application of the concept or word. There is always a question to be asked by historians

or sociologists about why a concept is being applied in the precise way that it is. The concept has been applied here and here, but why like that? The finitist says there is always a question of this form that needs to be asked by anyone (such as a historian of science) who is analysing the use of concepts. There are always circumstances and causes and potential problems that stand between previous applications and the next application of a concept in science, whether it is an empirical concept (like “water”) or a theoretical concept (like “molecule”) or a mathematical concept (like “polyhedron”). There are no fixed meanings that can be taken for granted.

There are some simple non-scientific illustrations of this which may help to explain why this theme is so important. Consider a government which signs a treaty, or an agreement with another government, and then begins to behave in ways that may not fit the term of the treaty. The participants to the agreement begin to argue about whether the words that have been agreed to do or do not properly mean this or that in the new case. There are some horrible examples of this in recent memory. The former President of the United States [George W. Bush] said Americans do not torture. In the normal previous applications of the word “torture” the appalling fact is that they did subject people to torture. But the American government argued that word was not applicable to what they were doing. They draw new categories or distinctions and talked about “enhanced interrogation”. Of course lots of other governments do the same thing or similar things but the point to notice for the present purposes is how little previous concepts and meanings constrain what happens.

Now it might be said that all these examples show is that people ignore or violate the meaning of the principles they profess and have signed up to. The finitist approach is more radical in its interpretation of what has happened in cases like this. The finitist accepts that the “spirit” of the agreement and the original intention behind it may have been violated but says that such intentions can never be embodied in words or concepts with fixed meanings. The reason is that meanings cannot be fixed. Meanings are always being created afresh in new situations. Does this mean that the finitist has to say that the lies of the Bush administration were therefore acceptable? No, that isn’t the implication. The implication is that a cogent criticism of such behaviour will never derive from merely conceptual argument about the consistency or meaning of verbalised principles.

I have moved a long way from science. I need to say why themes of this kind interest the sociologist of science. Again I need to stress what finitists are NOT saying. The finitist is not saying that science proceeds with the bad faith that attends so much politics and diplomacy. In this respect the examples I used to illustrate the idea were perhaps unfortunate. The reason why finitism is important for historians and sociologists of science is because conceptual change, adjustment, redefinition, reclassification and negotiation play a central role in science. Let me approach this in a particular way. The sociologist is always looking for what might be called “choice points”. Whenever scientists have to take a decision to do things this way or that way the sociologist might have something to contribute in explaining why the decision was made in the way it was. This is easy to see when scientists are reflecting on large matters of policy, e.g. should the science budget be spent on elementary particle physics or should it be spent on genomics? Here it is easy to see how interests and traditions and power struggles might enter into the choice in a way that the sociologist might be able to illuminate. But the finitist says that sociological

curiosity does not just deal with matters of high-level policy of this kind, it goes right down to the details of scientific thinking. In fact the same sort of questions, and the same sort of choices arise on a small scale every time a scientific concept is applied to anything. All concept application involves a sort of implicit "policy". This means that, in principle, the application of a concept could always become a matter of dispute and, in principle, the sociologist and historian might have something useful to say about the implicit policy involved.

Consider the following case. Suppose a scientist has a theory and uses it to make a prediction. Suppose, further, that the prediction appears to be wrong. This might mean that the theory is wrong but there is also the possibility that the theory and the concepts in it have been applied in a way that could be challenged or altered. The scientist might find it possible to adjust the theory and adjust the concepts in it so that now the theory fits the data. Here is one of the "choice points" I spoke about. This choice point was famously identified by the physicist Pierre Duhem. Duhem showed that there are no "crucial experiments" and that a theory that looks as if it has been refuted could in principle always be rescued. Most people do not notice that Duhem's argument can also be run in reverse. The same considerations show that a theory that has been (apparently) disconfirmed might (after all) be a correct theory also show that a theory that has been (apparently) confirmed might (after all) be an incorrect theory.

This is why Finitists say choices of this kind are everywhere. Every single act of concept application implicitly involves an element of choice with consequences that have to be weighed against one another. That makes the entirety of scientific knowledge a potential area for sociological curiosity. It means that some of the old fashioned restrictions on sociological enquiry don't apply. Philosophers have long said that sociologists are allowed to look at, say, large-scale scientific policy decisions but the science that is then done (once the policy decisions have been made) falls outside the scope of sociological explanation. That, say the philosophers, is "pure" rationality in action and has nothing to do with society. The arguments that I have just sketched in order to explain finitism show that the philosopher's distinctions are unreal.

Philosophers also develop other similar arguments. For example, they say that a concept has to have a definition and, of course, some people may prefer this definition while some people prefer that definition. Philosophers concede that definitions are matters of social convention but, they say, when scientists have decided on the definition of their concepts, the truth or falsity of their claims is no longer a matter of society or convention. It is simply a question whether the world fits or does not fit. Convention and relativity and the connection with society no longer play any significant role. This is a very wide-spread philosophical position. The finitist turns this position around and says conventionality, or relativity, does not just apply to some preliminary phase of knowledge, i.e. preliminary arguments about definitions. For the finitist nobody can fix meanings by decisions or definitions. Always and everywhere the application of concepts is potentially problematic so the attempt by philosophers to limit the area in which society and convention play a role is misguided.

To sum up, finitism is a way of thinking about meaning which helps to make visible things that sociologists can study and illuminate. It is a way of arguing against the many philosophers who seek to make the role of society as limited and invisible as possible. I find finitism a revealing perspective that raises deep questions about the nature of knowledge and meaning. A lot of discussions about science are

kept superficial because people have not thought sufficiently about the nature of meaning and concept application.

There is one aspect of the matter that I have not mentioned. Why is finitism called “finitism”? The reason is connected with how meanings are learned. The finitist says that ultimately all meanings are learned or transmitted or communicated by pointing to examples. When a child learns the word “cat” their parents point to cats and say “that is a cat”. In other words, concepts get their meaning through examples. Of course sometimes verbal definitions are given (e.g. “cats have four legs, a tail and whiskers”) but then the words in the definition must be given a meaning so one goes back to examples again to explain these. The important point is that, necessarily, *the number of these examples is finite*. Contrast this with what philosophers say about meaning. They do not deny that the number of examples is finite but they say that the examples somehow generate concepts that go beyond the examples. The meaning that are acquired, or so philosophers say, fix an *infinite* range of possible cases. The word “cat” (say philosophers) refers to everything in the universe that ever was or ever will truly be called a cat. The infinite class of things that fall under the concept is called its “extension”. Philosophers say that concepts like “cat” (or “electron” or “polyhedra” or “+2”) have extensions of a potentially infinite nature and they insist that this is the central and most important fact about meanings. Finitists say “forget about infinite extensions and concentrate on the finite number of examples”. Then you will understand what is going on. Then you will understand why, having learnt a concept by being shown 6 or 7 or 20 examples, there is always a potential problem of how the concept user gets to the 21st or 22nd case. Previous applications themselves don’t determine how the concept is correctly applied next time.

All sorts of facts enter how a concept is applied. Different people shown the same examples or instances of a concept could apply the concept in different ways, because they have extracted different things from the examples. They have created a different understanding, or connected the examples to different purposes, or fitted them into a different background framework. All of these things are intimately related to the fact that “definitions are based on a finite number of examples”. And that’s why the position on which a lot of sociology of knowledge is based is called “finitism”.

*Huang: In your book, you used a term “sociological” finitism (Bloor et al. 1996). It seems to imply that there is another finitism which is “not” sociological.*

Bloor: I accept that other writers sometimes use the word “finitism” in a slightly different way to refer to slightly different things. Amongst mathematicians, for example, there is a range of different ideas about what constitutes a proper proof procedure or a proper and meaningful mathematical operation. The word finitism is sometimes used in that context to mean that a proof must have a finite number of steps. Nevertheless, I think this is analogous to how I have been using it. It is not identical but it is similar. Perhaps it was to differentiate the sociological position from this (already existing) mathematical use of the word that I spoke of sociological finitism.

*Huang: That is why I think maybe there is a “non-sociological” finitism...*

Bloor: I have sometimes connected the word finitism with another word and referred to “meaning-finitism” (Bloor 1997). This was because I was drawing a distinction between meaning *finitism* and meaning *determinism*, which is a development of the idea that you can “fix” a meaning. My position is that meaning determinism is wrong. Meaning determinists think that meanings can determine

what a person does, for example in following a rule. The meaning of the rule determines whether the subsequent acts of rule following are correct or not. I remember drawing that contrast.

*Huang: You mentioned “we” discovered finitism. I suppose you were talking about Barry Barnes, or John Henry. Who are “we”?*

Bloor: Certainly Barry and I have done a lot of work on finitism. Barry has done some of the clearest and most explicit work and has done it very well indeed. The account he gave in his book on Thomas Kuhn is very good (Barnes 2003; Barnes 1982). Who else has taken up the notion of finitism? Donald Mackenzie has, Steve Sturdy has, Martin Kusch has, Massimo Mazzotti has. And certainly a number of other people use the idea and know how to use it.

*Huang: Can you talk about the similarities or differences between you and Barry?*

Bloor: I think we are very similar in our outlook except he is much cleverer than I am. I think though that we have slightly different styles in our work. Barry is very sociological in the way he thinks. I am probably a bit more philosophical in the way I think. Because of the deep sociological insight in his work I am a great admirer of his writing. I do not agree with every sentiment or opinion he expresses but he has a very penetrating way of developing his arguments. I have learnt an enormous amount from him. I am glad to say we still spend time talking to one another. It is very good that he has come back to Edinburgh after being away so long.

*Huang: To talk about you and Barry, I think we have to talk about the Strong Programme. In Taiwan or China we tend to use the umbrella term of “Strong Programme” to understand the Edinburgh School. Do you think it is meaningful in any sense to use this kind of term?*

Bloor: Yes, I think it is meaningful. The words “Edinburgh School” are very widely used. I think it is perfectly legitimate though, of course, it is an approximate way of speaking and care should be taken to acknowledge that people with a variety of different opinions and approaches have passed through the Science Studies Unit at various times. There has never been a “party line” and I hope there never will be. I should perhaps also explain that the label “Edinburgh School” is one that other people use to talk about us. It is not a label we tend to use among ourselves. People at the Science Studies Unit were not sitting around the dinner table one day and said: let us call ourselves the Edinburgh School. We discovered that other people spoke of us as the Edinburgh School<sup>1</sup> (Henry 2008).

*Huang: In your previous interview,<sup>2</sup> you also talked about the Strong Programme. But in that interview you said “I” basically use the term.*

Bloor: I tend to use the term. Barry and Steve (Shapin) did not and don’t use it very much. I don’t think that this difference is an important matter.

*Huang: So your ideas are very close with each other?*

Bloor: I think so. I only wrote down this programme in the 1970s in an attempt to identify the opinions and ideas that I thought were common to a number of people,

<sup>1</sup> About the history of the Edinburgh School and the Science Studies Unit see: Henry (2008).

<sup>2</sup> This interview was conducted by our colleague, François Briatte (f.briatte@ed.ac.uk). His interview covers some important topics, such as the creation of the Science Studies Unit, Science Wars, and relativism. A French translation of his interview was published in *Tracés* 12: 215–228, 2007 (available online at <http://traces.revues.org/index227.html>).

and not just in Edinburgh. I quoted and described what quite a few historians were doing. I was trying to make explicit what I thought was implicit in a lot of work. I think I correctly identified a pattern of thinking that was quite wide spread. This is what I think the Strong Programme is and how I tend to think about it and how my colleagues in Edinburgh think about it.

Some outsiders seem to believe (or they write as if they believe) that one day somebody said: "Oh, here is a programme. Let us write this down and now do some work that follows this programme." It was not like that. It was the other way round. Work had been done by many historians and sociologists and I thought "how can I characterise this work?" What are its essential features and what assumptions does it rest on? The programme came out of the work. The work did not come out of the programme. Perhaps for this reason "programme" was an unfortunate word to include in the label because it suggests guidance rather than following and summarising. I did not think of this at the time and now it is too late to change the title.

*Huang: Can you briefly explain why the Strong Programme is "strong" for our lay readers?*

Bloor: Some people say that the sociologist can legitimately explain deviations or failures of rationality but cannot (and should not try to) explain successful and rational thinking. Sociologists can explain failures but not success. They can explain error but not knowledge. That's what I called the "weak programme". The Strong programme says the sociologist has a role to play in understanding not just deviations from rationality but what we called rationality itself. Good science as well as bad science. Critics sometimes think that the word "strong" here means that those who follow the programme believe that "everything about knowledge is purely sociological". Critics think a "strong" sociological perspective means saying there is no psychology, no biology and no material world. This is wrong. The strong-weak contrast isn't meant to offer a choice between saying that everything is sociology or not everything is sociology. That is not the distinction.

The obvious reason why the distinction does not work in this way is because the idea of "everything" being sociological doesn't make any sense. It is an incoherent idea. We have a society because we are biological organisms of a certain kind and we are biological organisms because we live in a material environment. So you cannot have everything sociological. Yet, time after time, people respond to the Strong Programme as if this is what its adherents are saying. The critics say: Don't you think the material world has got something to do with science? Of course I do. From the very outset I presented the Strong programme as resting on a background of materialism.

*Huang: Can you explain the achievements of SSK (Sociology of Scientific Knowledge) from psychological, sociological and philosophical dimensions?*

Bloor: Those three disciplines, philosophy, psychology, sociology and history are resources that can be and have been drawn upon, for example, by historians of science. They are not the foundations of the programme so much as the resources which can be drawn upon in order to pursue the programme. It is very difficult to be asked to say what the achievements are because it might seem like an invitation to boast, and one should not boast [laughing].

Please remember here what I said earlier: that the work doesn't follow the programme; the work existed before the programme and, of course, has continued since but, in general, not because of the programme. In formulating the programme I was, of course, trying to identify "good work". Perhaps the question should be: what are the achievements of the tradition of work that I consider to be good work? I think the answer is that some very perceptive studies; particularly of a historical nature, have been carried out. Those studies show us something about science now, in the present, as well as about scientific thinking in the past. I think they have shown us what a very complicated and fragile thing science is. They have shown how many contingences it depends on. The studies show how impossible it is to lay down simple rules or to make any predictions about science, about where science is going, or even to make recommendations about how one should do it.

So, perhaps one of the main achievements of the tradition should be to tell us that we should be very careful when formulating science policies. It is almost bound to be the case that the reality we are trying to control or legislate for is far more complicated than we imagine. We are in the position of a person trying to work a very complicated machine with only a few very simple ideas about what will happen if you turn this handle or pull that lever. The machine is really beyond our understanding. Don't be confident that if you pull a lever you will be able to predict what the effect will be. You asked about achievements but this is a rather negative thing, nevertheless it is valuable to know the limitations of knowledge.

One of the major sources of problems in the world has been over-confidence in thinking we can control or predict the outcome of events and human actions. I don't think one should be too pessimistic or think all control is impossible. I don't think one should give up. It is legitimate to want to control our culture and institutions, but it is appropriate to moderate one's enthusiasm with a sense of the difficulties. This is better than having naïve theories or naïve policies based on naïve pictures of science. Not a very great achievement, is it [laughing]? It's rather a negative achievement.

I really should perhaps try to say something of a more positive kind. Perhaps I should say, the greatest achievement is to give us a more realistic understanding of ourselves and our own thinking. There is a sense, if one thinks about the quality of intellectual life, that we now have a deeper understanding of our cognitive predicament, and we have deeper understanding of who we are and what we are doing now than we had before.

We may not be able to use that knowledge in an immediate way. But if we are intelligent human beings, then we should value understanding where we are and what we are. It is valuable in its own right to know the ground we are standing on. It is almost a form of philosophical existentialism. I am talking about appreciating the truth about the human condition. I think we should try to appreciate the human condition in all its complexity and possibly in all its bleakness. I think there is nothing worse than seeing people going through life in a fantasy world, with superficial and self-serving beliefs about the past and the future and ourselves and society. Having a more realistic view might not do us any good. It might not be useful. But it's better to understand, even if it makes us unhappy.

*Huang: Can you talk about your current work. I have heard you will have a new book coming soon.*



Bloor: I hope it will be “soon”. I am finishing a book about the history of aerodynamics but it has taken a long time. The reason why it has taken a long time is because it is a very detailed book. It is concerned with theories about a phenomenon called “lift”. The question is: Why does an aircraft wing work and keep the aircraft up. There was a disagreement on this question between British scientists and German scientists. The disagreement lasted from about 1906 until about 1926. German experts and British experts went in different directions in looking for an answer to fundamental questions in aerodynamics. I am looking at this difference of opinion. Notice that this disagreement went right through the First World War, 1914–1918. Only after the war, in 1926, did the two bodies of experts come together in their opinions. I am studying this as an exercise of the Strong Programme in the Sociology of Knowledge. I want to know where the disagreement came from and what sustained it. The research involves looking in great detail at the reasoning of the scientists concerned. It involves going into the mathematical detail.

*Huang: It sounds like this book is about the history of science or a history of debates.*

Bloor: Yes, that is exactly what it is.

*Huang: According to Professor Jasanoff ...there seems to be a gap between history of science and the discipline of science studies. She published an article (Jasanoff 2000) in the journal and talked about how to bridge the gap. Do you think there is a gap?*

Bloor: No. I see things rather differently. Professor Jasanoff argued that while people in science and technology studies are open to work in the history of science the historians themselves kept their distance from work in the sociology of science. This has not been my experience. I have always found historians very open to sociological ideas. This sense of unity of purpose is perhaps not surprising since my entire career has been spent in the Science Studies Unit in Edinburgh and the Unit has always had at least one historian as a leading member of the group. With a list of distinguished colleagues which has included Steve Shapin, John Henry and Steve Sturdy my experience of historians of science does not fit the pattern described in Professor Jasanoff’s lecture. The question in the title of her address “Can Science Studies and the History of Science Live Happily Ever After?” hardly arises for me because in my experience they have grown up together and take one another for granted. To take just one example, think back to Donald MacKenzie’s work on the history of British statistics. This started as a doctoral dissertation in the Unit in the 1970s and was published as a book in 1981 (MacKenzie 1981). MacKenzie combined historical work with sociological and social constructivist ideas. I could produce many examples both from within the Unit and outside the Unit and cite historians from London and Cambridge and elsewhere who combine sociological sensitivities with detailed historical work on science. I should add that over the past few years I have had the privilege of working for some periods of time in the Max Planck Institute for the History of Science in Berlin and I have never encountered the resistance mentioned by Professor Jasanoff. Of course there are always a variety of opinions to be found, but the whole environment seems to me to be one of open, not closed minds.

I could never imagine feeling the need to address even the faintest reproaches to historians of science. Perhaps Professor Jasanoff's experience in these matters is just different to mine. Perhaps the British and German scenes are different to that in certain parts of the United States. I don't know. If I were to identify an academic group who should have been, but have not been, receptive to sociological ideas it would be philosophers of science rather than historians of science. There are some shining exceptions but in general there has been a routine and dismissive hostility from this quarter with no serious attempt to come to terms with work in the sociology of knowledge.

*Professor Zheng-Feng Li (Li): I want to know how you think about the situation of STS today.*

Bloor: I find that a difficult question to answer. The reason is because I feel that my knowledge of the literature is not very broad and nor is my knowledge of what is currently happening in the field. Certain limited areas I know deeply but I don't have a broad knowledge. This is not something that I am proud of. On the contrary it is something I am rather ashamed of. It has happened because I proceed in a very narrow way. I am very obsessive about my own work. So lots of things, lots of books, which I should read I don't read. So I am not confident that I know what the state of the field is.

Of course I have some impressions, but they are very subjective. I get the impression that many people in STS believe that they have progressed beyond the Strong Programme. They feel that the programme may have been interesting in the 1970s, but today they take themselves to be doing something different or deeper or more interesting. But my suspicion is that what has happened is that many people have actually found the Strong Programme too difficult. I suspect that they have set themselves goals which are not as demanding. They see that the Strong Programme involves finding causal explanations but that is very difficult so they invent sophisticated reasons for refusing to think about science and scientific activity in a causal manner. They exploit a long-standing tradition in which it is said that causation applies in the physical science, but the social sciences are concerned with meaning, interpretation and understanding. According to those who follow this tradition those working in science studies should use interpretive or hermeneutic methods *rather than* causal methods.

To me this sounds like saying (in effect): "Let's make life easier for ourselves". Let us not struggle and study until we find causes for things. Let us move quickly from one thing to another showing the diversity of interpretations. And then let us say this is the new way of doing things which is superior to the old way.

I think this is an evasion of the deepest and most interesting questions. When I suspect this is happening, then I think the field is not going forward. I think the field is going backwards. Of course, when people are 66 they always start saying grumpy things like that. Perhaps I am just getting a bit old.

*Huang: Before we ask our final question, I am curious about, as a student in the Science Studies Unit, how to choose a good and meaningful case study. We have got a course called case studies at MSc level [Case Study Methods in Science and Technology Studies: Theory and Practice]. How to choose a good case to conduct research?*

Bloor: The answer I would give may sound a little bit like a joke. I would say: choose the smallest subject that you can find. The reason is that almost every small

issue, when you look at it, and when you really turn the microscope down and look into the detail, will be found to have many connections and ramifications. What looks small will turn out to be complex but at least you might be able to describe this small event. If you choose something too big at the outset, then you will never come to an end, and you will say general and superficial things.

Choose something small, but choose something where you can identify a certain definite problem. For example, see if you can find a controversy, but not necessarily a big or famous confrontation. Incidentally, many people misunderstand the interest in controversy. They think that there are controversies in science but also lots of things are not controversial. They conclude that if you are interested in controversies, you are not interested in all of these other things. You have only looked at few special cases. But that is actually wrong. The reason why a sociologist or historian looks at controversies is to understand what goes on when and where there isn't a controversy.

The study of controversy will help to make the facts of knowledge formation visible, and these facts will still be present and relevant even if they are invisible in the non-controversial areas. What sort of facts? For example: the operation of certain interests, the influence of institutions, disciplines and traditions. So if you can find a small controversy you have almost certainly got something interesting and revealing to study. Small-scale controversies are everywhere in science. The work I am doing on the history of aerodynamics is a small controversy study. It concerns different views of the flow of air over a wing. In terms of fundamental science it was not comparable to the controversies over relativity or quantum theory or evolution but some surprising things, and some deep things, have come out of it.

In the course of the work, I also came across all sorts of things that would make interesting studies in their own right. For example, during the First World War, lots of factories had to produce lots of aircraft. These were very primitive aircraft, but you have factories producing large numbers of them ready to be sent to the Western Front in France and Holland. Now you have manufacturers making them, and the government sent in government inspectors to look at them. Of course there were controversies. The inspectors would say no, this is bad quality control, that's not good enough, go and built those again. And the manufacturers will say: "That's unrealistic; that's wrong; we cannot do that; this is good enough or that is your responsibility not our responsibility".

As soon as I saw this I thought somebody could write a PhD on this. They could look at the conflicts of interests, look at how these conflicting interests are expressed in detailed judgement of the quality of the wood, the quality of the metal, and the quality of the workmanship. What work counts as adequate workmanship, what sort of evidence is used to say the workmanship was not good?

So if you know what you are looking for (and this is where an understanding of finitism is important) you don't have to find a great dispute between Einstein and Bohr on determinism or something like that. You can find a good research topic for a case study in almost every aspect of daily life where there is a little friction, where there is a problem. Keep looking, and you will find it [laughing].

Find some little clue of that kind, then follow it up, follow the references. You will soon discover, oh there is a bigger argument here than I had realised. You start with one little footnote, then it will become four footnotes and it will grow like a

snowball. Soon you have got the literature of a controversy. Get 20 papers and you are on your way to a PhD.

*Huang: Before I ask my final question, you two have any follow-up questions to ask?*

*Li: I want to know that are there any changes after your construction of the Strong Programme.*

Bloor: Have there been any changes? I don't know whether to be proud or ashamed when I say no.

*Huang: But a lot of people say that finitism is a turn for you. Is it true?*

Bloor: It is undoubtedly a development. And it is a growth area in the work that is being done.

*Li: Deeply?*

Bloor: But I believe that I can point to the presence of finitism in the very early publications. But there is no doubt that this theme has grown in importance. But of course it was present in Wittgenstein's analysis of rule following, although without using the word. That was a finitist analysis. The justificatory arguments that were introduced right at the beginning of the Strong Programme appealed to Wittgenstein's work on rule following.

A lot of critics miss the fundamental role of Wittgenstein's arguments for the Strong Programme. When they hear supporters of the Strong Programme saying, "you will find social influences everywhere in science" the critics think this is just a dogmatic assertion, or a rather foolish generalisation. The critics want to say you may find social influences in some places but not everywhere. Sometimes science goes its own way without social influences. That's what they say and they believe they are being realistic and prudent.

The virtue of Wittgenstein's argument about rule following and the importance of the argument is that he takes a case where there appear to be no social influences, e.g. a case where the behaviour appears to be determined purely by meaning and logical composition. Consider the act of following the rule 2, 4, 6, 8, 10. Where is the social influence? Wittgenstein said, let us concentrate on that case. And then he examines how somebody might learn that rule. He studied what confident rule followers, or teachers, might say if somebody failed to learn it, or started learning it in a non-standard fashion. He showed, I think very convincingly, that all you had ultimately were various social influences. There is nothing else.

Consider the resources at the teacher's disposal. They will invoke the meaning of the rule or try to encourage the pupil to go on in the same way as the finite number of examples that have been given. What makes this "the" meaning of the rule? What makes this going on in "the same" way? What does "the same" mean? On every occasion the answer will either be more examples or it will be an attempt to specify a rule for following the rule. Of course, a rule for following a rule just takes you back to the original problem. Ultimately the teachers have nothing other to offer than: "we do it like this". The pupil must learn to do it like the teachers or they are not competent. What we do collectively is what counts as the competent way to follow that rule, and what we do tells you the meaning of all those words that we used.

Some philosophers say we should "disentangle" reasoning from social influences. And Wittgenstein, by this beautiful example, took something which is quite clearly

and correctly called “reasoning”; and showed if you “disentangle” it for social influences, there is nothing left. It is like saying here is a woollen garment, now “disentangle” it from the knitting. If you try you finish up with nothing. You don’t have a purer form of a garment; there is nothing left. That’s the point of Wittgenstein’s example, to destroy this simple-minded distinction between reasoning and social process. Wittgenstein showed that you can’t draw a boundary or “disentangle” them because the two things are really one thing. You can’t get a stronger programme than that.

*Huang: Can I ask the final question? We decided to ask this question to everyone we will have interviewed. STS is booming in East Asia. What suggestions do you have to help us construct our own STS research?*

Bloor: I feel very unqualified to offer suggestion or advice. I think the most that I dare say is something rather general. I hope you don’t make the same mistakes that were often made, and are still being made, in Europe and America. I would say encourage the historian of science and encourage factual and causal investigation. That will provide you with resources for thinking about science though it won’t provide you with a programme or a science policy. It won’t say “put money into this field or that field of natural science”. But it will be useful when you then come to think about policy. It is useful to understand some history and sociology of science in preparation for sitting around a table and listening to people making policy recommendations. Training in history and sociology of science helps you detect self-interest and self-serving ideology more accurately. You will understand through case studies. Your mind will be able to immediately link to the case. So, I would say: encourage history, encourage causal explanatory sociology and above all keep the philosophers out of it [all laughing].





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