East Asian Science, Technology and Society: An International Journal (2010) 4:601–617
DOI 10.1007/s12280-010-9156-8

Dropping the Brand of Edinburgh School: An Interview with Barry Barnes

**Ruey-Chyi Hwang · Zheng-Feng Li ·
Chih-Tung Huang · Rong-Xuan Chu · Xiang Fan**

Received: 1 September 2010 / Accepted: 1 September 2010 / Published online: 3 December 2010

1 Introduction

Professor Barry Barnes is not only a world's leading sociologist, but also a major founder in developing the discipline of Science, Technology and Society (STS). His career started in the 1970s at the Science Studies Unit at the University of Edinburgh. Before moving to the University of Exeter in 1992, he had become the

This interview has been translated into Chinese and will soon be published in *Taiwanese Journal for Studies of Science, Technology and Medicine*, Number 11 (2010):339–374.

Professor Barry Barnes is a major contributor to the Strong Programme in the Sociology of Scientific Knowledge. He is well known for his ground-breaking study on collective action, self-referring knowledge, and systems of power. His enormous influence helps transform the discipline of Science, Technology and Society (STS). This interview started from his naturalistic commitment, insisting that science should be studied in the same way as any other human activities. We then move onto the basic relation between knowledge and society. According to him, problems to do with knowledge and those to do with power are in fact two sides of one coin. His work on agency, collective action, power or even finitism can all be understood as analyzing different forms of the same awareness, rather than as a change of subject. At the end of this interview, we asked Professor Barnes for his suggestions toward the budding East Asian STS research. He encouraged scholars in East Asia to combine the Western resources with their own ones, and advised them not to simply follow a 'Programme' as it constrains their vision.

R.-C. Hwang
Academia Sinica, No. 128, Sec. 2, Academia Rd., Nankang, Taipei, Taiwan

Z.-F. Li
Tsinghua University, Beijing, Center for Science, Technology and Society, Tsinghua University, Beijing 100084, China

C.-T. Huang (✉)
National Chiao Tung University, Room 638, Center for General Education, Assembly Building 1, 1001 Ta-Hsueh Road, Hsinchu, Taiwan
e-mail: moenhuang@hotmail.com

R.-X. Chu
University of Edinburgh, Room 1.14, Simon Laurie House, University of Edinburgh, Holyrood Road, Edinburgh EH8 8AQ, UK

X. Fan
Sun Yat-Sen University, Room 104, Buildings 638, Yuanxi Area, Sun Yat-Sen University, Guangzhou Guangdong, China

director of the unit. In the by-now legendary unit, Barnes and his colleagues introduced a distinctive way to explore the relationship between knowledge and society. They suggest that as science itself is clearly a 'human activity', it should be analysed in the same way as any other human activities. Under this thinking, researchers from the unit started to analyse the practice of science, rather than what it ought to be. Their pioneering research soon invited severe debates and their research group was labelled as the Edinburgh School or the Strong Programme. Within the Edinburgh School, Barnes is especially known for his ground-breaking study on collective action, self-referring knowledge, and systems of power. His enormous influence helps transform the discipline of STS and won him the J.D. Bernal Prize of the Society for Social Studies of Science (more details can be found in Mazzotti 2008; Henry 2008).

This interview provides an interesting point of departure. It started from Barnes's naturalistic commitment, insisting that science could not, and should not, be exempted from sociological analysis. For him, relativism is a corollary of naturalism, meaning that if one is properly naturalistic, relativism follows. Thus, he stresses that relativism is defendable. We then moved onto the basic relation between knowledge and society. According to him, problems to do with knowledge and those to do with power are in fact two sides of one coin. In the interview, he used money and power as examples to highlight the importance of understanding self-referring system. For instance, power exists in so far as it is believed to exist and the person who possesses it is treated as 'powerful'. In this vein, his work on agency, collective action, power or even finitism can all be understood as analyzing different forms of the same awareness, rather than as a change of subject. At the end of this interview, we requested Professor Barnes's suggestions toward the budding East Asian STS research. He encouraged scholars in East-Asia to combine the Western resources with their own ones, and advised them not to simply follow a 'Programme' as it constrains their vision. He reminded STSers/Strong Programmers not to worry about whether the Strong Programme continues or not. They should be more proud of what they have achieved and think of the names second.

2 Interview

10 January 2009

Chih-Tung Huang (Huang): You are trained as a scientist, but later become a sociologist. Could you begin by providing us with some autobiographical background on what motivated you to conduct research in SSK and whether there was any initial tension between your scientific training and the approaches of the Edinburgh School?

Professor Barry Barnes (Barnes): I was not so much involved in it [the Edinburgh School] as one of the people who brought it into existence. So, it was not going to be difficult for me to fit with something I was helping to create.

People imagine a clash of science and relativism but there is none. For me relativism is a kind of second order thing following on from naturalism. If you are trained as a scientist, you see a physical world which is available for empirical study.

You see yourself as part of that world, made in same way as other things in that world. You have a monistic view about that, a unitary view. This scientific attitude is one I was trained in, which I have always felt committed to or comfortable with, over my whole career. I call this attitude, derived from my natural scientific training, naturalism. It's quite like the approach, for instance, of the scientific naturalist movement at the time science was beginning to emerge as an occupation in Europe in the nineteenth century.

When I came into sociology, I thought of it as a social science, devoted to the study of human behaviour or social behaviour. But sociology had a very strange feature: although it was like a natural science in some ways, and took it as a model, exemplifying an empirical attitude or objective attitude to things in most areas, it did not study science itself in that way, even though science itself is very clearly 'human activity'. There was a big difference between how science was studied and how tribal societies were studied, or everyday activities, political movements, etc.

One obvious example: if you are a sociologist or an anthropologist, you are interested in what people believe to be the case or what people think the world is like. The first thing you think about the ideas and perspectives that people have is that they are part of the inherited culture of the society in which they were born. At the time I started my work, which is many years ago, about the late 1960s, there were some good sociological works on science, but there was no sense of the culture of science existing as a tradition deriving from the ancestors. The first person who made this point really stand out in western writings on science was Thomas Kuhn (Barnes 1982a, b; Kuhn 1970). He met a lot of hostility for making what now seems very obvious points.

One of the things you must remember, I was a teacher of natural sciences students. Over my life, I must have taught around two thousand natural sciences students. I never found natural sciences students at all puzzled by the idea that they get their knowledge from their teachers in relations involving hierarchy or authority. They go to listen to lectures; they read textbooks. And they are aware that these things are authoritative resources that they cannot do without. But 40 years ago, philosophers and many sociologists had a very strange attitude toward this. They thought it tantamount to a criticism of science to claim that its knowledge was acquired through 'inheritance' rather than purely through observation.

My thought was that something was very wrong here. If we want to be scientific about human behaviour, then it should be studied in a consistent way. So, my thought was that we need to be naturalistic about the natural sciences as they are empirical phenomena like those they themselves study. We should learn from social science about how humans in general behave and apply their conclusions to scientists, because scientists are humans. And again, 40 years ago, this was an appalling idea. In fact, a lot of people still have not changed their opinion on this. They thought it a criticism to describe science as human behaviour such as sociologists' study in other contexts. I wanted to change this, to make sociological study of humans completely naturalistic, like other sciences, but it proved surprisingly difficult to do this.

I hope it is clear now what I mean by naturalism. It is a monistic view, a strongly anti-dualist view. It allows no difference to be made between the culture of science and other cultures. And now, if you wish, you can use the term, relativism, to denote the view that naturalistically all cultures can be understood in the same way, that is, in a naturalistic way. That, of course, can be applied to science, or to mathematics, or social science, or philosophy, and so on. People had difficulties with this. I also had difficulties understanding what their problems were [all laughing]. They had problems understanding that relativism follows on easily once you are properly naturalistic, that relativism is a corollary of naturalism as it were.

Have you heard about the Poincare conjecture (Mackenzie 2006)? Modern mathematicians tend to solve this kind of famous problem to create popular attention and publicise their subject. So 4 years ago when [Grigori] Perelman solved Poincare conjecture, an enormous fuss was made of this. But Perelman was not actually working primarily on this problem at all. Perelman was looking at a different problem: he actually went past the Poincare problem. Then he turned back and showed he had solved the conjecture too, but it was kind of incidental, a second order consequence that looks like it's the true achievement from the outside, not least in this case because a vast hunk of prize money was on offer to whoever solved it.

I see relativism as a corollary of naturalism in this way. Relativism is to be defended because it is implied by the position I actually find important, naturalism, being fully consistent in taking a scientific or naturalistic orientation to human beings and their cultures. It still surprises me that people create such a fuss about all this.

Huang: Can you provide us with some historical background about Edinburgh School, or as some people call it Strong Programme. Are Edinburgh School and Strong programme the same?

Barnes: I think in practice, yes. Of course, you have to ask those people who use the words [laughing]. You know what [Willard Van Orman] Quine said: Bachelor is an unmarried man, sometimes, but not always [laughing]. So, how can I say it is always the same. But there is no significant difference between the terms, for most people anyway.

Huang: David [Bloor] said you tend NOT TO use the term. Is there any reason why?

Barnes: You mean me [Barnes] or both of us [Bloor and Barnes]?

Huang: David said he tends to use the term more often.

Barnes: That is probably right. But there is no issue there. I suppose I do not like to talk about 'programme' very much. But it was a very effective way to draw attention to the ideas. So probably it is a good thing that David did it. David introduced those words, Strong Programme. It was probably a very good move at the time.

Later, there are possibly disadvantages, because ideas have changed and so on and so forth. 'Edinburgh School' is a little looser and allows for people who vary around some shared main theme. 'Edinburgh school' is perhaps a little bit better [than Strong Programme] as a way of talking about historical developments...

Huang: What has been changed for all these years? Because you said, it 'was' a good idea...

Barnes: Oh! Because there are more and more people. Originally it was just David and myself talking to each other. When David called it Strong Programme, it was a good idea. Everyone may now think there were a lot of people, but at one point, at the very start, there were only a couple of people in a room. It is very difficult to convey how extraordinary it is the way that things have changed. If you go to large conferences, like 4S (Society for Social Studies of Science[1]), [there are] hundreds of people.

It is very funny for me, because when I went to [University of] Exeter in 1992, I moved away from Sociology of Science more into General Sociology. I did not go to the meetings very much. For a few years, I was a bit out of the loop. When I came back, I recognised something radical had happened. I had been kind of a deviant in the field, but when I came back everybody was talking about the flaws of the 'orthodox view' and it turned out that this view included me [laughing]. I [the orthodox view] was wrong in this way, wrong in that way and so on and so forth. No doubt innovation in one generation becomes the orthodox for the next generation. I just did not notice this happening, but it was what happened. People wanted to 'move on' and did.

Huang: So, you did not go to Exeter to 'preach' Strong Programme, as some Chinese scholars said?

Barnes: No, the very opposite. I went to do something else. Oh, that is not true either. Let me go back and just go through this from the start.

What you have got to understand is I am a trained scientist who becomes a social scientist. I become convinced there is some very good work in the social sciences [mainly in sociology and anthropology, not so much in economics as we keep learning] and that the good work should be taken up and used to understand the natural sciences. This I do as part of Edinburgh School. I think this work is okay. We learnt a lot about human behaviour from studying what scientists do. It is a very interesting kind of behaviour to study. It is always surprising me that people never studied it in our way before, because it is an extreme kind of behaviour in a lot of ways and extreme behaviour is good to study. Think about mathematicians and what kind of culture they have got; it is a very unusual sort of culture. If you are a student of cultures, a sociologist or an anthropologist, you ought to be interested in a culture like that, because it is so unusual. So I saw this work as an important contribution to the social sciences, helpful in understanding human behaviour generally. And it seemed a good idea to take it back again into the centre of the fields that had inspired it and use it in efforts to solve some of the general problems of the social sciences. So instead of using knowledge of ordinary cultures to make sense of scientific ones, as I had done so far, I now wanted to use knowledge of scientific ones in understanding 'ordinary' ones. The thought was: we have been successful in studying scientific cultures so now the studies can be used as exemplars and sources of inspiration in studying other cultures. Thus, for example, all kinds of special status groups in society generally might now be understood by analogy with what had been learned of status groups of natural scientists.

I think this is how you should do the social sciences. You should have your special field; you should be most respectful of detailed studies therein that document or establish what has been done in particular cases. But you should also be willing to move out from there, and recognise a duty to advance the field as a whole. So what I

[1] See their website: http://www.4sonline.org/

have tried to do is to work back and forth between specific problems in sociology of the sciences and general problems of sociology and the other social sciences, i.e. the general problems of what some people call sociological theory. I have tried to do this all through my career.

A sociologist who did this very well indeed was Robert Merton. He was a major contributor to functionalist sociological theory, as well as having a lifelong interest in the natural sciences. What you can see in Merton's work is productive, beneficial interaction between the two roles, sociologist of science and sociological theorist. That is a very good model of how sociology is well done. The people who remain theorists and nothing more tend to become tedious, producers of words and nothing else, people who talk about their own talk and little more. But if you stay entirely in a narrowly demarcated empirical field, better though this is, you tend not to have the wide range of resources that you need to develop your work properly. So the interaction I've described is something I strongly believe in.

In the 1980s before I went to Exeter, I had already become deeply involved in several of the most general problems of sociology. One of these was the problem of understanding the nature of power. It had become increasingly apparent to me through the work I was doing in the 1970s that problems to do with knowledge and problems to do with activities are the same problems.

Huang: Are they the same problem?

Barnes: Yes, the same. It is only how we are looking at things that is different. To put it another way, problems that people call problems of power in society and problems that people call epistemological problems, or problems of knowledge in society, are the same problems.

Before this I had done some work on what philosophers call the problem of reference and published a paper in 1982 on finitism (Barnes 1982a, b). I had developed and elaborated a finitist view of how we use language to make reference to things in the world. Of course, this is not original: a closely related finitist view can be found in Wittgenstein. David [Bloor] (1997; 1983) did a lot of work drawing on Wittgenstein to help develop finitism. Ethnomethodologists and others in the US had done lovely work taking much the same position. But [as usual] I tried to generalise this work on reference as much as I could [finitists don't always like to do this] and this brought up the problem of how we refer to acts of referring. If we are to have a general understanding of how we refer then at some point we are forced to deal with the problem of self-reference.

After the 1982 paper, I was aware that there was an important sociological problem in need of investigation posed by the existence of systems of self-referring actions. I had very little idea of how to solve this problem, but I was convinced that it was an important one. Eventually, I wrote a paper called *Social life as bootstrapped induction* (Barnes 1983), which was a very bad paper and a very difficult paper. I felt very dissatisfied with its difficulty, but I could not make it better, because my understanding was still developing. I have still not finished working to clarify its ideas. I knew the ideas were important but I was not able to put them in a beautiful way. For some amazing reason [laughing], it was published nonetheless. Nobody was excited, or interested in it for about 10 years after it was published. Not even David. But in the

1990s people started to get interested in it. And meanwhile, I had written a book, *The Nature of Power* (Barnes 1988), which described societies as self-referring knowledge systems and set out what this involved a bit more simply and clearly. Self-referring systems of knowledge are made valid by virtue of being acted upon by the carriers of the knowledge, which entails the carriers believing the knowledge to be correct. The knowledge refers to states of affairs constituted by those who believe it is correct acting on the basis of it. Coming to believe such knowledge creates the referents that make it valid knowledge; for it refers to itself as believed and not to anything independent of itself. Through its shared belief in knowledge of this sort a collective may coordinate its individual actions, and render itself capable of joint and collective actions that can effect far more than disjointed individual actions ever could.

If you are describing what a collective is capable of doing, you have to describe what it knows, but you are also describing its 'powers'. Knowledge and powers are the same thing on different descriptions. Where do most of our powers come from? A community of people coordinates around shared knowledge, representation, language techniques, skills, artefacts, etc.—by virtue of that is able to do a million times more than they would be able to as isolated units, as the ridiculous creatures we call 'individuals'. So, nearly all of the power inhering in a collective [knowledge] comes from the organisational aspects and the shared culture, rather than from the musculature of isolated individual organisms. More or less all the power exists collectively. Or to look it the other way round, you can only understand what their knowledge is by looking at what is being done with it. You cannot understand knowledge without looking at its enactment, which is on another description the exercise of powers. The problems studied as problems of systems of knowledge and those studied as problems in systems of powers are the same problems.

Huang: I understand collective actions and powers. But how can we put finitism into that system you have just mentioned?

Barnes: If you are a finitist, then there are some intriguing second order implications about power. For example, suppose you are somebody at the top of a hierarchy, a bureaucratic hierarchy. Now, you know the enlightenment fantasy. Let me quickly caricature it. In the period of enlightenment in the west the power of reason is said to have grown to oppose the power of religious revelation. Secular elites grew in Europe believing in the power of reason. Immanuel Kant said wonderful things about reason, etc. Where does this arise? It arises in the European despotisms. It arises from France and Prussia above all. There is no enlightenment in England, unless you shuffle your definitions. The 'liberation' there comes more from empiricism with rationalism secondary. In Europe, empiricism is the second player in the liberation from religion and coercive structures and rationality or reason is the first player. This enlightenment liberation from religion is happening in despotic societies, like Prussia under Frederick the Great. The civil service of Prussia is founded by switching some of the officer class of the army to 'civil' roles. The military hierarchy spawns the administrative hierarchy. The idea is that the commander-in-chief, say Frederick, issues an order. This is the fantasy. And the order goes down the system to the person who has to do what the order says. And this person at the bottom pulls the lever Frederick asked to be pulled. There is an idea that the meaning of an order can be transmitted unchanged because it is inherent in the very instruction itself. The instruction itself conveys the intention of the commander onto the recipient and as it is necessary for the reliably exercise of the power possessed by the commander.

You can see the difference finitism made to this. Finitism suggests that any order has got to be understood by analogy with the particular situation the recipient is in. And the way you extend the analogy is not determined by the order itself. Basically, there is no 'fixed meaning' inherent in the noise of a barked order, or in one written on a piece of paper sent down to the one who carries it out.

Even so, something happens at the other end. What happens? There is a problem of lack of coupling and control here that on a finitist view is ineradicable. You can never make your order sufficiently precise to completely specify what is required at the other end. You can write a whole book as your order, elaborating instructions in vast detail and it will not help to solve the problem at the fundamental level. You just get more and more problems with the language. A good person on this, by the way, is Harold Garfinkel. His book, *Studies in ethnomethodology* (Garfinkel 1984), is the sociological finitist bible. Just like Wittgenstein is the philosophical finitist bible. So there is a problem of control in any system of delegation of powers. You cannot assume that Frederick 'has' all the power and the subordinates are mere expressions of his power. All the way, with every link in the chain of hierarchy there is a problem.

Incidentally, at the time I was writing this work and thinking about the issue, economists were beginning to think about it as well. They came out with much the same solution, but something must have gone wrong since [laughing]. There is a theory in economics called principal-agent theory (Sappington 1991). Principal-agent theory has a view of problems on delegation which is very close to the view of delegation that I have in the book on *Power* in 1988. I suspect the economists are not so purist as I am, that they do not see this as a fundamental thing, although they do recognise there is always a problem for the principal trying to control the agent, the person supposedly acting on the principal's behalf.

Even more incidentally, notice how the word 'agent' *means* the person acting on behalf of the principal. Just as an agent does when they trade on your behalf or make an agreement on your behalf. Compare with how the words 'agent' or 'agency' have been used in recent sociology in the UK, in the very opposite way to this traditional notion of an agent as someone working on behalf of others. You will get some very good insights into why modern sociology finds it so hard to understand agency properly.

Anyway, I hope you can now see how studying power is assisted by awareness of the finitist view: how an order can be successfully transmitted down a hierarchy as an instruction is problematised by finitism. The vision of hierarchies working like great machines with instructions put in at the top being automatically and reliably converted into the 'implied' actions lower down is exposed as incorrect. Everything finitist work says about extending scientific knowledge can also be said about giving instructions in the context of hierarchies of power. The two underlying problems are the same problem. So you can see how finitism is relevant.

Let me return to power and self referring knowledge systems now. If you look at referring activity, it seems to me that there are things on the end of those actions, referents, the things referred to. So if I say to you, pass the pen to her [pointing at a pen], you will see something on the end of my talk as it were, something you can extend your hand to, the referent of the talk, or the speech act as some say. I do not see this as a difficult notion–the notion that things are really there and we refer to those things as we speak. Perhaps it's because I trained in chemistry, the most realistic of the sciences, dealing with tangible materials most of the time. Anyway, I am a natural realist as most of us are. When I speak of 'this pen' and wave the thing around, I assume [the pen] is what I'm speaking about, the real world referent of my speech. But what is the referent when I speak of Frederick the Great's power? What constitutes the power; what does it exist as in the real world? The shared belief is what he possessed is what his power actually consisted in. And more generally, where all believe you have power you have it and where none does you do not. So you can see how there was a self-referring system of knowledge surrounding Frederick that constituted his power. Because he was believed to be powerful, people obeyed him. And because he was obeyed, people believed he was powerful. [I oversimplify of course.]

Similarly, money is money only in so far as it is believed to be such and permitted to function as such because of the belief that that is what it is. And here you can see why an understanding of the self-reference loops is of great practical importance. If loops of this sort are broken and we no longer accept that 'economic' entities like currencies and property rights and prices and so forth are what we believed they were then they cease to be what we believed they were, since it was only by virtue of being believed so to be that they were what they were. The person who took these ideas forward was Donald [Mackenzie] (1981) who has done wonderful work on option markets and the pricing of options (MacKenzie, Muniesa et al. 2007). It has been a great pleasure to me to see this important work being done.

Huang: We have interviewed David [Bloor] on finitism. And today you provide us with lots of historical perspectives on finitism. The way you interpret finitism is a bit different from David's, although I cannot pinpoint the difference between your idea and his. Maybe it is my misunderstanding; however since you have a long relationship with David, do you think that you and David are talking about the same thing when you are talking about finitism?

Barnes: You know there are problems with what you've said: What is 'the same'? [laughing]. I don't think David and I have any fundamental disagreements at all on any of this. But a problem with finitism is that one cannot take account of it all the time in speaking. It is part of the business of communication to speak as a realist, and most of the time this means simplifying or even ignoring the finitist view. And perhaps David and I simplify in different ways sometimes.

Strict finitism can encourage you to question whether and why 2 and 2 is 4, or even whether and why 1 equals 1, as Wittgenstein did in his *Remarks on the Foundations of Mathematics* (Wittgenstein 1956). Reflecting seriously on this you could find yourself problematising everything so much that you never said anything. You might find yourself thinking of endless things that words might mean or sentences might be claiming, and endless ways of being misunderstood if you did say a given thing. But in practice of course we take it for granted that everyone habitually or customarily accepts that 2 and 2 is 4 : i.e. we rely on the habits and customs of audiences when we speak. David and I tend to talk to different audiences with different habits and customs and perhaps this accounts for differences in how we present finitism. David talks to philosophers more than I do, so perhaps I present finitism more casually than he does: I don't know.

A nice book on this, that David has made good use of, is *Proofs and refutations* (Lakatos 1976). It is about mathematical proofs and the problem of relating new cases to an existing proof - for example of deciding whether to count a new instance as a disproof of the proof, or as compatible with the proof if the proof is understood a bit differently from the existing customary way. It is a very attractive book and illustrates the finitist themes we are discussing very well.

Huang: Can we move on to your recent work? I know your recent work is about Geno-technology or genes (Barnes 2002; Barnes and Dupré 2008). How do you use your theory mentioned before onto the specific gene case?

Barnes: One of the fascinating things with the rise of modern genomics is that people are looking back into the history of genetics with a completely new vision of the sort of things that are really going on. And the whole notion of gene is being problematised. If perfectly reasonable to suggest these days, even as a practical and not merely a philosophical idea, that there are not really any such entities as genes at all. Certainly, if you make a big deal of existing textbook definitions, nothing fits them unproblematically according to present knowledge. And while use of 'gene' continues to have some pragmatic utility, it is so situation specific that it's perhaps best to think of the term as a useful fiction that doesn't refer to anything 'really there'. For sometimes, in some contexts, references to 'genes' have ceased to be useful and actually become seriously misleading.

There have been some fascinating revisions of the history of genetics. Robert Kohler's book (1994) called *Lords of the Fly: Drosophila Genetics and the Experimental Life* is one. Where once work on Drosophila was taken as clear confirmation of the truth of Mendelism, we can now see [helped by Kohler] how it was not altogether easy to find flies that would breed in conformity with Mendel and how when they were found they were cherished and preserved even as other flies were disposed of. Rather than the flies confirming theory we can see theory shaping the population of flies. The intention was to facilitate research of course. But even so we can suggest in hindsight that Mendelism became accepted as true in part by creating phenomena that conformed to it and destroying phenomena that didn't.

The detailed story is fascinating. There was one particular guy in the group who looked after the flies, a guy called [Calvin] Bridges. He was the guy who knew which the good flies were, what they were good for and what not, how to keep them, and he kept the records of how they bred. All this underpinned Mendelian-Drosophila genetics. He was the linchpin of it and people tend to forget him, but in this book you see his enormous importance. He was a bit of deviant, a rule breaker, and was always running into trouble. The university and the authorities were occasionally inclined to fire him for outrageous behaviour. I don't know if he would have done very well in China [laughing]. Well, perhaps he would have done ok in China. But the head of the lab, [Thomas Hunt] Morgan, was always having to watch out he didn't get fired. 'I've got to have this guy. I don't care what he did', he'd say, or something like that.

So, just by accident I got into this area recently, and as well as been quite fascinating it's also recapping my own earlier interests. After I started in the Science Studies Unit here, I married someone from a Genetics Research Unit in Western General Hospital who was doing work on human sex chromosomes and I got very interested in genetics in the late 1960s and the early 1970s. Of course, there was also the Jensen Affair [on IQ] (Jensen 1991) ongoing at that time with lots of debates between sociologists and geneticists. In those debates, people would have done much better if they have been more careful in reading each other's ideas. A lot of it just turned into show business.

Anyway, I was in some detail interested in genetics and in its relationship with social sciences, and it has been really extraordinary to come back to something I spent a lot of time when I was younger, after a gap, see the changes, and look at recent work in genomics. I am not a biologist. I was a trained chemist as you pointed out. But this is a very good background for understanding genomics and molecular genetics, not as good as biological science but not far from being as good, because although there is an area of ignorance, there is for traditional biologists as well.

At this time I was close to the end of my ‘official’ career and was thinking about retiring, I had written a book on agency (Barnes 2000). It is quite an okay book including work on basic sociological theory and I was wondering what to do next, and then we applied for the genomics grant, which came through in 2002. So, it was a natural move back from general theory to science, part of the pattern of movement I've liked in the past. I only work half-time now, of course but it turned out to be a great move; for these biological sciences that I knew something long ago have changed enormously, in fascinating ways. And whilst it used to be very difficult to get sociologists interested in natural sciences, things are very different now. Except that they tend to call themselves different names now; they call themselves STS people or whatever, perhaps because some sociologists remain with this peculiar attitude that science shouldn't figure among their interests. The main sociology department here in Edinburgh seems not very interested in the natural sciences, I don't know why, when ‘sociological’ interest in science in Edinburgh is obviously very strong.

Huang: Some people in the Science Studies Unit are worried about, not really ‘worried’ maybe, but they are thinking about where to go in the decades to come; how to achieve our goals? Do you have any suggestions or opinion on this?

Barnes: I see good work a lot of people are actually doing. Were you here in the year when I came to examine a PhD here? Donna's [Messner] PhD (2008)? She was doing a PhD on regulatory regimes relating to drug development in the United States.

Huang: Donna? Sure, I know her.

Barnes: This was making use, very good use I thought, of the finitist view. She was showing how the formal rules and guidelines in regulatory agencies in any year tended to describe what had been done in the previous year better than what was being done at the time. Because what was happening was people were tending to change what they were doing first, then to rationalise what they were doing, as a kind of expedient reinterpretation of existing formal rules, and only then change those existing rules into new rules that more naturally and obviously conformed to the changed practice. So, the practice is going along, and rules are running ‘after’ it, if you see what I mean; and their interpretation runs afterwards. The PhD showed this in case after case in proper detail. It was not just waving the arms; it was citing studies, citing interview materials, citing legal judgements. It gave a very clear picture of what happened in regulatory agencies.

This is something that seems to be increasingly of interest in the west. Certainly the European Union is obsessively interested in regulation, and lots of people in the United States are interested in the best way to regulate complex, high-tech processes. People working with Sheila Jasanoff[2] have done a lot of good work of this sort. There is a whole group of people working on the World Trade Organization. There are people in European institutions working on these issues. So, there is obviously a lot of ‘mileage’ in this topic, if this word is OK: maybe they say ‘kilometre-age’ in China. Anyway this is an important area where there is a lot of funding available and also where a lot of people in our societies are mistaken in their existing views. They are thinking that rules do have clear implications, that there is something inherent in rules themselves, and can offer another perspective by work in this area.

Study of law is another good area of course. Lawyers are very interesting. They are both aware, and unaware, of what finitism says of rules. Because they are lawyers; they are always adapting and twisting laws to suit the cases they want to argue, so nobody better than lawyers knows the virtues of finitism and how, pro forma, you can do what you like with laws. But they don't like to talk about it. They like to present themselves as making compelling arguments, and their task as upholding ‘the rule of law’ (and not the rule of men, as it's sometimes said). So, lawyers are there already in practice, but nonetheless in the area of law there are masses and masses of opportunities for applying the finitist view. Mike Lynch and colleagues have a book on DNA finger printing that could be relevant here (Lynch, Cole et al. 2008).

Similarly, there are endless opportunities for finitist studies of bureaucratic administration, including administrative aspects of research and scientific laboratories. There may be some pointers in the chapter on bureaucracy in my power book (Barnes 1988), [but] that's 20 years old so you can't expect too much [laughing]. But, there are certainly some ideas in that that have not been taken up, and also in the discussion of Habermas in that book [*power*].

In the area of genomics, there are lots of examples of interesting regulatory problems. The problems of the regulation of stem cells, for example, are quite fascinating taking a finitist point of view. Again, the scientists know perfectly well that you can't built really good stable regulations on ideas of different sorts of stem cells, because stem cells are transformable and variable objects. So, there is a clash between the legal profession which wants tidy categories, and stem cells which don't

[2] See her website: http://www.hks.harvard.edu/about/faculty-staff-directory/sheila-jasanoff

give a dime about the legal profession. Similar issues surround debates about embryos of course, which are highly 'politicised' and where inputs from religious bodies complicate the debates.

Ulrick Beck (1992) had a point when he said that politics in our century is now done as science and what we used to call politics is becoming a kind of comedy presented on the media to keep people entertained. Professional politicians, he's implying, can't do much to identify and define the big issues. The big issues have all been worked out in laboratories and regulatory agencies. It was a bit of a rhetorical flourish at the time but it is worth thinking about. Certainly, [When] you look at science, you are looking at politics, [although that] doesn't mean you are not looking at genuine science.

Huang: Before I ask my final question, do you have anything to ask?
Xiang: Not really.
Huang: We decide to ask exactly the same Final Question to everyone we will have interviewed. Because STS and SSK are booming in Taiwan and China, do you have any suggestions to these new comers?

Barnes: Well, I don't know your traditions well enough, but they will not be the same as ours. One thing is, sociology in this country is a rationalist tradition. Even now, it has a tendency to Platonise and reify too much. In your tradition, it may be easier to draw on things like dialectics, the notion that nothing is fixed, that everything is in the process of transformation. But in any case you should not worry about combining the resources you might find here in the west with your local traditions. Beyond that, it is difficult to say, because you are the person who should know, not me [laughing]. You will know what your own resources are and what in ours will fit best with them. I came into sociology from a natural science and felt that is the only resource that I could make use of beyond social science itself. Your resources are a rich impressive tradition and I can't believe that there are not immensely interesting perspectives in your culture. In a sense, we in the west are impoverished compared to you, because you are getting thoroughly familiar with us and we are not making sufficient effort to understand Chinese culture, which we should. The first body of work I ever read when I was reading Sociology of Science included Joseph Needham's studies, which was deeply appreciative of the history of science in China and linked it and its development to Chinese political structures and modes of production. But for some reason I never read further into history of this sort. Of course, Needham was part of an important group of Marxist thinkers in the 1950s, a great source of ideas and materials that eventually was sidelined collectively, as should not have happened, to the detriment of sociological history and historical sociology of science here. Of course, we have recovered from this over time and historians now bring an admirably broad and naturalistic approach to their work on science.

I was in a conference in Berlin last year on the history of molecular biology. Many well-known scientists in the field were there and they were saying that the field is dead now; that nobody talks about molecular biology anymore. It has just gone. But they were perfectly cheerful about it. They were even saying: you know, when you think about it, it's never exactly existed anyway [laughing]. And: we did not call ourselves molecular biologists except when we wrote grant applications. Listening to this I thought to myself: if a group of sociologists got together and talked about the history, they wouldn't talk like this. They would (they do) talk about the crisis in sociology, and what we can do to stop it disappearing. The reason is simple. The molecular biologists have a tremendous amount of recognition for what they have done. They know how much they have affected other sciences around the place, how many of their achievements have been taken up into all kinds of fields of different names. They can just look anywhere they like and see the importance of what they did implicit in the practice of present day science. So, they don't care what it is called, or how specifically it's remembered. Sociologists lack this sense of status security, but in fact they too have done very important work, not just in science but elsewhere, that is now carried forward and made use of in many different fields and contexts, referred to with all kinds of different names. They should try to be both more proud of what they have done and more relaxed about its recognition.

With the Strong Programme, for example, it doesn't matter whether it continues or it doesn't, as a name or a brand or whatever. Its exemplary achievements have been taken up and are now used in other fields, in history especially perhaps, and its naturalistic approach embodied in specific instances has had beneficial effects elsewhere. So finally, I would say in reply to your question. Don't ask what you can do in Sociology of Science, or in STS, or whether SSK applies in China or East Asia. Look directly to particular studies and ask which might serve you as models, as exemplary achievements that you can build upon and modify to facilitate your own work. If a model impresses you in its particular application then assume it will apply in your context of work. The model itself can't speak and let you know whether it applies or not, whether it is specific or universal. Get on with applying it and find out that way. You will automatically find yourselves modifying it as you use it in order to make it apply, and if you have chosen well then it will in some way apply. And in applying it you will be both in accord with precedent and original, conservative and creative, at the same time. To put it another way, don't look for theories to follow or perspectives to constrain your vision, but for resources to make use of, exemplary achievements that can be extended to other cases by analogy to create interesting work. Say to yourself: this is good work, so good it must be applicable somewhere else, somehow. That's how to think. Look for resources to use in your work, find areas that interest you; think of the names second. It doesn't matter at all whether you become the Beijing School or something else.

One source of exemplars that is very important at present is Donald's work on economic activities which treats them as self-referential systems. Here there are connections waiting to be made, I think, between quite a few areas of economics now and work in the 'Strong Programme'. You [Xiang] must have a background that makes this especially interesting.

Xiang: Yes, I am from the rubbish economic background [laughing].

Barnes: More generally, I hope all of you find things of value to you that you can use. But you will have to evaluate them in detail and have the courage to decide for yourselves what is to be made of them; there are no labels stuck on them to tell you.

Huang: Thank you very much!

Barnes: Sorry, I haven't asked your background.

Huang: [laughing] I am the rule-bender. It's law. [All laughing]

Barnes: Very finitist.

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