Engaging Science, Technology, and Society 4 (2018), 366-385 DOI:10.17351/ests2018.220

It's Important to Go to the Laboratory: Malte Ziewitz Talks with Michael Lynch

MALTE ZIEWITZ[1]
CORNELL UNIVERSITY

MICHAEL LYNCH[2]
CORNELL UNIVERSITY

Abstract

Why would anyone still want to go to the laboratory in 2018? In this interview, Michael Lynch answers this and other questions, reflecting on his own journey in, through, and alongside the field of science and technology studies (STS). Starting from his days as a student of Harold Garfinkel's at UCLA to more recent times as editor of *Social Studies of Science*, Lynch talks about the rise of origin stories in the field; the role of ethnomethodology in his thinking; the early days of laboratory studies; why "turns" and "waves" might better be called "spins"; what he learned from David Edge; why we should be skeptical of the presumption that STS enhances the democratization of science; and why it might be time to "blow up STS"—an appealing idea that Malte Ziewitz takes up in his reflection following the interview.

Keywords

history of STS; Michael Lynch; interview; laboratory; ethnomethodology; parasites; explosive STS

MZ The field of STS now seems to be at a point where people enjoy a good origin story. Do you have one you would like to share?

ML I'll give you one that's very familiar, which gives a lot of credit to Edinburgh. I think there's a solid basis for this story, certainly other people will tell a different one and we all know that these things are constructed in light of where we are in the present. Obviously there has been history, sociology, philosophy of science for quite some time, so that to speak of STS in either of its guises as science, technology and society or science and technology studies gives it a kind of identity that didn't exist until around the 1970s.

[1] Malte Ziewitz, Email: mcz35@cornell.edu
[2] Michael Lynch, Email: mel27@cornell.edu

There were programs at Cornell and elsewhere, going back into the '60s, perhaps earlier. I haven't really traced them back much further than the '60s. These were teaching programs generally, similar to programs in engineering ethics or bioethics in universities today. This is still the case for some science, technology and society programs that mainly aim at providing humanistic education for undergraduates in engineering and sciences. There is an undergraduate honors thesis written at the University of York by Thomas Kelsey in 2013 that points to how the Edinburgh program developed from one that scientists such as C. H. Waddington helped to instigate. The thesis is entitled (it's a quote from David Bloor): "I'm not sure that Prof Waddington really got what he had hoped for." In line with the quote from Bloor, Kelsey describes how the program was set up by scientists and university administrators to broaden the education of science students, but the Science Studies Unit soon pursued a different agenda, which was to study science as a social phenomenon. I don't know how people in Edinburgh would react to the thesis, whether they'd see it to be accurate or not, but it made sense to me. We think of STS as something that started in the early '70s and as developing out of the sociology of science. It isn't necessary to think of it that way, but if you do think about it that way, clearly it had to do with critiques of Merton that were influentially launched by people like Mulkay (1976), who wasn't at Edinburgh but was allied with David Edge, who was the first director, and by Bloor and Barnes. The key move was to treat the sciences not as a model for sociology to emulate or as another institution within a framework of social theory, such as in Merton's or Talcott Parsons' theory (even though Merton was very keen on a sociology of science, it was one piece of what he did and one piece of what other sociologists did, and it was very much firmly ensconced in the methodological and theoretical strains of sociology at the time). Edinburgh was very important for leveraging it out of that kind of trajectory. The Unit had an institutional base, which existed since the late 1960s, and produced some very influential writings, treating the sciences as something to investigate without prior commitment to their epistemic status or methodological formulae. I'll get to my debates with Bloor later, but he and the Unit should be credited with making a move to be somewhat autonomous from the sociology of science, and of course they drew heavily from both the history and philosophy of science, primarily Kuhn but also Lakatos and to some extent Popper and Feyerabend. I think that it was essential at the time to break from the pre-existing sociology of science, which had two aspects to it that are still around, one being the bibliometric studies or sociometric studies as they called them, which attempted to map networks of citations, using them as indicators to get a sense of fields, the other being the theoretical connections to the structure of social action and theory of social structure from Parsons and functionalism. And then Latour and Woolgar's book and the other ethnographies in the 70s became sensible things to do in light of what already had been argued in Edinburgh. Social constructionism already was around, of course, but applying it to natural science seemed like a startling thing to do: treating the sciences as open to investigation, as open to criticism. Starting with the Strong Program, there was a buildup of concepts like symmetry, construction, social construction, the deconstruction of the social in Latour's later work, and Woolgar's and Ashmore's concerns about reflexivity. Those developments built momentum, so that we now have a field that has a literature and a set of concepts that students can go back to. That was, I think, pretty important in the United States when the Cornell Department formed, and also the Science Studies Program at UCSD. NSF sponsored both and they became flagship programs. MIT's program was already around and had somewhat different cast of characters and fields, and probably would tell a different history than I would here or as you would get out of Edinburgh.

MZ When I look at your CV, it's not quite clear when you entered that origin story yourself. Your own training was in sociology as an undergraduate at Cornell and later in ethnomethodology as a PhD student at Irvine, where you worked with Harold Garfinkel and what he called his "company of bastards." How did the connection with science studies come about? I assume you could have stayed rather comfortably within the field of ethnomethodology and focused on that.

ML I didn't know anything about the science studies that was going on in Britain when I started but I was very taken with Melvin Pollner's notion of reality disjunctures (Pollner 1975). His key sites for explicating this idea were two. One was psychiatric hospitals, where patients had rather extraordinary experiences that were interpreted as delusions and hallucinations because of their lack of congruency with what the psychiatrists believed were valid experiences. The other was a very mundane setting of a traffic court where people with traffic tickets come to contest or mitigate fines for speeding and running through stop signs. Pollner had this idea that exchanges in these settings were founded on what he called mundane reason. For example, if a cop says that you were driving your car at 45mph and you claim that you weren't over the 30mph speed limit, something has to give, you can't both be right. Mundane reason has to do with the idea of a singular, foundational world that stands in judgment of any accounts that different people would give, and Pollner was interested in how such disjunctures would be resolved with accounts such as, "this one's lying" or "the policeman's view was blocked" or "the speedometer was broken" or "the radar detector wasn't working properly"—those kinds of mitigating things. I found that very interesting, and like everybody I was reading Kuhn (1962) at the time and steeped in the notion of incommensurability. Kuhn was assigned when I started graduate school in just about every class in every subject, and not just for philosophy or history of science. Gregory Bateson's *Steps to an Ecology of Mind* was another text for all subjects. Again, I was in the School of Social Sciences with about 40 faculty, they had no departments and so you had geographers, economists, sociologists—very few sociologists actually, most of them were ethnomethodologists. I liked that kind of mix, I wanted to get out of the straitjacket of sociology. This idea of disjuncture led me to write some bad historical papers on Antonie van Leeuwenhoek's first discoveries and how they were received at the Royal Society. I never published those. I think one was going to be published in a journal that folded. Much later, I showed one of the papers to Steven Shapin and he said, yes indeed, they were truly bad. Nonetheless, they got me started on the idea that maybe in the sciences you would get some very interesting disjunctures, and could address questions on how they were resolved. Through a friend, I talked to a fairly well-known biology professor at Irvine who said that I really needed to get to the cutting-edge of a field to get access to debates that occur as the practitioners are doing their research. Another faculty member in Social Sciences, I think it was Louis Narens who knew Garfinkel, recommended that I look up a professor in the psychobiology department, Gary Lynch (no relation to me). He was a young professor who was very well thought of, doing some ground-breaking research on neuroanatomy and neurophysiology, to do with the recovery of the brain from injury. He was very open to my crossing the campus and spending time in his laboratory, so that got me started with that kind of ethnographic work. I didn't know that, at the time or shortly after that, others – specifically Bruno Latour, Sharon Traweek, and Karin Knorr-Cetina—were also doing ethnographic fieldwork in California laboratories. So I was not coming out of science studies but coming out of ethnomethodology with this kind of particular interest in reality disjunctures and how they were argumentatively resolved. In Gary Lynch's lab, I was involved with a group of electron microscopists who were doing a study of what they called "axon sprouting," and this got me interested in visual studies, because they would assemble and use micrographs, and I spent a lot of time observing and talking with them as they assembled micrographs and selected and marked particular things of interest. This was, in a way, consistent with ethnomethodology, especially a program in ethnomethodological studies of work that I'm associated with, and which partially lashes up with STS. In Harold Garfinkel's *Studies in Ethnomethodology* (1967), some of the studies are studies of sociology—of sociology research projects, including especially some that he actually performed himself with students. One such study involved student research assistants coding recorded interactions, using a coding scheme [Bales' Interactional Process Analysis] for actions on the tapes, and the students would map them out by reference to the categories in the coding scheme. That's where Garfinkel came up with the idea of "ad hoc practices," for encoding data, and he also had another study of how clinic records were interpreted (he and Egon Bittner wrote a chapter on "Good organizational reasons for bad clinic records"). So, in studies of natural science work, Garfinkel's orientation towards social science research was drawn upon; not only by me, but also by people like Michael Mulkay, and perhaps some in Edinburgh, to look at the detailed laboratory practices in biology, in physics and so forth, where so-called raw data are translated into organized mathematical arrays or statistical distributions. This was something that I didn't initially set out to do, but I saw a lot of that sort of coding being done with micrographs, and my observations linked up with the work that Latour, Woolgar, and others were doing. By the time I'd finished my dissertation I knew these people; not very well, but I'd met Latour, and I started reading Bloor when writing my dissertation. One of my chapters in the dissertation is an argument with Bloor on notions of agreement in Wittgenstein

(Lynch, 1985), which is kind of a precursor to the exchange that Bloor and I had in Pickering's book a few years later (Pickering 1992). I was coming at it differently, always with a somewhat different orientation than constructivism. In my view, ethnomethodology had already done a kind of constructivist study of the social sciences, where at the time it was often viewed as a critique of social science method showing that it's really just common sense or it's ad hoc, it's messy, all of those terms. Transposing that sort of approach to the natural sciences seemed to get rid of the distinction [between social science and "real" science] that the irony was based on. Of course this is something that Steve Woolgar and I could argue about and agree on as well.

MZ It's interesting because you guys were doing laboratory studies before it was a thing. There must have been a point in time when that became recognized as a field of study in its own right, and not just a bunch of people who happened to be interested in scientific practice.

ML I think it happened fairly quickly. At a Sociology of Sciences Yearbook Conference in 1979 at McGill, the participants were putting together a volume on not just laboratory studies but also various views of science and practice that were becoming available at the time.[3] The volume that Knorr Cetina and Mulkay (1983) put together, which highlighted constructivism but other things as well, also was key. There was a meeting at RPI in 1980 that also pulled these things together.[4] Two themes pretty quickly emerged. One was—and this is explicit, especially in Latour and Woolgar's (1979) book, which was the first one published—that science is constructed all the way down to the bench: it's not just that construction has to do with bad science, making up data or mistakenly interpreting artifacts as if they were natural substances and so forth. The idea was that science is quotidian, that ordinary conversational practices, messing around with your hands and instruments, ad hoc interpretation, these are all part of what is put together—what in the end looks very mathematically tractable and so forth. It's like the frantic process of preparing a meal in a restaurant, which later looks nicely delivered when you get it on your plate. Of course, there was plenty of sociology around to foster that way of thinking. The other theme was, and this very quickly emerged even by the time of Knorr Cetina and Mulkay's book in 1983, that this kind of work is not enough, you need to go beyond the laboratory, you need to bring in the big "S" sociology themes. And so that quickly reverts back to established views of sociology or politics or whatever. It's something I resisted because I figured that first of all that—and actually Park Doing (2008), who received his PhD in our department, has taken this line—the early laboratory studies remain open to a lot of questions, but many of those questions were closed off fairly early because it seemed so convenient that laboratory studies "proved" that science is constructed, whatever that means. But *whatever that means* is very important, and I think that we're still living under a question about what that could possibly mean. Even

[3] This resulted in the volume, *The Social Process of Scientific Investigation*, edited by Karin Knorr, Roger Krohn, and Richard Whitley, Volume 4 of *The Sociology of Sciences Yearbook*. Dordrecht: Kluwer. 1980.
[4] RPI Laboratory Life Symposium. Rensselaer Polytechnic Institute, Troy, New York, November 1980.

though the term *construction* is no longer much used, there's still a question of what is happening in science that could give it a special epistemic status. And I think that if you travel in 4S circles it's not a question anymore because it has been placed in the past. But if you go outside those circles, it's as if STS never existed. So there's a big gulf between understandings of the sciences in STS, and understandings by many scientists. (Of course there are scientists who have dialogues with us that are very interesting.) And then there is the general public, or the law courts, where science is treated as facts, and maybe interpretations sometimes, but there are those facts.

MZ Let's think a bit about what happened in the 1980s and 1990s. There's a couple of ways of talking about developments in STS. The most popular and persistent ones are arguably tropes like "turns," "waves," or "symmetries." There are so many by now that it is easy to lose track of them. Do you have a way of describing key moments in the development of STS, or is that too big a question to ask?

ML I think those turns might better be called spins. Because often they are efforts by people who have an agenda to get into the picture. Not surprisingly, many of the debates that you find elsewhere in the social sciences and humanities emerge in STS. But this historicist mentality of turns and waves, it's kind of strange for STS because you think OK, we see what's going on.

MZ Why do you think it's so prominent then?

ML I guess it works to get the agenda noticed. There's a funny thing you could see in many proposals of new turns. They acknowledge that they are using a simplification, and that obviously things don't work quite so easily, but nonetheless they're going to use it. You see this in the much-maligned [and much-cited] Collins and Evans "third wave." They explicitly say, obviously packaging the history of STS into waves is artificial, but here they are (Collins and Evans 2002, 237). And then people pick up the lingo: "am I in the first wave or the second or the third?" It's almost like Merton and the functionalists, who just loved fourfold tables, and they weren't alone. Sometimes lists of threes—these are pedagogical devices that are very effective little graphs and tables with names in the cells, and they work, but unfortunately people take them too seriously. I didn't answer your question did I?

MZ Well, you put a nice spin on it. You already mentioned the debates, which might be seen as another feature of the field. There seems to have been a time in the 1980s and 1990s when people worried a lot about chickens, bridges, death and furniture. Would you say that was something specific to that period or is it only hindsight that makes these arguments stand out?

ML I don't know. The first major one that I can remember was the Bloor/Laudan debate and I actually was there for that, it was in Toronto at a big meeting.[5] At the time, I noticed—not being overly partisan to one side or the other—that the audience treated it as though it was some sort of boxing match to see who would win. So there was that drama and

[5] The Present State of Social Studies of Science: A Symposium. Toronto, 1980.

presumably a very male sort of thing, a very British sort of thing, since in philosophy and the social sciences debating is one of the conventions of presenting and establishing work. I think that debate was very important for Bloor and others to try to establish that they wanted to make some big move, and these moves were resisted. Various philosophers like Laudan clearly tried to point to weaknesses in the arguments and I think that these debates themselves became items that helped to promote STS. It was an effective way to go. The next big wave of debates, if there is a wave of debates, were often cited as the chicken debates, and they followed the move that Latour made on putting non-humans into the picture and doubling symmetry. Incidentally, there's a bit of a story on that. Andy Pickering had planned a volume that he called *Science as Practice and Culture*. I'm not sure when the planning started but probably around 1989 or 1990. I was invited to write a chapter on the sort of empirical part, while other people would write about theory, and I rebelled against that. I'd written quite a few empirical papers already, and I wanted to write a polemic. At the time, I was at Boston University, learning a lot about Wittgenstein from Jeff Coulter. I was interested in Wittgenstein earlier, but contact with Coulter and his students intensified this interest. They had a very different version of Wittgenstein than the version in STS, either through Bloor or through Harry Collins, and so I wanted to put out a different version of Wittgenstein, different from Bloor's particularly, although I mentioned Collins as well. To put it in a very concise frame, the resources I was using were philosophers like Gordon Baker and Peter Hacker, who were the mainstream expositors of Wittgenstein in philosophy, and the argument was about actions in accord with rules, exemplified by the rule "plus two" —counting by twos. The arguments out of Baker and Hacker are in part pitched against Saul Kripke's arguments about Wittgenstein and the rules argument, which in Kripke's case resorts to notions of disposition to get past the relativistic arguments that question how you can maintain a mathematical formula through endless examples. I ran that argument at Bloor, where Bloor was using Wittgenstein's argument, as well as skeptical arguments from Quine and others, to open up logical uncertainties about the compulsive force of logic—the compulsion of mathematical order. According to such arguments, the skeptical problem is solved through the intervention of social conventions: through training, through discipline and that kind of thing, which reverts to society as Durkheim envisioned it and as other sociologists envisioned it. Coming out of ethnomethodology, I had less faith in sociology than people like Collins and Bloor who came out of other fields. [As Collins has said, distance lends enchantment.] And so the issue for me was that you can't revert to sociology to explain the sciences, because, first of all, the resources in sociology are pretty thin for explaining something that's extremely intricate when you view it in practice: it has its own concepts, its own methods, its own rigorous, or not, training. And sociology isn't some sort of storehouse of theory and method with which you can explain everything else. I had debates with Latour about that and he accepted part of that argument, and he began to move away from the idea that you explain the sciences with sociology, since the question is, why would you use "the social" *that* way. He acknowledges Garfinkel's influence in his *Reassembling the Social* (2005). This was the issue with Bloor, and we had this debate.

MZ That was the moment when you entered the boxing ring, as you called it, yourself. Did you feel comfortable doing so?

ML It wasn't a boxing ring, it turned out to be a love-fest because I wrote the first part of this argument and sent it to Bloor, and he said he would like to write a reply and graciously mentioned that I should have the last word. I didn't know him, I think I'd just met him a few times and had very brief exchanges with him, but he was a real gent in the way he responded, and we actually became friendly as a result. A year or two later we were at San Diego at the same time and I got to know him and his wife Celia, and son Conrad, probably named after Waddington. David is a very fine fellow and he handled this debate very well. But also, I think Collins got wind that we were doing this and so he broke out of what he was going to do for this book Pickering was editing, and started the chicken debates (Collins and Yearley 1992). That was a very fractious thing that nonetheless got a lot more attention than what Bloor and I did, but was touched off indirectly by our exchange.

MZ So despite, or rather because of, those arguments you were in very good standing with the Edinburgh group. You even took over the role of editor of *Social Studies of Science* from David Edge. How did that come about?

ML Very gradually. I think I first met David Edge at a 4S meeting in Ghent [1984]. We hit it off pretty well, he was very easy to hit it off with, a very affable fellow, a great communicator, and I remember at the 4S in '86 we watched the Boston Red Sox lose to the New York Mets in a very tragic game. I had just started at Boston University at that time and David was a big sports fan. He was of course the first director of the Science Studies Unit and one of the founding editors, along with Roy McLeod, of *Social Studies of Science*. He started sending me papers to review and I think he liked what I did, and I kind of enjoyed reviewing them. There were a couple of key papers that I did a lot of editorial work on as a Collaborating Editor for the journal, one was Stefan Hirschauer's paper on surgery, the other was Joseph O'Connell's paper about metrology and both ended up being very good papers by young scholars. Edge had cancer diagnosed and treated going back to around 1990, maybe even a little earlier, and he knew that he would need a successor as editor. When I moved to England in 1993, he asked me if I could be in-waiting, just in case he got too ill to continue. Fortunately for all of us, he continued for almost another decade, until he retired from the journal in 2002. We went through a formal process with the editorial board and publisher, but for the most part I was designated to succeed him and the 3S editorial board went along with that plan (3S has no formal connection with 4S). Unfortunately, he died within a year after giving up the editorship. My relationship to him certainly connected me to the STS field, but I was already involved with it before I met him. When I moved to Brunel University in 1993, I was no longer in a sociology department [the department was named Human Sciences]. I taught sociology courses, but I was in a very strong set of people in STS, including Steve

Woolgar and Alan Irwin, so that was really part of my intellectual milieu. Of course, when I came to Cornell [in 1999] STS had become my main intellectual environment and sociology and ethnomethodology were less a part of what I did and what I thought about, although I'm certainly still connected to ethnomethodology.

MZ What did you like about the editing work at *Social Studies of Science*? I know a lot of colleagues, who would have a more skeptical view of reviewing and reading other people's papers rather than writing their own. What attracted you to the job?

ML There were a lot of drawbacks to the time involved. It was partly David Edge's example. He was a very intensive editor, copy editor as well as in all the relationships involved in editing. I think if you talked to Trevor Pinch, Harry Collins, and many other people, they'd say that David and his collaborating editors gave them a lot of help with their earliest papers. I also once read something about Merton, about how he spent a huge amount of time editing other people's work, and I found that it was something that was highly valuable, both as a pedagogical and intellectual exercise. If you engaged with the content, which an editor has to be careful about doing, but you're not just correcting typos and grammar you're helping somebody's expression of thought, and the thought itself of course. So, it's a learning experience for the editor as well as hopefully the person who is being edited, and it's something I do with graduate students—undergrads not so much—that I think is really important. The more you do, the more obsessed with it you get. It is one of the reasons I quit after ten years. I noticed this with David as well. You can get obsessive. You want to back away from it when it gets like that, because you can't read anything without wanting to reorder the phrases, and it just gets nutty. But it was something that I found as a fascination. I often regretted the time I spent on it because there were other things I could have done, but it was a preoccupation.

MZ Did you think of yourself as a tastemaker or as a gatekeeper of what counts as STS? Especially at the time, *Social Studies of Science* was one of the very few outlets for people who wanted to publish under that label.

ML There was a time when I was in quite a few gatekeeping roles, if you call them that. I was impressed by how little it shaped the field, and I don't think anybody in particular shapes the field. When you're an editor, basically you're taking advice from reviewers, and unless you're a really bad editor you almost never override a clear set of reviews. If the reviews say this is a great paper, you read the paper and you don't think it's so good at all, you still have to go with what they say; otherwise why bother sending it out for peer review? Similarly, with papers where the reviews come back negative, most of the time I'd agree with them. However, I also did unilaterally reject a substantial number of papers when I could not see the point of sending them to reviewers. Occasionally when I did so, reviewers would write back and say "why did you send this to me it's so terrible; get rid of it yourself." So I tried making judgments of that kind. I don't think it shaped the field, because I did very little work to solicit papers on specific topics. I was always overwhelmed with the number of submissions we already had, they kept increasing in the period of time between 2002-2012, and we had to reject more and more of them.

Obviously, it's necessary to make judgments about who you send papers to, but a lot of the time you don't know who to send them to and so you take advice on that because the submissions we would get in 3S were so diverse. I took advice from others, rather than go with my own judgment on most submissions.

MZ Looking back, how would you say the journal changed over time? At least the publishing landscape in STS has changed quite a bit.

ML That's a good question, and in some ways I'm too close to the ten-year period and the day-to-day work I did on it to get a good overview of it, though I do try to think about that. One thing I recall, from looking back at some of the earlier issues from the 1970s, I was impressed with how you might think that this was the Edinburgh house journal, and so everything would be based on Bloor's and Barnes' early writings, their programmatic themes and so forth, or Collins' work and so forth. Not so. There always have been different kinds of work in the journal. I think that in STS as a whole, and 3S reflects this, you get fewer contributions than before in straightforward history of science or history of technology, because they go elsewhere. Similarly with philosophical work, they don't go to 3S. You get a kind of philosophical work, ruminations about Latour or comparisons between Latour and Heidegger and so forth. But that kind of work is particularized to the STS field as it's recognized today.

MZ Let's briefly talk about the role of STS outside academia. You've done quite a bit of work on that. I'm thinking of your study of the production of uncertainty in the O. J. Simpson trial. You have also written about the phenomenon of STS scholars standing up as expert witnesses in courts, such as Simon Cole's testimony on fingerprint evidence or Steve Fuller's testimony in the Dover case. How optimistic are you about these kinds of research making a difference in those settings?

ML I think that it should not be a surprise to STS researchers that the impact of STS research is contingent. That's one of our slogans, everything's contingent. It certainly works in politics as well as in scientific research about ostensibly non-political matters: how things get taken up technologically or scientifically is not very predictable. One of the things in the paper you mentioned, and in a book that I'm working on now, is that I'm pulling together material from politics, law and STS, and considering the implications of STS. I've written quite a few papers along those lines. There's one that is in a recently published book that probably not too many people will read but it's called *Science After the Practice Turn*, and I wrote a paper called "From Normative to Descriptive and Back: STS and the Practice Turn" that elaborates on some of these points (Lynch 2014). The thing that fascinates me, and I don't have a solution or a direction to indicate from this, but what fascinates me is that there seems to be a presumption in STS that what STS does is that it enhances democratization of science, and that that's good for science. It's good because it levels the authority of science to be consonant with other actors and public constituencies. Accordingly, STS has advice to give to actors in politics, policy circles, or the courts, and there's a political direction to this. It seems that the counterexamples are multiplying. STS research isn't necessarily directly involved, but arguments that are reminiscent of STS arguments are being developed by skeptics about climate science, or by creationists and intelligent design proponents, such as those who enlisted Fuller. You could say that Fuller was making arguments that are recognizable in STS, though it's not like he was presenting himself as a spokesperson for the field.[6] You can criticize his particular arguments, and it was startling to see an academic on that particular side of the dispute, when previously they'd all sided with the Darwinian side, or, rather, on the side that opposed the encroachments of religion or religiosity on the teaching of biology in the United States. This is not a new theme. Sociology of knowledge arguments, or constructivist arguments, or skeptical philosophical arguments can be used to question facts of all sorts, and it is not necessarily liberating to use such arguments. There's one line of argument that you could perhaps entertain in light of this situation, which is that we need a new criterion, or new set of criteria, for demarcating good science from bad science.

MZ Or good STS from bad STS?

ML Yes, good STS from bad STS. That's where I think that the Collins-Evans program is trying to go. I'm sure I oversimplify, but clearly they're trying to do demarcation that's grounded in research in STS. I've got my own arguments against what they're doing, and I don't see it to be working too well, at least not by my lights. Another way would be to reinstall the fact-value distinction, or to somehow develop an STS that backs up the good sciences rather than the pseudo-sciences. But I don't see STS to be very well suited for doing that, given its history of doing almost the opposite—not the opposite, but of questioning these boundaries, questioning these demarcations; or, as I'd prefer to put it, going to cases freshly and not figuring that these cases are going to work out in a consistent way, whether it's epistemically or politically. I do think that there's a problem if STS turns out to be ineffectual: that it offers no resource for people involved in public controversies, other than what they would come up with by exercising their own wits and training, like lawyers do. But I don't see STS in its current constitution to be struggling with that question, I think it *should* struggle with that question. Latour wrote a paper that I actually like, at least part of, "Why Has Critique Run out of Steam?" (Latour 2004). Obviously he's reiterating arguments he's made earlier against critical political vantage points which take a theory—it doesn't have to be a Marxist theory but a critical theory of society that's based on a set of assumptions about how societies work—and apply that to the sciences. For reasons that he's made very clear over the years, he doesn't go in that direction, but I don't think he offers a way to address these reversals that use rhetoric that looks, superficially at least, like STS arguments, in an instrumental effort to oppose regulations against tobacco or to delay action about climate change, and so on. There's an easy way out, which is to renew our faith in science. Or, we could simply count up the scientists on one side versus the other, the 97% solution (as in, 97% of

[6] *Kitzmiller et al. v. Dover Area School Board*. 2005. Available at: http://www.talkorigins.org/faqs/dover/kitzmiller_v_dover_decision.html

climate scientists agree that global warming is real and caused by fossil fuel burning). I don't think that really solves the problem, but I think it's a problem that our societies face, a problem that skepticism about science in general doesn't really help at all.

MZ Are political concerns about society, about public problems something that has motivated you in your work? You've written about the Iran-Contra hearings with David Bogen (Lynch and Bogen 1996), so you could see this as an intervention into a political space.

ML One of the points we make about the Iran-Contra Hearings is that—other people would disagree with this, I think—when you delve into the detail of testimony and, even when you're quite convinced for whatever reasons that these guys [witnesses for the Reagan Administration] are lying and failing to disclose and even admitting that they're shredding documents, you can't get them. Within the existing legal framework, they've got ways of slipping out of the noose, and that's what fascinated us. Not only is it that they can dissemble, and that you don't know if and when they're dissembling, but that they're using not only the language but the operations that they conducted on the ground to avoid detection, so that if they are detected they can deny the operations and avoid conviction, and if they are convicted the damage only goes so high up the government hierarchy. I think that's something that we see all the time in politics these days. The strategies work a lot of the time, they slip up of course, such as when they don't realize they failed to completely erase all traces of their emails or they run into a very tough judge. The tactics were to us quite interesting, not only as tactics but for what they told us about the lack of definitiveness of the legal or quasi-legal institutions that were being brought to bear on these practices. Of course, that's not news to the practitioners. The academic arguments are in the fields we study in funny sorts of ways, situated ways, and I don't see a possibility that, by writing in our university offices or interviewing participants, looking at transcripts of hearings, and so forth, we can solve these problems that have been so carefully crafted to avoid such solutions.

MZ Let's speculate a bit about the future of the field. Melvin Pollner (1991), who you already mentioned, once famously observed how ethnomethodology was "settling down in the suburbs of sociology." It's basically the idea that growing recognition of a field comes at the expense of its most original and promising initiatives. Do you think that something similar might be happening to STS at the moment?

ML I had some disagreements with Pollner about his argument, but I think that is something that happens. You don't need to be a sociologist to recognize the pattern. It's a two-sided coin in a way. The happy news is that students study STS, they have a literature, it's growing larger all the time, they have themes, they write dissertations on these themes or they write them on notable figures in the field and their theories. So it's got an academic status, a field, departments like ours at Cornell; not that many, but some. Of course, it's vulnerable, downturns in budgets in universities could certainly scuttle the whole business, and hopefully that won't happen, but at least the field has more recognizability, visibility, stability than it did 30 years ago, or 20 years ago. It comes and goes, some departments and fields or programs get eliminated, others form and it's not growing by leaps and bounds by any means but it's still a presence. The downside to that is not just the loss of excitement: this idea of moving into the suburbs, where things get safe and boring and everybody grows their gardens and sends their kids to good schools, and there's very little friction. It's not just a romantic longing for those old debates when everything seemed to be up for grabs. There is a problem, I think, with the pedagogy, which is that students can now learn STS through the STS literature, which I think many do, particularly in the humanities. Then, they can figure that this is the picture of science that they can accept, and they can just repeat the slogans that science (any science) is contingent, that it's uncertain, that laboratory work is messy, that scientists are like other people, politics infiltrates science, and so on. I asked a PhD student defending her dissertation a couple of years ago, do you think that science and politics aren't any different, that science is political all the way through? An unqualified yes was her answer. I think that's a problem, because first of all it fails to differentiate among cases, it takes for granted something that had to be established through argument, and I think always, in each case, has to be established through argument, not necessarily because science and politics are separate, but to articulate just how science is political, and what that means, in which sciences, and under what circumstances. This isn't getting dealt with sufficiently. Another thing I've noticed, and I think you can look this up by doing searches of conference programs and journal articles: hardly anybody is talking anymore about physics in STS; they still are doing so to an extent in history and philosophy. Hardly anybody is talking about chemistry in STS. But if you go to STS conferences, most of what's presented is about medicine and biomedicine. STS is diffusing to other fields, to the point where STS is invoked as the body of concepts to analyze just about anything under the sun, almost like a theory. I wouldn't say this is dangerous for the field, it may help the spread of the field, but it takes abstractions as if they were a sufficient basis for understanding the sciences, the subject matter of the sciences, when I don't think that these abstractions first of all are very detailed or challenging, and what they mean in practice has to be worked out. Obviously, a lot of people do work them out, but I think STS vocabulary has become too much of another discourse to throw around abstractly. It's something that is absolutely predictable when a field gets established through 30-40 years, but my tendency is to think that can we invent another one or blow this one up and see what happens, if it doesn't blow itself up. By blow it up I mean just try to find something new to do because it gets kind of stuck in its vocabulary.

MZ So what's the next big thing?

ML I don't know.

MZ What do you want it to be? Is there still a role, for instance, for one of the bodies of work that you've been associated with a lot, ethnomethodology?

ML I've got my problems with where ethnomethodology's going, but that's another topic. I see this as not something that's going to happen, but my own preferences—and I'm not going to do this in my own work at this point—is for people who have the preparation to do it, and that's not everybody, to take a very close look at instances of science that are very well-respected. The idea is not to do hagiography, but approach the practices with an open mind and not just to throw themes at it that other STS studies have done. The reason why I think it's important to go to the laboratory or other site of scientific practice, and I think the macro/micro picture really messes that up, is that if you go back to the philosophy of science, the philosophy of science is about doing projects in science, not that it gets very close to the projects as they're performed. That's micro work in the sense that it's usually fairly small collectives of people doing work on specimen material. At least in biology it's that way. Obviously particle physics is a somewhat different ballgame as Knorr-Cetina and others have worked out. But the details of the practice are extremely important, and I think they're glossed over by all the work that's been done to date, including my own. You can't assume that just because a few people, actually more than a few, have done ethnographies of laboratories that scientific practice is well understood. It's very difficult to understand in every case. I did some research in recent years on nanotechnology and I've written a few papers about it. I'll write some more, but clearly I can't engage with the practices in any depth, it's just too complicated. Why is everybody fascinated with graphene? I can get a vague understanding of it and it's wonderful to hear them in conferences say things like, "it's the perfect substance," and you can quote that and laugh about it but to get into what that's about is really fascinating. I think this is where Latour is actually someone who is worth listening to on these points, he doesn't reject the science, he doesn't see the sciences as doing nothing unusual or being always in the pay of the corporations and the military; not that he doesn't see that, but what interests him is what's being done that is innovative, novel; it brings new things into the world and it isn't just to be written off. It's not like society reproducing itself or culture reproducing itself, or western culture reproducing itself at the expense of everybody else. And so, if what the Edinburgh School began in the 70s was to take a hard look at what scientific practice involves, rather than to explain it away, I think that work has yet to be done.

Blowing up STS

BY MALTE ZIEWITZ

Michael Lynch offers an interesting proposition for the future of science and technology studies (STS): blowing it up. This might seem surprising for someone who is a professor in one of the few departments explicitly dedicated to STS, who served as the long-time editor of *Social Studies of Science*, and who attended almost every meeting of the *Society for the Social Studies of Science* since

1981. From my own reading of the interview, I get the impression that the suggestion is meant to be taken quite seriously. "By 'blow it up' I mean just try to find something new to do because it gets kind of stuck in its vocabulary," says Lynch. So what to make of this call to metaphorical arms?

One striking theme that emerges from the conversation is the extent to which the field of STS has thrived on being parasitic. Lynch's anecdote about the institutional origins of Edinburgh's Science Studies Unit is a good illustration. Tasked with broadening the education of science students, David Edge, David Bloor, and colleagues developed their own spin on the official mission. Instead of providing sociological assistance to a science program and teaching students about the ethical, legal, and social implications of science, they ended up initiating a way of studying science that made scientific practice a veritable object of inquiry. Turning their host into a hospitable environment, they produced a research program that set out to rethink rather than sustain entrenched assumptions about scientific practice. In doing so, the science studies scholars took advantage of the scientists, produced noise that interfered with their original mission, and thus gave rise to a new set of ideas that helped create a field of study in its own right.

The logic of the parasite, as Michel Serres (2007) argues, is the logic of the troublemaker. As the uninvited guest, it operates alongside its host, constituting a *para*-site of activity. In Serres' fable, the parasite exhibits a number of qualities. While some of these fit the popular image of the miserable free-rider, others paint a more differentiated picture. The parasite is an invasive species that takes without giving and weakens without killing; it is a guest that exchanges talk, praise, and flattery for food; and it is a static, a noise, an interference that generates new relations from existing ones. Taken together, these features highlight the dual role of the parasite as someone who not just paralyzes but also catalyzes relations. In other words:

> The parasite invents something new. Since he does not eat like everyone else, he invents a new logic. He crosses the exchange, makes it into a diagonal. He does not barter; he exchanges money. He wants to give his voice for matter, (hot) air for solid, superstructure for infrastructure. People laugh, the parasite is expelled, he is made fun of, he is beaten, he cheats us; but he invents anew. This novelty must be analyzed (Serres 2007, 35).

Parasitism thus takes a distinct "political form" (Lezaun 2011, 740)—it both subverts and generates new problems, audiences, and vocabularies.

The fable provides an interesting model for thinking about the development of the field of STS. Starting out with science as its original host, it could be argued, students moved on to technology and later medicine, markets, the environment, politics, and the digital. In each of these cases, more or less careful study of practices *in situ* yielded new observations and insights. While early ethnographic and historical studies of laboratory work drew attention to the everyday practices at and beyond the bench, technology studies highlighted topics like the role of users or politics in design. Of course, offering accounts of someone else's (or even one's own) practice will likely be perceived as provocation—of realities, ideas of self, or ways of seeing. In fact, perhaps it has been exactly this potential for causing trouble that accounts for the field's reputation as both "innovative" and "destructive." Not surprisingly, this sort of friction tends to trigger a whole range of responses, from attempts to expel the parasite to adaptations to its presence. While the so-called "science wars" of the 1990s might be regarded as a particularly lively illustration of the former, the uptake of STS-ish ideas in fields like media studies, medical sociology, or science education speaks to the latter.

Yet, as intuitive as this tale of STS as iteratively infecting sites of practice may be, it only tells one part of the story. For parasites, as Michel Serres (2007, 4) points out, are "parasitic in a cascade." That is, the role of the parasite is not given once and for all, but rather changes in some sort of "parasitic chain, the last to come tries to supplant its predecessor" (Serres 2007, 4). One person's parasite becomes another person's host. This might be the situation Lynch has in mind when he talks about how "many of the debates that you find elsewhere in the social sciences and humanities emerge in STS." In a reversal of roles, now STS itself is called upon and challenged as a practice of inquiry. Rather than infecting knowledge practices elsewhere, students study "the field" and its "canon." Scholars start talking about "using" STS. Actor-network approaches become a "theory" to be "applied." Job adverts mention "training in STS" as a desirable qualification—until, eventually, the original parasite is "settling down in the suburbs" (Pollner 1991, 370), becoming a host itself in need of a challenger. As Lynch suggests, this might be inevitable from an institutional point of view. However, it also poses a problem to those invested in the practice. Is there still a way of being parasitic when being colonized oneself? Would this even be desirable? Who or what might be the challenger for STS?

Different people will have different answers to these questions, but it might be useful to highlight a number of developments. One ideal-typical solution might be to embrace the new identity as host and work hard on making it sustainable—a strategy of *consolidation*. This would mean a shift in registers and possibly in personnel, investments in more universally applicable concepts, theories, and methodologies, and a set of institutions that sustain this set of practices. This might include, for instance, writing new textbooks featuring "key concepts in STS," canonizing the field in the form of "core readings," organizing summer schools and executive programs that emphasize the utility of STS for a range of academic and professional endeavors, or promoting new techniques like "digital methods." Yet, while this strategy might be a useful way of building "the field" and securing resources, it does come at a cost. As Lynch points out, intellectual stability can mean that things get "safe and boring," resulting in a "loss of excitement." An alternative strategy would therefore be to plough ahead and reclaim the parasitism that helped create the situation in the first place—a strategy of *provocation*. In Serres' (2007, 13) imagery, this would require us to move back to the end of the chain: "The parasited one parasites the parasites. One of the first, he jumps to the last position. But the one in the last position wins the game." So what can be done to reclaim the frontiers of the parasitic cascade—or, in Lynch's words, to "blow up STS"?

Even a cursory look at recent writing reveals that there is no shortage of suggestions. One option would be to resort to philosophy. For the last position in the chain, Serres (2007, 13) suggests, is "the position of the philosopher." A prominent example of science studies scholars-turned-philosophers (at least in Serres' sense) is Bruno Latour and his team's initiative for *An*

Inquiry into Modes of Existence (2013). Drawing together more than two decades of research on a collaborative cross-media platform, Latour (2013, 7) has set out to develop an anthropology of the moderns, "using a series of contrasts to distinguish the values that people are seeking to defend from the account that has been given of them throughout history, so as to attempt to establish these values, or better yet to install them, in institutions that might finally be designed for them." The ambitious project thus reclaims the parasitic frontier by enrolling a range of sensibilities and resources in a form of philosophically-driven world-building. Another option would be to look elsewhere and see how seemingly different analytic sensibilities might challenge existing tropes in STS. John Law and Wen-Yuan Lin (2015), for example, call for "provincializing STS" by making more systematic use of "non-Western analytic resources." Starting from the observation that the field has mostly drawn on Euro-American analytic terms, their goal is to challenge the "analytic-institutional complex" of STS by adopting different perspectives. Meanwhile, Donna Haraway (2016) challenges us to rethink our relations to the earth and its inhabitants more fundamentally. To capture our engagements with a variety of companions and the things that might emerge from them, she introduces the metaphor of compost as "a place of working, a place of making and unmaking" (Haraway, in Franklin 2016, 51)—an idea that resonates well with rethinking agencies as forms of "configuration" (Suchman 2012, 49). Yet another option would be to foreground provocation even more and put a premium on challenging entrenched beliefs both within and beyond the field. An example might be Steve Woolgar's (2004, 347) suggestion that "a central, recurring feature of many different incarnations of STS is the ability to provoke, highlight and challenge our taken-for-granted assumptions, and to unsettle and disturb our inclination to depend on safe formulae and on comfortable analytic perspectives." A return to parasitism would mean to recruit new audiences and cultivate a form of provocation that strategically establishes *para*-sites for skeptical renewal and engagement.

Against this backdrop, Lynch's suggestion of "blowing up STS" offers an interesting alternative. While broadly in agreement with the need to find new ways of being parasitic, Lynch does not call for a collaborative anthropology of modernity, a shift in analytic resources from "Western" to "non-Western," a recourse to configuration and composting, or the generation of new audiences by way of provocation. Rather, he offers the rather anticlimactic advice to "go back to the laboratory." This might seem disappointing as one could argue that such studies have been conducted galore and that Lynch therefore reveals himself as hopelessly nostalgic. However, there is another and more exciting way of reading this suggestion.

As Lynch makes clear, the point of going back to the laboratory is to "take a hard look at what scientific practice involves, rather than to explain it away." What we might have, then, is parasitism of a rather different kind that cannot be cast neatly into Serres' fable. This kind of parasitism would require us to immerse ourselves in practices that eat away our *own* resources—an approach that might better be understood, I would suggest, as an inversion of the figure of the parasite. That is, the guest becomes a host in that it takes advantage of the field to have its own vocabulary challenged. Instead of mobilizing local practices to control them through an analytic framework, the goal would be to expose one's own assumptions and put them up for grabs. This strategy not only promises to generate new concepts and ideas. It also works more generally against consolidating tendencies in academic fields that come of age, allowing our objects to become parasitic on ourselves.

Alright, then. Back to the laboratory, everyone.

Author Biography

Malte Ziewitz is Assistant Professor and Mills Family Faculty Fellow at the Department of Science & Technology Studies at Cornell University. Previously, he was a postdoctoral fellow at the Department of Media, Culture, and Communication and the Information Law Institute at New York University. He holds a D.Phil. in Management Studies (Oxford), a Master in Public Administration (Harvard), and a First State Exam in Law (Hamburg). Broadly based in STS, ethnography, and public policy, his research aims to understand the changing role of governance and regulation in, of, and through digitally networked environments—the dynamics at work, the values at stake, the design options at hand. His recent work has focused on the practical politics of novel review, rating, and ranking schemes in healthcare and web search. He has also published on the rise of algorithms as a topic and a resource in the social sciences and humanities; the history and performativity of internet governance; the nature and uses of "crowd wisdom" in regulation; and the emergence of shadow cultures in technologies of evaluation. As Principal Investigator, Malte headed the ESRC-funded *How's My Feedback?* project, a collaborative design experiment to rethink and evaluate online review and rating websites.

Author Biography

Professor Michael Lynch has a BA in Rural Sociology from Cornell University and a PhD in the Social Sciences from the University of California, Irvine. He is currently a professor in the Department of Science & Technology Studies at Cornell University. Lynch has written numerous books and articles on discourse, visual representation, and practical action in research laboratories, clinical settings, and legal tribunals. He received the 2016 J.D. Bernal career award from the Society for Social Studies of Science, and also received the 1995 Robert K. Merton Professional award from the Science, Knowledge and Technology Section of the American Sociological Association for his book *Scientific Practice and Ordinary Action*. He also received the 2011 Distinguished Publication Award from the Ethnomethodology/Conversation Analysis Section of the American Sociological Association for his book (co-authored with Simon Cole, Ruth McNally and Kathleen Jordan), *Truth Machine: The Contentious History of DNA Fingerprinting* (2008). He is Co-Director of the Cornell Law and Society Program. He was editor of *Social Studies of Science* from 2002 until 2012, and President of the Society for Social Studies of Science from 2007 until 2009.

References

Collins, H. M. and Robert Evans. 2002. "The third wave of science studies: Studies of expertise and experience." *Social Studies of Science* 32(2):235-296.

Collins, H. M. and S. Yearley. 1992. "Epistemological chicken." In *Science as Practice and Culture* edited by A. Pickering. Chicago: University of Chicago Press, 301-326.

Doing, P. 2008. "Give Me a Laboratory and I will Raise a Discipline: The Past, Present, and Future of Laboratory Studies in STS." In *Handbook of Science & Technology Studies*, 3rd ed. edited by E. Hackett, O. Amsterdamska, M. Lynch, and J. Wajcman, 279-295. MIT Press: Cambridge.

Franklin, S. 2017. "Staying with the Manifesto: An Interview with Donna Haraway." *Theory, Culture & Society* 34(4):49–63.

Garfinkel, H. and Wieder, D. L. 1992. "Two incommensurable, asymmetrically alternate technologies of social analysis." In *Text in context: contributions to ethnomethodology,* edited by G. Watson and R. M. Seiler RM, 175-206. New York, NY: Sage Publications.

Haraway, D. 2016. *Staying with the Trouble: Making Kin in the Chthulucene*. Durham: Duke University Press.

Knorr Cetina, K. and M. Mulkay, eds. 1983. *Science Observed: Perspectives on the Social Study of Science.* London and Beverly Hills: Sage.

Kuhn, T. S. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Latour, B. 2004 "Why Has Critique Run out of Steam? From Matters of Fact to Matters of Concern." *Critical Inquiry* 30(2):225-48.

Latour, B. 2013. *An Inquiry into Modes of Existence: An Anthropology of the Moderns*. Cambridge, Massachusetts: Harvard University Press.

Latour, B. and S. Woolgar. 1979. *Laboratory Life: The Social Construction of Scientific Facts.* London: Sage.

Law, J. and W. Lin. 2015. *Provincializing STS: postcoloniality, symmetry and method*. Bernal Prize Lecture, Denver, CO.

Lezaun, J. 2011. "Bees, beekeepers, and bureaucrats: parasitism and the politics of transgenic life." *Environment and Planning D: Society and Space* 29(4):738–756.

Lynch, M. "Two notions of agreement." Chap. 6 in *Art and Artifact in Laboratory Science*. London: Routledge & Kegan Paul, 1985.

Lynch, M. 2014. "From normative to descriptive and back: Science and Technology Studies and the practice turn." In *Science after the Practice Turn in Philosophy, History, and the Social Studies of Science* edited by L. Soler, S. Zwart, M. Lynch, and V. Israel-Jost, 93-113. London & New York: Routledge.

Lynch, M. & D. Bogen. 1996. *The Spectacle of History*. Durham, NC: Duke University Press.

Lynch, M., S. Cole, R. McNally and K. Jordan. 2008. *Truth Machine the Contentious History of DNA Fingerprinting*. London: University of Chicago Press.

Mulkay, M. 1976. "Norms and ideology of science." *Social Science Information* 15:637-656.

Pickering, A., ed. 1992. *Science as Practice and Culture*. Chicago: University of Chicago Press.

Pollner, M. 1975. "The very coinage of your brain: The anatomy of reality disjunctures." *Philosophy of the Social Sciences* 5:411-430.

Pollner, M. 1991. "Left of ethnomethodology: The rise and decline of radical reflexivity." *American Sociological Review* 56(3):370–380.

Serres, M. 2007. *The Parasite*. Minneapolis: University of Minnesota Press.

Suchman, L. 2012. "Configuration." In *Inventive Methods: The Happening of the Social*, edited by C. Lury and N. Wakeford, 48–60. Abingdon: Routledge.

Woolgar, S. 2004. "What happened to provocation in science and technology studies?" *History and Technology* 20(4):339–349.