

STS as a Program of Ontological Disobedience: Koichi Mikami Talks with Steve Woolgar

KOICHI MIKAMI¹

UNIVERSITY OF TOKYO

STEVE WOOLGAR²

UNIVERSITY OF OXFORD

Abstract

Is science and technology studies (STS) a luxury that our society cannot afford anymore? In this interview, Koichi Mikami tries to learn lessons from Steve Woolgar's distinguished career on how the kind of sensibilities treasured within the field of STS and the type of critical engagement that its researchers aspire to might be best exercised in a changing landscape of higher education and academic research. Woolgar explains how he, at some key moments in his career, managed to create "a room" for reflexive thought and critical engagement in domains that could otherwise have been dominated by simple deterministic discourses. He explains that the questions of *how* and *to whom* you sell your criticism deserve as much attention as *what* your criticism is. A reflection by Koichi Mikami follows the interview.

Keywords

reflexivity; skepticism; business

Early Excursion into Sociology of Science

KM Can I start by asking you how you became involved in the field of STS?

SW Sure. I was doing an undergraduate degree in engineering at Cambridge and became extremely bored with it. It seemed to me that there were few engaging puzzles or challenges. One had to learn a lot of facts and was not encouraged to reflect on them. So I was looking for a way of changing away from that. I even considered switching to medicine at one point. Then I discovered this option within the engineering degree, which was called the management option. At that time, and in the Cambridge context, it had rather little to do with management really. That was long before business schools, and in

¹ Koichi Mikami, Email: Mikami@komex.c.u-tokyo.ac.jp

² Steve Woolgar, Email: Steve.Woolgar@sbs.ox.ac.uk

fact it was a combination of statistics and mathematics, industrial relations, sociology of work, sociology of organization and so on. So I took that course in my final year at Cambridge rather by default and discovered that I really enjoyed it. And one of the teachers on that course was Michael Mulkay. He was at Cambridge at that time. The year turned out really well, and they asked me if I wanted to stay on to do a Ph.D. I was meant to be the person who united the management side of the group with the sociology side of it, but after a few months I realized that I really wanted to do sociology, so I did a sociology of science Ph.D. under Mike Mulkay. It was on the discovery of pulsars—the emergence of pulsars as a research field and their discovery. Then after a year of the Ph.D., Mike Mulkay got fed up with Cambridge and got a job in the Sociology Department at York. He suggested that it would be best for me if I went with him and became part of what he called “a real university.” The sociology group at York was really active and well known at that time, so I did that. I moved to York but my Ph.D. continued to be registered with Cambridge. That really was my earliest excursion into the field of STS. And you know, at that time, we are talking about 1973, 74, or something like that, I recall there was a manuscript sent to Mike Mulkay to comment on by Harry Collins. That was when Harry was doing his work on the core set—the TEA set. The other things happening in the field then were, apart from Harry, who was pretty much a lone figure at Bath, the group at Edinburgh. I became aware of Barry Barnes first, then David Bloor and David Edge. In 1975 I got a job at Brunel as a lecturer in sociology. They quite liked the idea of there being a sociology of science, but it wasn’t a big subject by any means. They really wanted somebody to teach a whole range of things in sociology, but since I had an engineering background, they also wanted me to teach statistics and things like that. Around that time, one was reading Barnes, Bloor, and Collins. They were the main centers. Interestingly, everyone was aware of what was going on at Sussex, at SPRU,³ but there was rather little connection with it from my colleagues and supervisors. There was also some work going on at Manchester at that time. I think it was called the liberal studies of science or some such. We actually had some connection with them. I remember one of the guys from there decided to study radio-astronomy and upset Mike Mulkay because he turned up in Cambridge and started interviewing all the radio-astronomers. Which was exactly what Mike Mulkay and I were working on then. That was in 1976 and I finally finished my thesis in 1978, about two, three years into the lectureship at Brunel.

KM You mentioned several names but were they the people who wrote the materials you read when you were starting your career? Did they recognize themselves as STS scholars as such?

SW I don’t think the label STS was in use at that time. David Edge started the journal with Roy MacLeod, who was a historian of science, in 1971 and it was called *Science Studies*. It was called *Science Studies* to start with, and then a few years later they changed it to *Social Studies of Science*, which still is its title. In terms of titles, Harry Collins was pushing for the

³ Science Policy Research Unit.

- “sociology of scientific knowledge” because he was pressing the idea that one had to understand the content of the knowledge, rather than just the social relations between scientists. Indeed, that was pretty much part of the Strong Program too, so it was very resonant. Harry came up with this sociology of scientific knowledge, SSK, which was the acronym used around that time. My recollection is that in those early stages there was a little bit of work in Bath, a little bit of work in York, where I was, there was something going on at SPRU—but which paid very little attention epistemologically to the content of science—and, of course, the Science Studies Unit at Edinburgh.
- KM** Did Mike Mulkay have other students?
- SW** Oh yes! Mike Mulkay has a long and distinguished pedigree of students. His first Ph.D. student in sociology of science was Nigel Gilbert. Nigel was a year ahead of me at Cambridge and moved to York at the same time as me. He got a lectureship at York and subsequently went to a lectureship at Surrey. The next person through was me, and the next was Andrew Webster, who of course now is very prominent and back at York. He was a contemporary of mine at York. Another contemporary of mine was Jonathan Potter, who has become a star in social psychology by persistently and inventively beating psychologists over the head. Another student was Malcolm Ashmore. Malcolm and I, and probably Mike, started what we called the Discourse And Reflexivity Group (DARG) at York. We had a number of meetings over a couple of years, which were terrific, really good fun. One outcome of it was the edited collection *Knowledge and Reflexivity* (Woolgar 1988). Jonathan went off more into discourse analysis and used the same initials “DARG” to describe his discourse analysis research group at Loughborough. They hired Malcolm Ashmore and he spent nearly all his career at Loughborough with them.
- KM** So these were the people you had close contact with?
- SW** Yes, they were my contemporaries there. One of the big influences for me at York was Paul Drew, who was a very high church conversational analyst. I shared a house with Paul for a year and it was pretty amazing. That story is in the piece I wrote in a collection edited by Alan Sica and Steve Turner called *Disobedient Generation* (Sica and Turner 2005). It is a collection of articles reflecting on the 1960s. It is about what happened to radical sociologists of the 1960s and what they are doing now. It is a fun volume because it includes a lot of pictures of people with huge lapel jackets, moustaches and long hair, looking very groovy.
- KM** Were you also like that?
- SW** Yes, we all were. The book comes from that time. It is a very interesting book and the whole business of looking back and reconstructing the historical questions that arise with that are very vivid in it. It is a good collection. I left York in 1975 to go to Brunel, and taught bits and pieces in sociology of science but there wasn’t really a group there, there was nothing like that.

Life with *Laboratory Life*

- KM** *Laboratory Life* (Latour and Woolgar 1979) is now one of the classics in our field, and was first published in 1979. Did your move to Brunel also coincide with the starting of this project?
- SW** Yes, it did. I got an invite to go to a conference at Berkeley, on “The Use of Quantitative Measures in the History of Science.” The reason I got this invite was because my first-ever published piece was a critical attack on the use of quantitative measures in studying science. The main argument being that people don’t address the conceptual issues behind what is being measured and simply take the numbers at face value. It was a very simple argument. Based on that, I got an invitation to go to Berkeley. It was a meeting of lots of historians and sociologists of science, especially Americans. I remember I was about to give my talk and some guy came up to me and said, “Hello, I am the chair of your session, Robert Merton.” Wow! Bruno was also at that meeting. He and I were really out of place in this meeting with all these quantitative historians. So we spent a long time chatting together. That was in 1976, and he invited me down to San Diego, to the Salk Institute, where he had recently landed a fellowship. He had been invited by Roger Guillemin, and his remit was something like studying the career paths of immigrant scientists. When I met up with him, he showed me around the lab where he was based. He had been trained in French anthropology before that, and it was just fantastic because he had this wonderful instinctive distance on everything happening around him. I remember he picked up a pipette—one of the measuring devices—and said “with this, they imagine they can measure the quantity of liquid” and then he put it down again very carefully. I thought: “This is magic, this is absolutely what we need, this kind of skeptical analytical distance on the practice of what is going on in the lab.” Nobody had done that. That was in the time of Kuhn and great sweeping historically detailed studies, of Bloor and Barnes types of approaches, or of interview studies, which Harry Collins was doing. Nobody had actually gone to sit in the laboratory. I thought “this is special, this is fantastic!” So we agreed to work on it together and finally published the book with Sage in 1979.
- KM** Were you also sitting in the lab?
- SW** No, it was Bruno’s fieldwork. At that time, Bruno’s English was nothing like as good as it is now. So the book was very much his fieldwork but my writing. That was the basis of our collaboration. Also, he didn’t know much of the literature in the sociology of science at the time. I think he knew Merton and a few things like that, but he was really unaware of the sociology of science, so that was what I brought to the writing of the book. That was a delightful collaboration. We really enjoyed working together. We had no idea that it was going to be such a big hit. For a couple of years after it came out, we went to conferences and looked at each other saying “there’s a lot of stuff in the book, there’s a lot of good stuff in our book.” But the initial reviews were not very generous.
- KM** Really?
- SW** I remember some saying “this is a really bumpy ride over a rough terrain,” “it’s difficult to follow” and that kind of thing. A colleague congratulated me on having “kept the French argument under control.” Another review from a philosopher of science said “unwittingly,

Latour and Woolgar prove everything that philosophy of science has said about laboratories.” It was really strange. But I think the big appeal was that nobody had done that before—an on-the-ground, nuts and bolts, what happens in practice everyday, observing the life of scientists—it was just not a topic. A few other people were starting to work on the same general idea. Karin Knorr Cetina was doing some work in a lab in Berkeley, but perhaps not quite as ethnographic, as we say now, as ours. Well, nobody used the word “ethnographic” back then, it was rather called “anthropological.” There was Sharon Traweek, an anthropologist, doing her study of Stanford particle physicists. Mike Lynch was also doing his laboratory study under Garfinkel. All that was bubbling through. That move and the emergence of Bruno as a strange, amazing, inventive, unusual, French, non-English scholar were quite interesting. There were some quite heated debates too. The big debate didn’t really appear in a one-concentrated form but if you look at Pickering’s 1992 volume *Science as Practice and Culture* (Pickering 1992), there you start to see some of the initial cracks in the alliance. Up to that point we were all post-Kuhnians together, our common enemy was objectivist philosophy of science, but in that book you start to see divisions: you see David Bloor arguing with Mike Lynch about the use of Wittgenstein, and you see Bruno arguing with Harry Collins about what it means to have social construction and its limits, and so on. In the meantime, the reputation of the Science Studies Unit at Edinburgh was on the up and up because of this British-American tension. Among sociologists, Robert Merton ruled the roost in America, and the British were saying, “No, this is not about the social functions of science or social institutions and relationships between people that happen to be scientists. This is about the *content* of science, this is about epistemology, this is about what affects the character and content of scientific knowledge.” This approach was quite distinct from Merton’s. The Science Studies Unit’s reputation was really enhanced, firstly, by the journal that David Edge and Roy MacLeod started. Roy MacLeod was at Sussex, but David Edge was pushing it from Edinburgh and he was the powerhouse in terms of the work he put into that journal, a fantastic editor. Then, there was this dispute between David Bloor and Larry Laudan, and for a period of a year or two, there were Laudan-Bloor debates, replayed in various different venues, conferences and workshops. I went to a few of these, and there were really edgy, stand-up disagreements. I recall one meeting in Oxford, when Steven Lukes had Clifford Geertz visiting, and they had invited David Bloor as well. The room was filled with objectivist philosophers and they were saying “this is a complete nonsense, this stuff.” At one point Steve Lukes said to David Bloor: “So, if I paid you enough, David, you will change your argument, right? That’s what you sociologists are saying? It’s just a question of resources, right?” They were very keen to oversimplify the whole argument in order to dismiss it. I thought David Bloor was terrific in that situation, he was really clear, careful and refused to be rattled. There is another story about the reputation of the Science Studies Unit. I got invited around this time to give a talk in Chicago. Just before I went on to talk, my host asked me: “Just a bit of background to introduce you, you are with the Science Studies Unit at Edinburgh, aren’t you?” From that distance they thought all Brits were

from the Science Studies Unit. So I said: “No, no, I’ve been working on these issues which are quite different from Edinburgh, and I was based at York and then moved to Brunel.” But when he came to introduce me to the audience, he said, “This is Steve Woolgar. He has recently *left* the Science Studies Unit.” So, “the Science Studies Unit” stood as a signifier for all British, non-American sociology of science. Which I think speaks volumes for the reputation of the place.

KM But at the same time, there were scholars like Mike Lynch and Sharon Traweek in the States starting their ethnographic work. So you had some colleagues over there, right?

SW Oh yes, that’s right. But I am not sure if there was ever a kind of meeting or a theme in a conference focused on laboratory studies. You have to remember that the first meeting of 4S was in 1976. I had met Bruno that same summer and then went to the first ever meeting of 4S at Cornell. That was also really formative. I presented a paper on “scientists’ accounts,” at the time I was very influenced by ethnomethodology and was interested in how scientists describe themselves and in the vocabulary and concepts they use to depict who they are and where they come from. But the organizers put me in a panel on the “economics of science”! Well, the paper was about “accounts,” you see! I remember saying to one of the organizers “my paper is not economics,” but he had no idea about ethnomethodology. This was all new and rather troubling.

Reflexivity, Analytical Skepticism, and Networking

KM The next question is about changes in our field. One of the things that happened was “the turn to technology” and there was a bit of debate about what it means to turn to technology, which I believe you were part of. Can you elaborate on that?

SW Well, Bijker and Pinch saw the opportunity to use social constructivist ideas in relation to the production of technologies and just laid out that formula—SCOT, the social construction of technology. My complaint was about the adoption of a formula for analyzing science to analyzing technology. It is a recurrent theme in my work that too little attention is paid to the thing you are trying to study and how that might affect how you study it. People are far too ready to seize on a formula and then just apply it to another thing, another thing and another thing. For me what is exciting about STS is that it is continually changing what it is interested in and how one does things and is continually arguing with itself. These features of STS are what make it so exciting, what keep it so alive. So the idea that you can simply apply the same formula from science to technology seemed a good thing to try but also hopelessly unreflexive and unsophisticated. What I think is great about the best STS is that it is constantly looking for more challenges and more difficult and curious problems. So, in the days of SSK, the “hardest possible case” for sociologists was scientific knowledge. The argument was that if you could show that scientific knowledge was a social construction, then one could assume that other (lesser kinds of) knowledge are a social construction. Because scientific knowledge was apparently so hard, knowledge of things like abortion rights, rates of immigration, decline

of banking values or whatever, can be confidently considered social constructions. So going for the hardest case was a strategic move. But it seems to me what happened is that scientific knowledge is no longer the hardest case. The interest and energy has turned our attention to other more difficult areas and issues. That is in a way what gives the momentum to the field, that is what keeps it going and keeps it able to renew itself. For me as a graduate student, going to listen to David Bloor arguing with Larry Laudan was really exciting. It really felt like there was “danger” around there, and they might at any moment start yelling each other. Well, they did yell at each other. I know a lot of my more recent graduate students at Oxford went to 4S conferences and reported back that “nothing really happened.” They said they were rather envious of the early days when there was some acrimony and disputes between people in the field. A few STS scholars are now trying to re-inject danger and provocation by trying to identify topics and arguments which might cause upset. That’s all great. But the field as a whole has become very big, hasn’t it? It has a lot of energy, it’s very diverse for good reasons. Many have seen that STS offers sensibilities which are extremely challenging to what you take for granted. And the best of it, I think, has the sense that it’s never at rest with itself, it’s never going to be satisfied with giving news about over here for these people over there. On the other hand, it is a very elastic brand. Many different kinds of people now say that they are doing STS.

KM In terms of your own research, has it changed since you started your job at Brunel? What were the kinds of things you studied? Was there any influence from your colleagues?

SW There was a big injection of research support in the 1980s for understanding new technologies. I saw opportunities there. I had been doing ethnographic studies of IT companies, for example. The question was again what counts as the technical core, what counts as technical knowledge and how is that worked out in different social and organizational situations. Then the British government put a lot of cash into trying to address the effects of the so-called IT revolution. That was quite a significant move for me. I was in San Diego for a year when a colleague emailed and said: “There’s a job here to be director of this new program of Economic and Social Research Council (ESCR).” I applied for it and got it. Initially, I was quite concerned about the scope for pursuing analytic skepticism in such a venture. When I applied for the job, the program had already been defined—it was entitled “Virtual Society: The Social Science of New Electronic Technologies.” That’s what it was called and the central premise was that new technologies would extensively affect education, social life, banking, you know, the assumption was that IT would dramatically change everything. And I couldn’t see much skepticism in that. So after I got the job I said “I need to re-write this program spec.” But the ESRC said, “you can’t do that, this has all been approved by the committees. You can’t now redefine what the program is.” My solution was to use a question mark, to insert a question mark after “Virtual Society.” So the “Virtual Society Program,” became the “Virtual Society? Program,” and that got past the committees. I remember the program chair, Geoff Robinson, a very supportive former physicist who had been Chief Scientific Advisor to one of the conservative governments and was head of Hursley IBM research at that time. A

couple of years into the program he looked the title on some documentation and said “have we always had the question mark?” He hadn’t previously picked up that I had done that as way of signaling the need for STS skepticism. I think it’s really interesting that in such situations you have to play to standard accounts of what technology might do and the fears that go with that and, at the same time, you have to maintain skepticism about such claims. To analyze it, you need to be skeptical. So how do you do both things? You can’t just be skeptical, and you can’t just be deterministic. You’ve got to manage the two. That was my attempt to address that challenge, and it worked quite well. The Virtual Society? directorship was very interesting. I didn’t publish a lot during those three or four years but it was an incredible learning experience. I had to go around and convince captains of industry that they needed academic social science.

KM Was it difficult?

SW Well, it was very interesting. I found that such people often don’t conform to their stereotypes. So you imagine that they would be against social science because they come from technical background or because they are high-power managers. But actually quite a lot of them are interested in being associated with universities and social science research. So there emerges a way of managing, drawing them into a network and making sure you appoint them to the right committees, so that they help you and so on. I love all that sort of stuff because that seems to me like networking in practice long before Actor Network Theory. Against expectation, I often found that the skeptical approach would be well received in lots of companies. We had a big meeting with BT at one point and I said: “As social scientists, the most important thing is to maintain analytic skepticism about the claims made for the new technologies, while still developing relationships with the people involved.” And one of the BT directors piped up: “Well, that’s exactly what BT pay me to do! I am the paid skeptic, they have to run every new idea and project by me and they expect me to criticize it, cut it down, and show how the expectations are socially constructed.” And I remember commenting that my role was similar but more as an underpaid skeptic! For me the whole process of using STS-type sensibilities to build relationships with people outside of STS and outside of academia is very important and very interesting. Some people try to place me at the extreme end of a theory-practice continuum because I am interested in reflexivity. But reflexivity is a means to build relationships with people, and it turns out that a lot of the people that you deal with in industry and so on are themselves very reflexively attuned. So if you just try unthinkingly to give them what you imagine their stereotypes want, you can quite often go wrong. I’ll tell you this anecdote. When I was running the research center (CRICT) at Brunel, there was an occasion where we had a group of visitors from London. We wanted research money from them, so we knew we had to give a presentation. I told everyone in the Centre that it was presentation time and that we needed really snappy PowerPoint slides, suits and ties and so on. So we rehearsed and rehearsed, and I said, “no nonsense here, no reflexive stuff, no clever double thinking, we are going to give them straight facts, nothing else.” The day came and we gave the presentation but you could tell very quickly that it

wasn't working. After a little while, one of the visiting group spoke up and he said, "this is all very well, all this stuff, but haven't you guys heard of postmodernism?!" It's a wonderful example of how we, social scientists, can misjudge what other people want. What they actually wanted was some kind of stimulation of a French theoretical nature to take back to their workplace. For them this had value in enabling them to tell other people about the latest trend, about a new way of thinking. That would serve their purposes really well and we completely misjudged it. I think as researchers we often do this, because we have these uninterrogated preconceptions about what policy people are, what managers are like, what business people are like, who are "users" and so on.

Building Institutional Homes of STS

KM While you directed the center at Brunel, did the STS group at Brunel grow over time?

SW Let me see. It was certainly in the 1990s that it started growing. As I became more senior at Brunel, I became more influential about what kinds of people we would appoint. So I could sometimes push for a sociologist of science and technology for the new appointment. We would look for somebody who can teach a range of sociology but could also have specialism in this area. Then we got the research center, which brought in a lot of fellows and students also working in what became known as STS. We were very lucky to appoint Mike Lynch to Brunel and we had 6 or 7 marvelous years together. We also appointed people like Alan Irwin to a senior lecturer position in that period. Who else...we also appointed Andrew Barry, Ros Gill. Yes, that was a very interesting period, and it was possible to grow the group. An important development was the start of the research assessment exercises (RAEs). I realized that a key way to help improve Brunel's sociology rating was actually to be on the Sociology RAE Panel. The head of the panel John Urry decided what sociology needed on its panel was an expert in the sociology of science, which was I think quite a signal moment in the history of science studies because it was when the sociology establishment recognized that they need expertise in STS. So I became a member of the RAE panel, and I was on the RAE sociology panel on two occasions. That was very good both for Brunel and for STS to have that presence there.

KM And then you moved to Oxford. Was it in 2000?

SW Yes, 2000.

KM At Oxford, you were professor of marketing. I don't think there are many STS scholars who have this kind of title.

SW No, and it's a curiosity which I love. I was approached by Anthony Hopwood, who was the first Dean of the Oxford Said Business School there. He asked me if I would like a job in Oxford. His idea was that he wanted to re-build and re-fashion business education. Business education is usually organized in very strict silos; you have finance, marketing, operations management and so on. He wanted to jumble these up and offer an education both at undergraduate and MBA level organized around cross-cutting intellectual themes. He thought the way to do that was to appoint really unlikely non Business School people

to Oxford. So he approached me. I was interested because I was coming to the end of the Virtual Society? Program and I'd been the Head of Department at Brunel and directed the research center (CRICT) there. When he told me the job was Professor of Marketing I said, "I think you have the wrong guy!?" But he said, "no, no, you don't understand. Oxford responds to the wishes of its benefactors and the guy who is giving money for this post wants it to be called Marketing. You don't actually have to do marketing research and you don't have to teach marketing. You just have to bear the title and we'd like you to develop STS." He even said, "you can probably drop the title after a year or so, if you want," but I decided not to drop it because it opened up some amazing opportunities. I once got a phone call from somebody who asked if, as Professor of Marketing, I could help re-brand Bulgaria. It seemed such a crazy idea but I couldn't think of anything more interesting! The title has also been very helpful. My former student Elena Simakova did ethnographic studies of a marketing department—the entrée to that was helped by the professor of marketing title. The work I've been doing with Tanja Schneider on neuromarketing again takes a lot from my Marketing title. It's proved much more interesting and useful to retain the title than to ditch it. In a parallel life I have also become "a brand guru" in China. Marketing is a very under-nourished discipline intellectually, so insights from something as vibrant as STS have a big part to play there. So that was what happened. Anthony encouraged me to build up STS and I got grants to start building a group. Then Steve Rayner also came to Oxford to run the ESRC "Science in Society" Program. What was interesting at the business school was that in terms of organizing the different groups for teaching, research and so on, there was finance, marketing, organizational behavior and...STS! At Oxford, STS became a distinct organizational category within business education. A lot of colleagues in business schools and management departments up and down the country were rather envious of this because this put STS on the map within business schools. That was very good. It changed after Anthony's retirement, but for a time it was just terrific.

The STS Sensibilities and Their Relevance

KM Can you tell me a bit about the teaching side? Did you teach MBAs?

SW No, I didn't. I could have done and possibly should have tried it. It was really a question of priorities. I couldn't take that on top of the research and research-training I was doing. We organized a lots of substantial conferences and workshops, at least one a year. That was also great fun. We would sit down each November and say "what whacky conference theme shall we run next summer?" The first of those was "Does STS Mean Business?" That was really a response to the novel situation of STS being housed within a business school. "Does STS Mean Business?" of course has a double meaning—"is STS useful for business and management?" and "is STS getting serious now?" We had two of those meetings, and they were really successful and well-remembered. There was a lot of support for that topic—it is of course a proxy for the more general question of whether and how any

- academic endeavor can be “useful - though the resulting publication wasn’t taken up perhaps as much as I would like. After that, we had a string of summer meetings: “What is it with brain these days?” “From scale to scalography” and “A turn to ontology in STS,” which resulted in the special issue of *Social Studies of Science* which came out in 2013. Which again, I am delighted that it has attracted so much attention.
- KM** One thing that the Science Studies Unit at Edinburgh does is teaching STS to science students. What do you think about it?
- SW** I am in favor of that. It’s really interesting, and one should grab the opportunity to do that. I think there is an unresolved issue about whether social studies of science is good for science. I remember it being said a long time ago that the last thing that scientists need is social studies of science, because they are a distraction from scientists’ work. But it depends what you think is core STS. If you try to teach scientists to step back from their science and see it in “the wider picture,” subject to the forces of capitalism and the upshot of historical processes, their eyes are likely to glaze over. You can see they immediately become bored and uninterested. But if you say, “in the course of your experiment when you are making that measurement what assumptions did you build in to the use of that particular measurement device?”— they tend to be much more interested. So in general I am in favor of the idea, but the most productive way of doing it is teaching about science as it’s done, science in practice, and maybe interweaving some interesting kind of philosophical, epistemological issues there.
- KM** The one last thing that I want to ask you is about the future of STS. How do you see the future?
- SW** I am very positive about it. As I was saying earlier, STS has this fantastic propensity for arguing with itself, renewing itself, and coming up with new puzzles, new curiosities, and new phenomena to study. As long as one is careful not to get into a rut, not get into a standardized way of seeing things and forcing people into particular ways of doing research then it will go on growing and expanding. I am slightly, just slightly, worried that so many different things nowadays get counted as “STS,” but on the other hand we need to continue to be expansive and generous: all publicity is good. I am interested in whether and how STS can make inroads into other areas. In the particular case of business schools and management, there are some interesting arguments which say we are coming to the end of the MBA and that MBA is now recognized as not teaching people very much. Perhaps it needs to be renewed, and maybe STS has a future role to play in business schools in this regard. People say that education has become so vocational and that luxuries like STS are going to get squeezed out but I don’t see it as luxury. For me the Science Studies Unit has always been, certainly in its origins, slightly distant from policy issues and so on, certainly compared to places like SPRU. On the other hand, more “relevant” research goes on. For example, Steve Shapin has an interesting trajectory. He is something of a traditional scholar in the history of science but has recently done quite a lot on business in science, which is about how science survives and grows by making relationships with the business community.

- KM** That's interesting. Donald MacKenzie is also doing studies of financial market.
- SW** That's true. Donald's work on finance has been very influential in lots of different ways. He and I were at a meeting organized by Linköping University on algorithms. Algorithms is a new hot topic. It's fascinating because here is this massively widespread technology which appears to automate decisions and is the ultimate kind of technocratic system—you outsource the responsibility for a decision to a mathematical formula. Donald is working on that and some of the Linköping people too; not so much about use of algorithms in business but in state organizations, in security, for example, what kinds of surveillance of data do you develop in order to spot likely terrorists, or predict the spread of disease. I think that's fabulously interesting. Again a really good opportunity for STS to do something quite critical and quite revealing of the assumptions that are being made, and at the same time doing that in relationship with the people who are building these things.
- KM** I feel sometimes STS people are caught up in policy terms and the critical bit can be left out.
- SW** I don't know about that, it depends what you mean by "critical." What counts as critical is often not well understood. There is one model where you say, "I absolutely stand away from the thing I am studying so I can be really critical of it." One difficulty is that nobody takes notice of you if you stand completely away from it. A good example within business and management studies is "critical marketing." Critical marketing is very critical of marketing along traditional Marx-ish lines. It's the same thing again and they say, "this is outrageous, the assumptions they are making, taking advantage of consumers" and so on. Much more interesting to my mind is *how* and *with whom* you communicate in being critical. It's especially ironic in the case of critical marketing because marketing purports to be about communicating with people and trying to sell them what you've got. We, social scientists, are just so poor at selling people what we've got. I think we could much better sell people criticism. I think that's very important. Bruno had that lovely article called "Why Has Critique Run Out of Steam?" (Latour 2004). His complaint was that all the early heady days of STS had come to nothing particularly because the worst people in the world—climate deniers and holocaust deniers - are now using constructivism to argue for their case. My hunch is that his argument is based on a rather poor understanding of what critique is or can be. I don't think we've really looked into what being critical could be. I think STS has loads of potential for developing that much further.

STS as a Program of "Ontological Disobedience"

BY KOICHI MIKAMI

I was excited about going to Oxford and doing this interview with Steve Woolgar, not only because he is a distinguished scholar in STS, but also because of his reputation for being quite extreme in

terms of his emphasis on reflexivity. On receiving the Society for Social Studies of Science's (4S) J.D. Bernal Prize at the Society's 2008 Annual Meeting, he gave a talk on mundane governance using cases of "breaching experiments."⁴ Some of my Japanese colleagues felt his work was too philosophical and impractical, and I confess that at that time I shared their feelings to some extent. And now here came the opportunity to explore how Woolgar himself sees his place in the field.

About ten minutes into the interview, however, I learned that he had already published an autobiographical piece. My excitement died down and soon I became concerned that my interview would mostly repeat what he had written there. So on my return from Oxford, I went to the library and grabbed *The Disobedient Generation: Social Theorists in the Sixties* (Sica and Turner 2005)—the edited volume in which his piece appears. To my relief, it turned out that his piece, while similar in some regards, is quite helpful in teasing out an important lesson from the interview. This reflection piece is therefore my attempt to demonstrate how I understand the lesson—the importance of not only being critical of our own work but also of being inventive in selling such critique to our colleagues in order to foster the strength of our field.

Instrumental and Ontological Disobedience

In his autobiographical piece, Woolgar (2005) draws a contrast between two kinds of disobedience—*instrumental disobedience* and *ontological disobedience*. The former is a challenge against an existing orthodoxy in order to articulate and establish an alternative position. Its ultimate goal is to build a new orthodoxy. The latter is "a form of constant revolution," intended "to be constantly unsettling, challenging, destabilizing but with no specific end in mind" (Woolgar 2005:314). The former is temporary; the latter endures.

In the interview, Woolgar highlights the continually changing, reflexive features of STS, in a way that corresponds well to the second kind of disobedience. Yet, there is nothing intrinsic about this correspondence. In other words, it demands considerable work if one is to construct our field as one kind of disobedience instead of the other. Our field started as a challenge to the then-dominant deterministic, positivist view of scientific knowledge, and there have been some decisive moments in its history when it could have established itself as a new orthodoxy and become static. Woolgar's arguments against the simple application of SCOT formula to studies of technology and similar comments in his autobiographical piece in which he argues that the Strong Program in the sociology of scientific knowledge (SSK) offered a "relatively stable system of explanation" (Woolgar 2005:320) indicate that these were some such moments.

However, the growth of STS over the last 30 years, which Woolgar both celebrates and worries about, and also our rather familiar experience of finding it difficult to define exactly what the field "is," suggest that our field has retained its open-ended and never-at-rest-with-itself characteristics. In some sense, we, STS scholars, are not disciplined; instead, we participate in a program or a mode of viewing and thinking—hence the title of this reflection piece. From this perspective, Woolgar's lifework of challenging taken-for-grantedness—not only in science and

⁴ They also appear in Woolgar and Neyland (2013).

technology but also in mundane objects and everyday practices—and his being both provocative and “dangerous” in our field represent an effort against the establishment of STS as a new orthodoxy: a way of keeping it as a form of ontological disobedience.

For Being Recognized as “Dangerous”

The anthropologist Mary Douglas (1966), who also won the J.D. Bernal Prize in 1994, argued that “danger” is a state of being out of place, or more precisely, that of being recognized as such. In the interview, Woolgar mentions that on their return from 4S Annual Meetings, his students are rather envious of the early days of STS when there was a sense of “danger” in the field. If this captures a general feeling among newcomers to our field, then perhaps we should be worried that our program is changing its character—more towards instrumental disobedience, establishing itself as a new orthodoxy and becoming a stable and comfortable home to those who currently take part in it. That might not be a bad thing. As a new orthodoxy it can possibly secure us some seats in education, in policy, and even in business. Such seats, however, probably come with clearly defined roles to perform, leaving little space for critical engagement with the topics we study. If that is not what we want, then we have to do something about it. Here I see a valuable lesson from my interview with Steve Woolgar.

From the interview, I felt that a key reason why Woolgar is accepted as a central figure in STS is, paradoxically, his career-long effort to remain peripheral to it. His “discovery” of the inventive and unusual French scholar Bruno Latour and their collaboration on the book *Laboratory Life* (Latour and Woolgar 1979) were early instances of his effort to challenge the mainstream approaches in the then emerging field. The difficulty of describing his analytical approach—other than calling it (thoroughly) reflexive—might also, at least partly, undergird his position in our field. Unlike SCOT or SSK,⁵ his approach cannot be described without invoking his name, which another prominent STS scholar, Trevor Pinch (1993), once did.⁶ In his autobiographical piece, Woolgar describes such an ambivalent way of being as “a familiar anthropological mode of managing being an insider and an outsider at the same time,” which allows one “to stay engaged [...] while remaining skeptical” (Woolgar 2005:311). It is this ambivalence that has allowed him to stay “dangerous” in our field and remain skeptical of our own knowledge.

Playfulness for a Purpose

For Woolgar, being simultaneously a member and a stranger is a strategy to get noticed, and this strategy allows him to sell his critique. In order to sell others our critique and influence their practice, we need to be noticed by them first; only then can the relevance of the critique be

⁵ In the interview, Woolgar actually mentioned: “Acronyms are first used by the Bolsheviks as a means of social control. If you knew what the acronym meant, then you were part of the system of regulation. So whenever you get new acronyms coming up in STS, be wary.”

⁶ In responding to Woolgar’s (1991) critique of “the turn to technology” in social studies of science, Pinch (1993) called his approach “the Woolgar Formula.”

examined. Therefore, where one stands is highly important. This explains why Woolgar particularly liked the title of “professor of marketing” and the curiosity it aroused: the title guaranteed his membership in the community of business and management, even though he is critical of their practice. His experience of directing the ESRC Virtual Society? Program also suggests that we may be able to represent our own standing strategically in relation to existing preconceptions about what we study—in his case by simply inserting a question mark into the title of the research program.

When it comes to selling our critique to ourselves and maintaining our field’s dynamic and reflexive intellectual culture, however, positioning oneself in such an ambivalent status can be a challenge. The elasticity of STS and its inclusiveness with respect to different approaches mean that one’s position depends on how the field of STS is understood. Woolgar’s recent work suggests an interesting solution to this challenge. In an article he wrote with his Oxford colleague Javier Lezaun, a question mark again appears in its title: “The wrong bin bag: A turn to ontology in science and technology studies?” (Woolgar and Lezaun 2013). They explain that such use of the question mark “is not merely a matter of whimsy or stylistic preference” but is “a deliberate choice” to sell their argument (Woolgar and Lezaun 2015:464). By obscuring and confusing where they stand, the authors take advantage of the elasticity of STS and invite their readers to engage with the original argument and appreciate its relevance from different perspectives, while reflecting on how they understand the field.

Can We Sell Us “What We’ve Got”?

This playful but serious argumentation has been a remarkable feature of STS⁷ and can be observed particularly when one tries to engage critically with our own field.⁸ In a society in which we are expected to make an “impact”—whatever that means—using our STS sensibilities, it may be inevitable that we incline toward a focus on selling our critique to others. However, we should also remember that for at least the last three decades our field has grown and flourished with some deliberate efforts to keep it as a form of ontological disobedience. Selling ourselves our own critique can be potentially more challenging than selling it to others, but the interview with Steve Woolgar suggests that the more we claim that our knowledge is valuable for others, the better we need to be at engaging critically with our own field and knowledge claims.

Author Biography

Steve Woolgar is Professor at the Department of Technology and Social Change, Linköping University, and Professor of Marketing Emeritus at Saïd Business School, University of Oxford. Before joining Oxford, he was Professor of Sociology and Founder and Director of the Centre for

⁷ A remarkable example of this kind of playfulness, which came to my mind immediately, is the work of Bruno Latour published under a pseudonym (Johnson 1988).

⁸ A notable example of this kind of engagement, suggested by Steve Woolgar, was the work of Malcolm Ashmore (1989).

Research into Innovation, Culture and Technology (CRICT) at Brunel University. Under the supervision of Michael Mulkay, Woolgar studied the discovery of pulsars for his doctoral research at Cambridge. Since then, his work has examined a wide range of topics, from information technology and neuroscience to governance and accountability and provocation and intervention. His work exemplifies the value of the “sensibilities” that science and technology studies (STS) offers in de-stabilizing existing preconceptions and assumptions and presenting other possibilities—characterized by the phrase “it could have been otherwise.” In 2008, he was awarded the J.D. Bernal Prize of the Society for Social Studies of Science for his distinguished contribution to the field of STS, and is known particularly for his reflexive analytical stance. His ability to be critical of the subjects that he studies while at the same time to be engaged with them has been pivotal in his work, including his directorship of the ESRC Virtual Society? Program, and can provide a valuable lesson in thinking about the future of STS.

Author Biography

Koichi Mikami is a social scientist of the life sciences and biomedicine. He naively entered the field of science and technology studies because of his interest in cultural differences in perceptions of life, body, and life course. He completed his doctoral training at the University of Oxford under the supervision of Professors Steve Rayner and Sarah Harper in 2010. His doctoral study examined the role of research environments in shaping future trajectories of biomedical intervention—in this case, tissue engineering and regenerative medicine—based on fieldwork conducted in the UK and Japan. At the time of this interview, Koichi was a research fellow of the research project entitled “Making Genomic Medicine,” a Wellcome Trust-funded medical history project led by Professor Steve Sturdy at the University of Edinburgh. His research investigates the roles of patients and patient organizations in making “rare diseases” an important agenda for biomedical research and public health policy, as well as the costs that they have to bear in order to become and then remain active in highly-specialized domains of science and policymaking. In April 2017, he took up the position of project assistant professor of Science Interpreter Training Program at the University of Tokyo but continues to be part of the project as a visiting research fellow.

References

- Ashmore, M. 1989. *The Reflexive Thesis: Writing Sociology of Scientific Knowledge*. Chicago, OH: University of Chicago Press.
- Douglas, M. 1966. *Purity and Danger: An Analysis of the Concepts of Pollution and Taboo*. London: Routledge and Keagan Paul Ltd.
- Johnson, J. 1988. “Mixing humans and nonhumans together: The sociology of a door-closer.” *Social Problems* 35(3):298-310.
- Latour, B. 2004. “Why Has Critique Run out of Steam? From Matters of Fact to Matters of Concern.” *Critical Inquiry* 30:225-248.

- Latour, B. and S. Woolgar. 1979. *Laboratory Life: The Construction of Scientific Facts*. Princeton, NJ: Princeton University Press.
- Pickering, A. 1992. *Science as Practice and Culture*. Chicago; London: University of Chicago Press.
- Pinch, T. 1993. "Turn, Turn, and Turn Again: The Woolgar Formula." *Science, Technology, & Human Values* 18(4):511-522.
- Sica, A., and S. Turner. eds. 2005. *The Disobedient Generation: Social Theorists in the Sixties*. Chicago, OH: University of Chicago Press.
- Woolgar, S. ed. 1988. *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*. London: Sage.
- Woolgar, S. 1991. "The turn to technology in social studies of science." *Science, Technology, & Human Values* 16(1):20-50.
- Woolgar, S. 2005. "Ontological Disobedience—Definitely! {Maybe}." In *The Disobedient Generation: Social Theorists in the Sixties* edited by A. Sica, and S. Turner. 309-324. Chicago, OH: University of Chicago Press.
- Woolgar, S. and D. Neyland. 2013. *Mundane Governance: Ontology and Accountability*. Oxford: Oxford University Press.
- Woolgar, S. and J. Lezaun. 2013. "The wrong bin bag: A turn to ontology in science and technology studies?" *Social Studies of Science* 43(3):321-340.
- Woolgar, S. and J. Lezaun. 2015. "Missing the (question) mark? What is a turn to ontology?" *Social Studies of Science*, 45(3):462-467.