STRIFE OF BRIAN

*Science and Reflexive Reason as a Public Project.
An interview with Brian Wynne*

by Marie Antonsen & Rita Elmkvist Nilsen

We met Brian Wynne in late April 2013. The place was Hell, Norway, which is nicer than it sounds, especially if you are attending the first Nordic STS Conference. We had recently established NJSTS, and when we heard that Brian Wynne was giving a keynote lecture at the conference, we took the opportunity to interview a pioneer in the field about the so-called political turn in STS. The topics of Wynne`s work ranges from technology and risk assessment, public risk perceptions, and public understanding of science, focusing on the relations between expert and lay knowledge and policy decision-making. He has promoted STS and its democratizing potential since the very beginning, and has never been known to shy away from the more controversial aspects of public understanding and engagement in science. Neither did he in this interview: It seems despite his strifes, he is still going with a strong programme.

Professor Wynne has addended the interview with some clarifications and references.

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MA: Brian Wynne, you were part of the Edinburgh school, one of the cradles of STS. What are your thoughts on the development of STS from then to now in relation to the political turn we have seen in STS in recent years? And do you agree with the premise that there has been a political, or normative, turn in STS at all?

We need to start with a key distinction here, between STS and sociology of scientific knowledge (SSK). SSK, which has always also been a history, anthropology and philosophy of science project – maybe also a politics of science project – has been a key but quite small part of STS as a whole. STS research as it has developed to its present impressive range and variety actually investigates or even questions the processes of scientific knowledge-production in all those various places and forms in which it is constructed, challenged, articulated and used as 'innocent' knowledge, or as non-innocent public authority. SSK itself was composed of several distinct approaches, like those of Edinburgh (Barnes, Bloor), Bath (Collins), and Paris (Latour, Callon), but all of these built on a critical extension into scientific knowledge-processes of the Mertonian sociology of science, which examined scientific institutions. Edinburgh particularly emphasized its intellectual debts to Kuhn's heresies.

When I came to the Edinburgh school and the Strong Programme in SSK in 1971, straight from my PhD in the electron microscopy labs of Cambridge University's materials science department, there was actually already a political turn in play, alongside distinct but connected philosophical commitments, in the emerging science studies research front. This was a very different kind of 'political turn' from that which is referred to in STS today. The context in those days was very different, and STS was partly born out of a broader political reaction against the military-industrial complex and its evident embedding of science into its own agenda, as with the US use of the chemical pesticide Agent Orange all over the Vietnam people during their global anti-communist crusade of the 1960s and 70s. Over 50% of the research and development budget of countries like the US and Britain at that time was military research. The Vietnam War, the developing cold war, and the military-industrial complex's systematic 'embrace' of knowledge-generation for its own ends was a big issue, and STS was a (critical) part of this. The role of science and technology in reinforcing that kind of military-industrial power, and less prominently the question of how alternatives could be imagined and brought into being, was a defining intellectual issue for STS, and the same for many people who joined science studies, as students, researchers and teachers.

The Edinburgh 'strong programme in SSK' school distanced itself from that kind of overtly political impetus of early STS, and was more inspired by Kuhn's apolitical history and philosophy of science. It went into philosophy, history, and sociology of scientific knowledge in a way that was really quite technical. It was not interested in rousing scientists to become more politically aware and active, as most of the rest of STS – along with critical scientists in the 'social responsibility in science' and 'radical science' movements

– was. As a naive scientist ignorant of all these currents and entering the Edinburgh school in 1971, all this was something completely new for me. The first three publications on my CV are scientific publications, not sociology of science stuff at all, so for me Kuhn, Popper, Polanyi, Fleck and Feyerabend and others in philosophy and history of science were all completely new. The Edinburgh move was actually away from all of those politics and into the technicalities of Mannheim, Kuhn, Fleck, the Frankfurt School, and also engaging cultural anthropology – albeit reinterpreted by the Edinburgh School. I came in very naively, learning all of the Strong Programme ideas and all the resources on which they were drawing, but actually being interested in that political set of issues that its leaders, who were teaching me (like Bloor and Barnes), had set aside. Unlike them I was also trying to work out the connections between the Strong Programme and the politics of science. I was getting involved in the emerging anti-nuclear and environmental movements in the early 1970s. Scientific knowledge was being drawn in as a powerful actor – as expertise – in public authority for controversial political commitments like nuclear power, industrial chemicals, supersonic air-transport, and pharmaceuticals. I was interested to see whether SSK as deployed for academic research science could also say anything interesting about scientific knowledge as constructed in public arenas.

I left Edinburgh and went to Lancaster in 1975, where the new agenda of sociology of scientific knowledge in public arenas (SSKiPA) was developed more clearly. I did journal articles, for example a special issue of *Social Studies of Science* in 1976, on the SSK research which I had done for my Edinburgh MPhil in early-20th century history of quantum theory which stretched back empirically into late 19th century history of science. This remained quite technical history and sociology of scientific knowledge, very directly influenced by my Edinburgh teachers and colleagues, Barnes, Bloor and Shapin, and on academic science, not public science.

However, alongside this more straightforwardly SSK work on academic research science, and encouraged by David Edge, Director of The Science Studies Unit at Edinburgh and defender of the controversial Strong Programme, I was also developing my own SSKiPA agenda with a critical analysis of Technology Assessment as a self-proclaimed 'scientific' policy tool (an article in *Research Policy* 1975 especially), as well as getting involved as 'activist' practitioner-ethnographer of the 1977 Windscale Public Inquiry into the controversial THORP nuclear fuels reprocessing plant at Windscale-Sellafield. This work, which was "like being sucked into a whirlpool" as I described it to a radio interviewer at that time, became my 1982 British Society for History of Science book, *Rationality and Ritual*.[1] In this I was attempting to show how a particular version of scientific-legal rationality became a ritual of broader public authority for political commitments, partly by rendering publics and their concerns passively accepting of them.

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1 Republished with a new updated introduction by Earthscan in 2011.

This was a very different approach to the dominant STS politics of public science which then prevailed, which was the interests-based approach of my good friend and colleague Dorothy Nelkin and her students (eg, *Controversy*, 1979).

My own SSKiPA approach – as it developed into SSK of risk science, public issues and critical public understanding of and engagements with science – went off on a lengthy diversion from mainstream SSK. In the early 1980s this encountered the reflexive challenge of Latour-Callon and Woolgar. It is interesting to recognise in retrospect how there was an implicit perspective on publics and science in the Edinburgh strong programme of SSK (see for example Barnes and Shapin's *Natural Order*, 1979). Thus one sociologically interesting but initially neglected element of academic scientific cultures was their collective scientific concern with authority or 'social control' over publics and ill-defined other audiences. Paul Forman's 1971 article on the Weimar Republic's culture and the shaping of quantum physical explanations (in part a response by physicists to widespread active public feelings), also reflected this analytical interest in publics, and was influential in the Edinburgh school during my time there.

Though he had left Edinburgh when it was published, Shapin exhibited his continuing interest in such public dimensions of science, as a key feature of his seminal work with Simon Schaffer, *Leviathan and the Air-Pump* (1985), on Robert Boyle and his social constructions of witnessing as an instrument of wider authority for his 17th Century science. Although this interest in public dimensions of scientific knowledge was an explicit element of the Edinburgh Strong Programme's analysis of scientific knowledge in academic science arenas, examining processes of construction of scientific knowledge for public arenas and political authority was not a distinct concern. We approached publics at Edinburgh more as dimensions of the imaginaries of and mutually constructing influences on scientists and their knowledge, but it was only slowly becoming conscious to us as we were doing it. This was all a completely different and much more roundabout and oblique way of approaching the questions about science, democracy and publics – and hence politics of science – than was true of what was being developed elsewhere, for example in neo-Marxist STS (eg, Hilary and Steven Rose) and embryonic green-feminist STS (eg, Brian Easlea). It is interesting in retrospect to see that there was embryonic interest in the politics of science, publics and science and democracy developing within the Strong Programme, but it wasn't at all a significant or prominent part. Nor did it embrace the reflexive concern to see human subjects as 'in the making', and to include non-human actors in politics, which characterised the Paris school of Latour, Callon and colleagues.

MA: How would you compare that to now?
From the late 1970s I guess mainstream STS went in the direction of lab studies, which became the dominant theme through the 80s and 90s, and some very good work was done in that vein using anthropological resources and methods in various parts of the world, for example by Knorr-Cetina on epistemic cultures in particle physics and molecular biology. But this was itself very different from Latour's interests in lab science as obligatory passage-point, and centre of calculation for wider world building, as expressed in his 1983 title *Give me a laboratory and I will raise the world*. With these different currents, that was the mainstream of STS-SSK, along with the 1990s Science Wars inflicted on STS by militant defenders of scientism against STS, atrociously misconstrued by them as anti-realism, and as championing the stupid idea that "we can believe what we want to believe."

Meanwhile, however, I had been somewhere else with my SSK training, in a unique cold-war East-West international scientific institute, IIASA, the International Institute for Applied Systems Analysis in Austria, trying to lead an East-West research group on risk, and also investigating IIASA's attempts to introduce large-scale modelling of environmental and energy systems. There I was working with – and at the same time studying – environmental and energy modelers, still trying to conduct SSKiPA, but on a completely deviant track from mainstream STS, largely on my own as an ethnographer amongst the scientific risk and modelling tribes and their ambitions to influence international policies.

Myself and an ex-modeller from the IIASA energy group had looked into some real problems with those energy models. IIASA had just funded, conducted and published a huge Two Volume study called *Energy in a Finite World*, claimed to be the results of simulation modelling of global energy systems. The study, published from IIASA and circulated globally, was very influential in the US, and basically the storyline was: "We need nuclear power, we need nuclear power, we need nuclear power". My ex-energy group friend, Bill Keepin, who came to express his insider concern, said, "There's a real problem in what they're doing, and they're not acknowledging that, they're not being honest about what's going on." We painstakingly examined the models, their architecture and their operations, inputs, and outputs. On this we wrote an internal working paper and gave a seminar for the institute, with the energy group there. We laid it on the line, and said "well, correct our interpretation if you can, but you've been overselling these models". These huge simulation models were supposed to represent something about the complexity of the real world, in terms of energy demand and production, simulating the dynamics of the system and then run forward into the future.

What Bill had worked out was that none of this model technical complexity, hundreds of parameters, and thousands of variables, dynamically representing global energy demand, consumption, distribution and production, did anything at all. All of the outputs could be calculated using a pocket calculator. From the assumptions and the input variables that they had chosen, the model did precisely nothing; the 'feasibility space', a key internal technical 'operational zone' of the model, was zero. In other words, its

outputs were hard-wired to its inputs, which were not validated by anything. Yet, the models were being represented as if their validated mathematical-calculative architecture was producing those outputs through this complex scientific intelligence – outputs which were basically saying, "we need to build a nuclear power station every two days somewhere in the world, for the next fifty years," if we want to solve the global energy crisis. And this of course fitted in very well with many powerful international interests and commitments, including the funders of the IIASA energy programme! We simply documented this rather sharply deflating analysis of the energy programme's most influential 'scientific' public knowledge-product, and pointed this out. I was still doing SSKiPA! SSK was only there in my toolkit and motivation, otherwise I was on my own adventures, with new colleagues from very different disciplinary backgrounds and interests, and new ethnographic sites.

We published this SSKiPA work eventually in in a special issue on the IIASA energy models, in *Policy Sciences* (Wynne, 1984), and a much shorter version in *Nature* (Keepin and Wynne, 1984). It was very highly publicised, and quite influential. This was in the time before the Velvet Revolution and the breaking of the Iron Curtain – and this was the only East-West scientific institute. Reagan was saying, "Evil empire, get rid of them," and wanted to close IIASA down. Harvey Brooks, Harvard physicist and big guru of American science policy, went to the White House to try and persuade Reagan's advisors that IIASA was OK. And he said to me: "The only thing I've got to try and persuade these guys about IIASA's worth is *Energy in a Finite World*. And you and this unknown guy from inside the energy group have blown this apart!"

Harvey, to his credit, said to me: "Listen. I'm going to pass your paper around to my scientific colleagues in the field, I'm going to ask them to review it carefully – peer review! – and if I find you've done anything wrong in that paper, you are in big trouble. But if you haven't, I'll support you." And to be fair to him, he came back to us a while later and said, "I can't find anything wrong with it. I'm going to support you." To the extent that it ever became a big public thing, and it was a huge problem for IIASA that we did not wish to inflict, he did. He was true to his word. Considering that his own reputation was on the line with that – he'd been a big supporter of the energy in a finite world study – that was a great example of integrity from someone put in a very difficult position. This was sociology of scientific knowledge (in public arenas), but it was also straightforward science – correcting collective mistakes. They just happened to be mistaken scientific knowledge-products on which huge political and social normative commitments had been built, and then represented as if objectively determined by 'science'. This is (one kind of) scientism, and it remains one of SSKiPA's important tasks, to expose these normative commitments – deliberate or inadvertent, it does not matter – for what they are. This is not the same as using our own very modest scientific authority to justify one normative stance over any other(s). It is just modestly

to expose public debate and political decision making to normative questions which have been concealed for society as if they were only scientific. This is a necessary normative role for STS; but it is not taking sides or pretending to have any authority in the substantive normative conflicts themselves.

Mainstream STS was doing lab-studies while I was inadvertently doing politics of science at IIASA. I'd actually gone to lead the risk group at IIASA, and tried to bring STS-SSK into that work (Wynne, 1987). Risk for me was becoming a big STS-SSK issue, where science and society meet, in a very strongly political environment – assessment and decision-making about new technology etc. – but one where there are really interesting and important STS-SSK questions. That was always my interest, scientific knowledge as public authority, but that wasn't always as central for Barry Barnes, David Bloor and Steve Shapin. They approached it in a more roundabout way, through the imaginaries of publics that were in scientific knowledge. It's a rather different kind of approach, although I think the two are coming together a bit more now. I think STS through the 80s and 90s was very underdeveloped in terms of its politics, and the people who tried to do it, like Daniel Kleinman for example, haven't been given the prominence I think they deserve.

So nearly everyone went off instead with Latour, ANT and its enthusiasms, and thought that was politics of science; but as feminists like Vicky Singleton and Susan Leigh Star and others were pointing out, and as I think Latour now recognizes, ANT has its own political problems and lacunae. STS entered the last decade really underdeveloped on the front of science and politics. Various leading STS scholars have tried to do it. I remember workshops in the 1990s, in the Cornell STS Department under Sheila Jasanoff's leadership, with invited political scientists like Yaron Ezrahi, who did publish in STS journals, and wrote a great book, *The Descent of Icarus* (1990), but not one which explored the deeper reflexive issues which are now familiar in post-structuralist social sciences and humanities including STS. Political science itself hasn't actually done what Latour proposed, which was to open the black boxes and look at the science and technology, for its own internal sociological, political and normative issues. This is still mainstream STS-SSK fare, whether or not one takes it into the rarefied and potentially elitist atmospheres of Latourian philosophy and politics of non-human actors, or whether one wishes to keep the normative focus on human democratic mutual responsibilities. Apart from Ezrahi, unfortunately most of political science was off on its rational choice tracks, while STS – informed by cultural anthropology and continental philosophy – emphasized relational ontologies and questions. STS scholars like Sheila Jasanoff did try to develop towards politics, but politics was not receptive, in disciplinary terms, to actually engage and develop something that would have really helped STS to improve and strengthen its own political thinking. The 'political turn' of STS is only of relatively recent ilk; and it has not yet integrated co-productionist STS thinking with democratic theory and reflexive modernity perspectives. The prevailing STS

work on expertise would need a lot of further development to handle the crucially important cultural perspectives of scholars like Judith Butler and Gayatri Spivak.[2]

My own work on publics and science has been informed more by that broader reflexive perspective in STS-SSK, and in humanities at large. The Edinburgh school of SSK did always have an interest in how scientific work imagined its 'publics', even if this was not a central intellectual pillar of its approach. Barnes and Shapin published several historical sociological papers of this kind, as well as *Natural Order*, dealing with these relations, in a way perfectly consistent with co-productionism, as Jasanoff (2004) later described this. Recently published STS work supposedly dealing with science and politics avoids these issues.[3] In dealing with publics, we cannot push SSK questions about science out of the picture; the two are or should be in the same frame. Most of the mainstream social science on publics has not done that, because it has not wanted to problematize science. Ian Welsh and I deal with this in a recent paper in *Science as Culture* (Wynne and Welsh 2013), and I write about it in a forthcoming article in P*ublic Understanding of Science* (January 2014). It's that classic thing that Latour describes in 1993, where nature is for the natural scientists alone, and humans and society for the social scientists alone, and the latter do not go and look at what natural scientists are looking at, how they are doing this, and what they or their paymasters are trying to do with that work on nature – but also, on society. It is that categorical nature-culture distinction that Latour quite rightly criticized, and that all the post-structuralist and post-positivist social sciences and humanities and STS since the 80s have been gradually overcoming and superseding. In the academic world there is, of course, a large amount of work that takes that approach now, but this has had too little influence on the world of real politics and real policy. When people celebrate me as an STS scholar that has gained a lot of policy influence, I reply by pointing out that scholars like me have very little influence on the real worlds of science, power and politics.

REN: In two recent articles co-written with Fern Wickson you criticize both the notion of risk as defined by EFSA and the very idea of science, inherently communicated in the latest proposal by the European Commission. Would you care to elaborate on that?

The analytical starting point here really is co-production. Nature and culture are interwoven and you cannot identify clear boundaries between those. EFSA, the European Food Safety Authority, is the designated scientific authority by the European Commission on things like food safety, but food safety in this case actually equals *environmental* risk assessment, like new crops and GM technology generally. Thus, food safety is coming to encompass environmental safety, interestingly enough. This is already an extrapolation that is problematic because the disciplinary scientific inputs to analysis and

advice from EFSA are inadequate for such broader ecological and agricultural processes that influence environmental risks. Usually laboratory molecular sciences are those dealing with food safety. Environmental risk raises a whole lot of questions about the relationship between the laboratory and realities of different kinds out there in the field – in real agricultural and ecological circumstances – which vary far more than controlled lab testing conditions.

The authorities have not really recognized the importance of this. The co-production point leads you immediately to recognize something that I have tried to emphasize to friends in NGOs that have been hammering EFSA on this issue. They should not only be looking at EFSA, because EFSA is operating as the science producer for policy. It is operating as science-producer to terms of reference that are dictated to it by the European Commission as policy client in the form of DG SANCO, the Consumer Health and Protection Directorate General of the Commission. When you are looking at EFSA and the way in which it defines and frames risk in order to do risk assessment, then you have to look at the commission as well and see where those terms of reference for such 'science' come from. What questions can EFSA ask existing scientific research knowledge, or to its commercial applicants for approval of GMOs? These are influenced by policy, and indeed by industry, as much as by science itself. This is true not just for EFSA but also for every scientific advisory body around the world. They are operated, as 'independent science', to terms of reference which are laid down by policy; and those terms of reference are often the key in terms of the what is produced as supposedly impartial scientific knowledge as claimed or attempted public authority for decisions on GMOs.

Fern and I wrote a paper in *EMBO reports* in January 2012 (Wickson and Wynne, 2012) on the EU Commission's proposal for new GMO regulation for Europe. The standard formula has been that the EU Commission is the competent authority of the whole of the European Union on issues like GM crops and foods. It deals not just with GM crop cultivation proposals in any member state but also with GM food imports from the US. The US has been hammering to get free market access for its GM exports in Europe. Thus, the standard practice is that EU Commission asks EFSA to do risk assessments, EFSA does the risk assessment, and member states can then comment on it. If EFSA says there is no evidence of any harm, then, in that case, the commission approves it and the permission to cultivate or import becomes law. That is how EFSA operates in practice, as scientific authority for the whole of Europe.

A human body in Portugal is probably pretty similar – within the range of human bodies' differences such gender, age, and all the usual kinds of differences – to a body in Finland, Poland, Italy, Greece, or Portugal, all-around the whole of the 27 member states of the EU. So a human health risk assessment for a given GM food or crop may be thought to be valid across the whole European population, if it is valid for any member-state's population. With environmental risk assessment, however, there is a different set of

questions, about variations across European environments, both between and within member-states. The environmental conditions in Finland are different from Mediterranean Italy, Spain, or Greece, or to other parts of Europe. The environmental conditions' differences may or may not be significant in terms of environmental risks and harms, yet this is not just about ecological conditions, but also agricultural. For instance, Austria is particularly strong on organic agriculture. If you have got a strongly organic agriculture and then release GMOs, there will be cross-contamination between organic crops and GM ones through environmental processes such as wind, pollen, birds, etc. Then you may have damage to a significant part of that economy, to organic crops and certification of them. Environmental conditions are also agricultural conditions. How crops are managed and how food is processed from those crops can affect environmental risks.

EFSA has not been good at actually recognizing what might be scientifically objective differences in environmental risk for the same GM crop in different parts of the European Union. It has operated – and defined this as science – on the premise that the EU is a singular environment, for the environmental risk assessment of GM crops. However, there are powerful economic factors lying behind this questionable (but rigidly defended by EFSA) scientific stance, which is that they want to have a one-stop shop for the regulatory appraisal of any kind of trade and imports, including GM foods and crops. They do not want any prospective import to have to go through 27 different member-state regulatory decision-processes in order to get that product into Europe. For the European environment, this is not realistic. Yet it is a key part of European science, for policy. Indeed the EC itself has acknowledged this point in a different but related context, effectively contradicting its own single-market, single-environment assertion. This was exposed in 2006 by Greenpeace and Friends of the Earth Europe, in the EC's confidential case to the 2005 WTO disputes panel, defending itself against the US complaint to the WTO, on alleged delays in GM imports to the EU.[4] EFSA has been given the job of conducting environmental risk assessment and human risk assessment, partly because Europe wants to be a political unit, and here this political-economic aim – The EU Single Market, and behind this political union – is being conducted through the framing of the science, since the EU does not have political legitimation through adequate parliamentary accountability processes. The European Parliament does not have that role, and the EU has always had that kind of built-in democratic deficit.

Interestingly, this is clear when we review the origins of the European Union, in the European Coal and Steel Community in the early 1950s. Political scientists like Jan-Werner Müller have commented on this. At that time, Europe had been through two devastating world wars within a couple of decades. The Coal and Steel Community was basically a technical-practical way of trying to achieve what were perfectly honorable human objectives, which was to make more war between European states impossible. One of the ways to do that during those days was to communalise the main resources of warfare. In order to build tanks you need coal to make steel. Communalise those and you basically dissolve the capacity of any state to make war on its neighbors. That was the main start by which those honorable political objectives came into being. Then followed the European Common Market, and it continued being expanded in 1987 with the EU Single Market, and so on. This is something we wrote about in the *Taking European Knowledge Society Seriously* report (Felt and Wynne, 2007): There has always been that kind of assumption of unity which could be exercised through scientific authority, as an attractive alternative to the kind of numbingly pedantic, painstaking and time-consuming effort of doing political negotiation and working out institutional accommodations when you've got different institutions and cultural histories in different European states. Diverting political and normative authority to the singular voice of Nature as provided by science, seems a seductive short-circuit to the risky business of straightforward politics.

Going back to the GM case, this is a sharply political thing, because countries like Austria, Italy, Greece and many other regions of the EU have been resisting the conventional authority, which is that EFSA produces risk assessments on the terms laid down for it by the Commission, and this 'scientific advice' is translated into decision by the EC as policy competent authority. EFSA as 'independent' scientific authority can only ask those questions that it is allowed to, and not other questions that an independent science might ask on rational grounds. If on these restricted terms EFSA doesn't find a problem of harm, then the commission approves. Then, member states cannot refuse, but are obliged by European law to accept that GM crop in that country. Yet despite this, member states are repeatedly refusing such formal approvals, and there are also over 200 municipal, local and regional GM-free zones declared on the basis of public resistance. There is relentless and intense pressure, from the US mainly, big corporations coming through the commission meeting with this bottom-up resistance in EU member-states like Austria, Greece, Italy, and sometimes also France and Germany. Basically there is enough resistance that the EU Council of Ministers told the Commission, in late 2009: "Provide us with legislation which actually allows countries to have a free-for-all on GM cultivation." In other words, Austria can decide democratically for itself if it doesn't want to cultivate GM crops, and that will be law, and it won't then be taken to the European Court as having acted against EU law. The EC produced such a draft legislation originally in July 2011; it was examined and then amended in important ways by the European Parliament in July 2012, and it has since then been in purdah, under confidential negotiation between the EC, the EP, and the Council of Ministers representing EU member-states.

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4 The report is available online; http://www.greenpeace.de/fileadmin/gpd/
user_upload/themen/gentechnik/greenpeace_hidden_uncertainties.pdf

The key thing that we identified and criticized in the Commission's proposal was that the only grounds on which a member state was allowed legally to refuse to cultivate – this is after an EFSA approval, on environmental and health safety grounds – would be on non-scientific grounds only. In other words, the scientific point that there are objective environmental conditions that differ between member states around Europe, and which could be extra factors of environmental risk to those considered by EFSA, is not deemed legitimate science. This is precisely what GM refusenik member-states like Austria and Hungary, and for some GM crops Germany and France also, had been stating as scientific grounds for refusals, in face of EFSA risk assessments. Instead, they were to be allowed only non-scientific grounds for a refusal. Fern Wickson and I criticized this EC stance as generated by EFSA.

Corinne Lepage, the vice-chair of the Environment committee of the European Parliament, took our advice on this and said that they need to produce some amendments to this Commission legislation that actually allow member states to refuse on scientific grounds, and have good objective scientific grounds to decline a specific GMO on grounds which are scientific, but different from those on which EFSA has approved it. That's what we critiqued about the Commission proposal: A form of scientization of Europe by trying to advance Europe as a political-economic unity, but doing this as if it this politics – legitimate and sound in principle – were scientific necessity, revealed by EFSA's scientific advisory panel. We are not objecting to the principle of European political unity – indeed for me personally, quite the opposite – but we don't want this to be a technocratic, corporate-dominated political union with democratic deficits all through it. That is no basis for building a robustly democratic Europe, which remains an ongoing political project.

The 60-year history of that project is not finished yet. The European Parliament amended the Commission's proposed legislation, which is a standard constitutional procedure. The Parliament has stronger powers now than it used to have. A big majority was in favor of this Lepage-led Environment Committee amendment, and so the EC was given a bloody nose on that one; it is now back in the murky smoke-filled room politics, utterly unaccountable lobbying and pressure-politics. Member states, the commission itself and parliament representatives are busy haggling in private over exactly what will come out. The specific outcome on GMOs may be less important than whether the EU can resist the false temptation – in face of admittedly daunting political pressures – to reduce its politics to scientism of this anti-democratic kind. Significantly, Jürgen Habermas has made a similar kind of analysis of European defence of the Euro, in which unaccountably-decided policies of economic restriction to save the common currency, equivalent to those of the Single Market norm, are being pushed through member-state Parliaments, and where the ensuing social impositions are also required in the name of economic necessity, not democratically negotiated political choice.

MA: You were talking about different cultures, and we have a question for you about that. You have also done a lot of work on the roles and performances of the Advisory Committees and similar bodies in the UK. What are your thoughts on the roles of national, institutional context and cultures in shaping expert advice to policy? What role would these play in the composition and practices of such bodies?

There is a lot of good political theory in perspectives from both beneath and above the nation-state. In a way, the reason why the EU is an interesting case is because it is a kind of metastate that is still very much in formation, and is likely to be in that state for a long time, maybe forever. That's really a post-structuralist point: The nation state never was a given in that sense. Regarding your question about the comparative issues, various people studied it very productively in the 70s, not only in Jasanoff's and colleagues' studies about comparative regulatory cultures and policy outcomes, but many others as well. The standard research finding here was that there are different countries making decisions about for example approval of chemical pesticides for commercial use in that country. They have the same science available to them, because the science is global. Each of them evaluates the same scientific research and yet they reach different decisions. So what is going on? There were lots of comparisons in the 70s and 80s between US and Germany, US and Britain, and sometimes between Europe or Scandinavia; research of that kind on a variety of different technologies, from car-safety to chemical pesticides to contraceptives and pharmaceuticals, and radioactive emissions standards.

Usually, the US committees would decide – or just assume – what is relevant as a standard scenario for risk assessment, and the UK committee would decide that something different is relevant, so it is actually a different profile of salience of the many combined technical factors involved. As always there are multiple factors in real risk situations, so then the question becomes which of these are relevant for addressing public interest policy outcomes. While scientists as I was are trained in ways that lead them to believe otherwise, that is not an issue that scientific committees should decide alone. It is a democratic issue, one that should be informed by scientific knowledge but not framed and determined by it. Meanings and concerns should arise within democratic settings articulated through democratic political processes. They should be informed by science, of course, but this is not the same as allowing science to define those public concerns and meanings. There is no reason why something that is democratic and political shouldn't be informed by science. The idea of some kind of either/or, science or politics, is a stupid response, induced as far as I can understand by fear and anxiety on the part of those, usually scientists in positions of authority, whose privilege is challenged. They feel threatened by such democratic openings up, of expert processes which have been previously too closed, and too unaccountable. I would not and cannot start from there.

Going back to your comparative question, very often a finding of the comparative research is that different countries and cultures are framing the problem differently. The US National Research Council, in 1983 in the famous so-called red book which first articulated the relationship between risk management and risk communication, recognized that there is an extra stage that one can call risk assessment policy where scientists can formulate the questions about risks, but they can't necessarily answer them. So there are two kinds of policy inputs. One is: "What's the main problem we're defining, for science to answer, if it can?" That's a political issue ultimately. The second is what kind of inference bridges are used when all the relevant scientific evidence available is not complete enough, or not directly representative of the real risk-situations in which people or environmental entities which we value are placed. Then, there may be inference-questions that ideally scientists could answer from evidence alone, but where policy has to dictate which choices they make, because they themselves cannot answer scientifically, at least with current scientific research-understanding. A current example is where policy may determine for scientific risk assessors, what factors to use in translating observed harm in lab rats under test, to human beings. Often a policy choice has to be made here, even over an ostensibly scientific question, because scientists can't answer it, and it's ambiguous as to whose responsibility it is to answer. This is Risk Assessment Policy. A committee of EU experts has recently recognized it, following the Food and Agriculture Organisation's 2005 recognition of it, and of its importance. Yet it was first recognized as a public policy matter for Risk Assessment, by the US National Research Council, as long ago as 1983! It still has not been translated into proper practice, in regulatory processes anywhere in the world.

For GMOs in Europe, both in the framing questions and in the inference bridges, EFSA is often making those kinds of normative and policy-weighted judgement as if they are science, because it is expected to do this by DG SANCO – and these terms are not accountably open to debate, because DG SANCO doesn't want to take public responsibility for them. If I were a scientific chair of such an EFSA expert panel, I would make it clear where such normative choices exist – insofar as these are identified as such – inside what is currently defined as the science. The policy bodies, in this case DG-SANCO, would then have to take political responsibility for such choices, and justify these in public. Part of the EU's democratic deficit would then be dissolved, even if it has many more such awkward crevasses to reveal, and handle. A central example is what comparators are chosen for defining the harms from GM crops. It is taken by EFSA to be normal intensive industrial agriculture, which is not a sustainable form, but is commonly in place. Against this standard, GM crops can be risk-assessed as no more harmful than normal equivalent crops – even if against a different available standard, such as agroecological cultivation, they would be scientifically deemed to be unnecessarily damaging.

Because of the democratic deficit in Europe, nearly every science-intensive policy system is hiding behind the science and pushing politics into the science. I happen to believe that they are being more naïve than deceitful here, but the responsibility for resolving that kind of problem lies with both science and policy actors. It is not one or the other, neither the scientific advisory committee nor the policy body who is setting the terms of reference, and the implicit mutual accommodations. It's both together. I just want to refer to a good concise paper on that by my close colleague Andy Stirling in *Nature* in 2011, called "Keep it complex" (Stirling 2010). He is saying the same: Scientists should refuse to give black and white answers to policy where black and white answers don't exist. This does not mean that they have nothing useful to say; but they should render their advice to policy-makers conditional, and if necessary also plural, and then the responsibility lies on the policy-makers to justify the policy commitments which have been going in to the available science and coming out as if it is only science and nature speaking. Then the policy-makers will be forced to actually do better politics. Of course, any policy official is going to say: "You cannot expect me to do that!" Also, in the relationships between Parliament, the cabinet, the executive and the administration, that's an institutional redesign issue, and it seems more radical and difficult because it has been allowed to accumulate over decades without anybody really noticing what we were doing. So how can you expect to solve that historically accumulated problem immediately? Of course we can't, it is going to be gradual, and an ugly and difficult process. It needs good social science and humanities, as well as good, principled and independent science.

MA: I wanted to ask you about bioethics, in terms of asking the questions and answering them, because bioethics is in many ways difficult when it comes to evaluation, when it comes to humans. Any thoughts on the role of bioethics or bioethics boards?
You might not be able to amend this into a polite version – but, yes, there has been a kind of tension. STS has attempted unapologetically to open technoscientific black-boxes where it can, look at the upstream processes of science and technology and look for the social, the political, the normative, the ethical going on silently there. Bioethics by and large, doesn't want to do that, and in fact has very actively resisted doing that. I see much of the political-intellectual basis of bioethics to be too individualistic, and choice-dominated, rather than institutional and responsibility- and accountability-dominated. Bioethics needs to be rendered more challenging of power and of processes of reflexivity-suppression. At its best, anthropology and culturally informed STS-SSK can do this. I think there is a politics here that I find dishonest; and this is not only a disciplinary paradigm intellectual thing, it is an ethical thing. It is as if some disciplinary cultures in social sciences and humanities have been too afraid to ask challenging questions. We have the responsibility to raise those questions, and we as specialists can, assuming access, go and look inside the science and identify questions that aren't being asked which should be. That's

my kind of understanding about the normative flow. It is to identify the questions and not pretend to be able to answer them yourself because that's a different and more collective responsibility. It is a democratic issue, ultimately, so why should any academic discipline be pretending to do things that democracy should be collectively negotiating?

Bioethics, by and large, has declined or refused to enter into the terrain that science and technology studies have regularly entered into, trying to actually problematize – though not to pretend to answer, that would be for a democratic society – the normative questions which scientific practices and R&D cultures are answering by default, just by dint of promises, relations of accommodation with power, funding and commitment. I was asked by philosophers from Cambridge University a few years ago to give a talk on what STS can say to bioethics. I mentioned it to a friend from Lancaster and said that I was ruminating on what to say; and she said, "oh, you mean besides: *fuck off*!" I did not go and say that to the philosophers!

MA: If money, time, space and institutional requirements weren't an object, what would be your dream research project?
Oh, there would be so many dreams! I just have an instinct about diversity. So I do not have a problem about STS being in different schools, with different kind of emphasis, different chosen topics of interest, methods, and so on. That is a good thing for me. As long as we can keep talking to and learning from each other in conferences like this, and in networks like the Nordic STS and international networks. To me diversity is an important quality. How much diversity of aims or purposes of our field we should include, I am not sure; but even here, if we define such an aim or intellectual-social role, share it, reconsider it, and justify it, that's ok. I do not want a program imposed, but I would also emphasize the importance of what we should not forget when studying publics, politics, and participation in science. We also need to be problematizing what the science in play is in various forms, as public authority or attempted public authority, when publics encounter what is called 'science', but is usually a public encounter with institutional forms of 'science' embodying other unstated agendas, interests, presumptions, and 'spin'.

In all of these ways questions about the involved technosciences need to be kept alive, explicit and addressed, when we are working with publics or stakeholders in relation with science. It is wholly wrong only to look at and problematize the publics here. Downstream issues with risk and risk assessment need to be developed and extended into upstream questions – which typical publics normally themselves focus on – about what other different innovation trajectories are available but are not being tested or developed by appropriate alternative R&D commitments, perhaps with new and different stakeholders as partners. If we move in the direction of publics, that's fine, there is a lot of good

work being done, and more to do, but it's going to be difficult to maintain those essential relationships with the critical questions of technoscience, innovation and power. That's challenging of course, because it means strains in different directions, and I guess this just leads us to the dream project. For me, the elephant in the room here, the big kinds of hidden un-question that should be *relentless* questions, are basically about power. We can't go and do ethnographic research on the powerful because they would tell us (and *have* told us!) to fuck off, and maybe even put some pressure on us if we ever do anything that really threatens their authority.

So for me the dream project would actually be research of that kind, in the places where we haven't been able to go and ask questions yet. Studying academic kinds of science, that's simple: It is sustained hard work, but nevertheless the salient access is the problem, and with most – but not all – of the scientific labs, if you make the right approaches you'll be given access. If you do your homework and ask questions that they can recognize, you can then get informed, systematically. Most scientists I have come across are ready to talk, are ready to give you access as long as you are not practically getting in their way. The big issue is access to those big commercial and government military laboratories, field-stations and centres, because that is where the world is being shaped. There has been very little STS in such fields of technoscience, yet this is where the real action is. To spend say five years with such access to Monsanto's or Syngenta's diverse R&D labs and field-stations, and the strategic management meetings, asking questions about how the technoscience of GM crops and synthetic biology is being shaped and conducted as determined world-making of a particular political economic kind, and how non-scientific factors are being woven into the dominant technoscientific innovation trajectories, into a global narrative of technoscientific determinism and necessity, not choice for humankind, would be my dream project.

There is another interesting philosophical as well as an empirical STS question which follows from this. We are forgetting again to try to understand how we do forgetting, and knowledging. I found this a key issue in my research, not yet published, on how radioecology scientists in the UK came to make a major mistake over the behaviour of radiocaesium in the upland mountain soils of the English Lake District, after the 1986 Chernobyl accident and fall-out. How would we do the research that would encourage the collective forgetting of nasty things like genetic weapons, or nuclear weapons technology – as a world-changing innovation? What corresponding or preconditional kinds of institutional or cultural innovation, or maybe just plain collective work, would also be needed? I think that that is a very good ending. That is my dream. If you could come up with an answer for that, then you would have done the dream project. It might even have been an STS-SSK project – though it would have needed something more, too!

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