

The Joys of Truth: Science Policy in the Public Interest

A 'light' Festschrift in
Honour of Erik Millstone

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Edited by Professor Ben Martin

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Contents

- 3** **Foreword**
Erik Millstone

- 5** **Erik Paul Millstone – Academic Career**

- 7** **Alternative narratives and alternative facts:
reflecting on science-policy predicaments through environmental
and health lenses in West Africa and the UK**
Melissa Leach

- 11** **Realist constructivist contribution to environmental safety policy
for genetically modified crops and foods**
Claire Marris

- 14** **Pastoralism in Africa: some (very brief) realist-constructivist
reflections**
Ian Scoones

- 16** **Millstone Grit: putting meaning, courage – and fun – into ‘*realist
constructivism*’**
Andy Stirling

- 20** **Some brief reflections on the past and future of Erik’s contribu-
tion to policy relevant sociology of science**
Patrick Van Zwanenberg

- 22** **Speaking Truth to Power: a philosopher brings Truth, and Science,
to Earth**
Brian Wynne

Foreword

By Erik Millstone

When I came to Sussex in 1973, I was ignorant of, and naïve about, almost all aspects of the sociology of science. I was, however, rapidly persuaded by overwhelming evidence that the historical, institutional and social contexts in which people, including scientists, operate have an influence on which issues concern them, which questions they ask, which beliefs they adopt and which knowledge claims they make. The fact that the substance and content of beliefs and knowledge claims, including those of scientists, are contingent on the context in which they are adopted and articulated seemed undeniable, but the relevance of that to issues of science and technology policy was not immediately clear to me.

That lack of clarity was, in part, a consequence of the fact that several influential sociologists of science in the 1970s were arguing that, when sociologists of scientific knowledge endeavor to explain why particular knowledge claims were articulated in specific contexts, it was essential to be entirely agnostic about the truth or falsity of the claims whose articulation was to be explained (e.g. Bloor, 1973; Collins, 1981). My prior training as an undergraduate in Physics and as a postgraduate student in Philosophy meant that my epistemological perspective was incompatible with the radical agnosticism (with respect to truth and falsity) advocated by such scholars. When I encountered the issue of food safety policy (in the New York Review of Books in March 1974) and became increasingly preoccupied with the science and politics of food safety, my discomfort with agnosticism in respect of the truth or falsity of claims about food and chemical safety intensified. Food safety policy debates were polarized between, on the one hand, official and industrial assertions of the unproblematic safety of all permitted products and processes and, on the other, claims from critics either that safety had not been established or that evidence of risks was available. It was obvious that the science of regulatory toxicology was replete with uncertainties, while any suggestion that all claims concerning the safety of industrial products and processes were equally uncertain seemed epistemologically untenable and singularly unhelpful when seeking to explain or critique prevailing policy regimes, or to identify ways in which policy regimes should change.

In collaboration with several doctoral students, I developed a policy-relevant approach to the sociology of regulatory science. Paddy van Zwanenberg and I called our approach a '*realist constructivist*' sociology of scientific knowledge (van Zwanenberg & Millstone, 2000). We contrasted it firstly with the realist anti-constructivist approaches characteristic of technocratic narratives, which attempt to portray regulatory science as if it were entirely independent of all policy context or considerations, and which is supposedly on everyone's side by being on no-one's side. Secondly, we differentiated our perspective from radical social constructivist approaches that were resolutely anti-realist, for which certainty and uncertainty were always and only in the eyes of the beholders.

The methodology appropriate to our realist constructivist perspective focuses on identifying the location of key points in the processes of social construction at which vital decisions were taken, which influenced the beliefs and knowledge claims of scientists but which could never have been settled solely by reference to scientific considerations. It assumes that, at those key decision points, alternative options and pathways were available. It seeks to identify and characterize those alternatives, and to investigate how and why particular decisions were taken, as well as the implications of those decisions for environmental or public health, for example.

The approach gained purchase on policy debates because it assumes that understanding the ways in which competing representations of key aspects of nature or societies have been constructed enables us to make well-informed and evidence-based judgements about the relative reliability of competing knowledge claims. It recognizes that not all constructions are equally well-constructed. Some are remarkably robust constructions whilst others are flimsy and unreliable. The method has frequently been applied by taking policy-relevant claims that purport to be purely scientific, showing that they were socially constructed hybrids of both scientific and normative considerations, and then disentangling those contrasting sets of considerations. Jasanoff made a helpful contribution here by drawing attention to the fact that not only can such claims can be *de-constructed*, but they can also be purposefully

re-constructed with explicit acknowledgement of relevant value judgements and their contributions to science-based policy-making.

SPRU researchers, both staff and students, have applied realist-constructivist approaches to a wide range of topics. These have included the putative safety and risks from the commercial use of such products as food additives, insecticides, fungicides, herbicides and contaminants. Other applications have examined 'recommended daily allowances' for essential nutrients, such as vitamin C, and debates about the safety of food-packaging materials such as plastics. Similar approaches have been applied to the debate about a cluster of cancer cases in the environment of the Sellafield, and the science and politics of BSE and GM foods.

SPRU most certainly has not had a monopoly on the use of realist constructivist approaches, however. Melissa Leach and Ian Scoones in IDS have applied similar approaches to debates about land-use and wildlife protection policies in sub-Saharan Africa, and the relationship between the lay expertise of African pastoralists and the scientific perspectives of urban experts, and their relationships with local power structures. Brian Wynne has contributed by applying a similar approach to numerous issues ranging from radioactive contamination of Cumbrian farm land to the testing and regulation of GM crops and foods. A comprehensive list of other contributors would be lengthy.

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Erik Paul Millstone

Academic Career

Erik Millstone is a Professor of Science Policy. Erik gained a first degree in Physics, followed by three postgraduate degrees in Philosophy. He was then appointed as a lecturer at the University of Sussex in History and Social Studies of Science. He joined SPRU in 1987, becoming a senior lecturer in 1995, a reader in 2001 and a professor in 2005. Since 1974 he has been researching into the causes and consequences of scientific and technological change in the food and agricultural sectors. He is one of the UK's leading independent scholars of food safety policy. Most of his research has focussed on the UK and the EU, but in recent years his interests have extended to cover food and agricultural policies in developing countries.

Qualifications

1983 PhD in Philosophy, University of London
1971 MPhil in Philosophy, University of London
1969 MA in Philosophy, University of Kent at Canterbury
1968 BSc (Hons) in Physics, University of Kent at Canterbury

Selected Publications

Articles

Kashani, E. and Millstone, E., 2016. Experts against precaution: analysing the process of biosafety regulation in Iran. *New Genetics and Society*, 35, 29-48.

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Alternative narratives and alternative facts: Reflecting on science-policy predicaments through environmental and health lenses in West Africa and the UK

Melissa Leach, Institute of Development Studies

Scarily, we seem to be in an era where politics and policy are dominated by populism and soundbites; where expertise and experts are discredited; where argument and justification have little place; and where facts and ‘alternative facts’ can seemingly be marshalled as besuits the interests of the moment – or to discredit others’ causes. Many factors surely underlie this present predicament of social and political ordering. And it’s a dangerous world, allowing in climate change denial, pathological authoritarianism, corrupt behaviour by politicians, and even hate speech masquerading as ‘valid perspective’.

Without attempting to analyse the origins, it is worth noting the odd predicament that this current state of politics and policy creates for sociologists or anthropologists of science, at least for those of us who count ourselves lucky enough to be part of the Millstone school. For as realist constructivists of sorts, we have learned to be sceptical of single truths and facts; to ask who says so, in what context, and underlain by what social and political commitments. Truth, yes, but truth as always subject to multiple, partial perspectives, and as always provisional and contestable; a process or work-in-progress. We have learned to critique also the unquestioned dominance of ‘experts’, recognising multiple forms of expertise, experiential as well as official, citizen as well as accredited, and to ask always about the credibility of claimed expert positions. Yet somehow these days, making these points all too easily seems to align one with the relativist ‘anything goes’ camp – or at least can be interpreted as such. Scarily, in moments of heated policy and political debate in development, environment and health circles, I sometimes find myself pushed to defend the importance of ‘experts’, ‘evidence-informed policy’ and ‘sound science’ in forms of language that I would not have dreamed of using even a few years ago – and which grate on my sociology-of-knowledge sensibilities, even while being politically expedient.

So how to avoid selling out? My response would be that these days a realist-constructivist stance is more important than ever. However, we need to be more carefully symmetrical in giving weight to both elements. The facts and the realities, in all their complexity, and explored by science, really do matter. And to do that, we have to distinguish carefully between facts (and alternative facts), and framings and narratives (and alternative framings and narratives), and keep the proper study of alternatives to the latter.

Let me elaborate and illustrate through two strands of work in West Africa and the UK, both of which go back to the 1990s, yet have re-emerged in new guises today amid current and future global challenges.

The first concerns forest cover change. James Fairhead and I went to Kissidougou in Guinea in 1992 to learn about the social and political causes of deforestation in the forest-savanna mosaic, which over the last century, we were assured by policy and ‘scientific’ documents, had been degraded from the once-dense forest cover across the region to its current ‘relic’ islands of forest around villages in a sea of grassland. Our ethnography, oral history, and consultation of air photos and archives soon revealed a quite different history of forest islands growing around villages at the hands of their inhabitants, interacting with soils and vegetation in the course of their daily lives (Fairhead and Leach 1996). We have more recently updated the forest-building story by exploring the formation of the black earths which enable them, again created through (often women’s) everyday knowledge and practice (Fraser et al 2015, Frausin et al 2015) Yet a century of foresters, botanists and environmental scientists had overlooked this history, instead using a-historical methods and participating in the construction of a discourse of deforestation that stigmatised local farmers but supported the interests of the state (in fines and forest control) and donors (in projects for combatting deforestation). Although we thought we had demolished this discourse in a trilogy of books (Fairhead and Leach 1996, 1998, 2003) and numerous policy and media interventions throughout the 2000s, a generation on we find it being re-invoked amid current climate change concerns. Forest islands and reserves are now being protected again in fortress mode in the name of preserving forest carbon from assumed destruction by local farmers. New methods co-

constructed with new markets – such as the ‘baseline scenario’ – reinforce this (Leach and Scoones 2015)

One could tell this as a tale of competing, alternative narratives – a landscape half empty and emptying of forest, or one half full and filling. Moreover, such narratives are underlain by different social and political commitments: most bluntly, supporting control by state and external agencies, or villagers and farmers. We wrote about this in an article in 1995 for a special issue of *World Development* edited by Emery Roe on ‘Development Narratives’ (Fairhead and Leach 1995). The title of this article is compelling: we called this ‘false forest history, complicit social analysis: rethinking some West African environmental narratives’. For as we argued, and as I would still argue for the new carbon forests of West Africa, the deforestation story is false. There is vanishingly little evidence to support it. Indeed, one could say that the deforestation narrative occupies the realm of alternative fact. Once one sees that, one has to be even more alert to the politics underpinning it. I discovered the work of Erik, Brian Wynne and others in the SSK and realist-constructivist traditions only in the mid-1990s at the end of this first phase of deforestation work, but it proved invaluable in giving conceptual shape and justification to the cognitive and political injustices we had observed in the field.

The second strand is about health. In the late 1990s and early 2000s, again with James Fairhead I started to explore controversies around child health and vaccination, in both West Africa and the UK (Leach and Fairhead 2007). We were especially interested in why some parents refuse the apparently self-evidently good technology of vaccination for their children; in a context where policymakers assumed that in Africa this seemed to be because people were ‘pre-rational’, having not yet moved from tradition to appreciate science, while in the UK they were ‘post-rational’, overly reflexively concerned with technological risk. Engaging full-on with debates about public (mis)understanding of science, we developed a framework that sought to understand the social logics of parental anxiety. This was definitely work that foregrounded parents as bearers of (experiential) expertise, and sometimes as citizen scientists. However, the contestation – and the plural alternatives – that mattered in this work were much more about framings than facts.

Thus in the UK, where this was the time of the MMR controversy over possible links of MMR to autism, the government Department of Health (DH) insisted that ‘MMR is safe’. They backed this view with large-scale epidemiological/statistical studies from child health records purporting to show that there was no statistically significantly greater chance of a child developing autism after MMR vaccination. This was a convenient, and one might say necessary, argument to maintain the mass childhood immunisation programme, a key plank of UK public health. Yet a significant number of parents took a different view – that ‘MMR may be safe, but not for my child’. Whereas the DH pitted scientists against parents around a question framed at a population level, parents were asking quite different questions about individual clinical histories and vulnerabilities, and the plausible existence of small sub-groups of children for whom MMR was not safe. The medical establishment was not prepared to admit to this possibility. These are quite different, alternative framings. Both are valid. Both can be supported by evidence – facts – but they ask for quite different sorts: epidemiological/population statistics vs. clinical and life history data. There was, of course, an irony – the public health establishment marshalled its experts and commissioned its population studies, and its view ultimately prevailed. The parents’ medical champion was ostracised and discredited, and the parents’ questions went uninvestigated. Power ultimately shapes which framings prevail over others, and which questions get investigated and which do not.

An anecdotal rider to this story came a decade later when in 2010 the man who had been DH head of immunisation at the time of the MMR controversy attended a STEPS conference on a quite different topic. Chatting privately, he confided that the ‘MMR is safe’ statements were made on very shaky evidence indeed – the epidemiological studies were not yet ready, and the DH ‘winged it’ for the imperative of protecting the mass immunisation programme. ‘Those were heady days’, he commented.

Even headier days prevailed – and these forest and health stories came together – in 2014-15 as the West African Ebola outbreak took hold in those forests where we had lived, and I spent long days in the DH offices as the social scientist on the UK government’s SAGE (Scientific Advisory Group in

Emergencies) on Ebola. There the alternative framings were even more extreme. Was Ebola a global pandemic threat about to emerge 'out of Africa' to UK shores? The risk managers of Public Health England, and often the Chief Medical Officer and Chief Scientist, framed much of their concern and inquiries – of modellers, or UK isolation hospital provision and airport screening capacities – in these terms global health security terms. A different framing – shared by the tropical health doctors, aid agencies and social scientists in the room – saw this mainly as a devastating health crisis in Africa, and looked to understand its transmission there, the social factors influencing this, and the combinations of external and local outbreak control measures to contain it. A group of us set up the Ebola Response Anthropology Platform (www.ebola-anthropology.net) and accumulated evidence that it was community learning, and a 'citizen science' of villagers and frontline health workers learning to think like each other and adapt their behaviour accordingly, that turned the epidemic around, as Paul Richards has shown so convincingly in his 2016 book (Richards 2016).

Again, many facts were not in question – the character of the Ebola virus, how infection happens, its progression and timescales. But why this matters – to whom, where, at what scale, and whose knowledge and action might be best-placed to address it – very much were.

A final episode as this intertwined story comes full circle involves scientists and policymakers seeking the origins of the 2014-15 epidemic identify its reservoir in bats, located in forests once separate from people, but now encroached by recent and ongoing deforestation. In other words, the false forest history is being invoked again, now purporting to explain pandemics in a powerful narrative of global Eco health threats (Leach 2015). You won't be surprised to learn that one of our current projects is to question and deconstruct (yet again!) this deforestation narrative, this time with its associated sub-narrative of male hunters, in order to open the way for other questions to be asked – questions about different origins and transmission routes, ones more routed in women's lives and not necessarily involving bats.

It is easy to overstate the distinction between alternative facts and alternative narratives. Facts and values are of course co-constructed. Facts are selected by scientists as social beings. All of this was happening in these episodes. Yet somehow there is still a basic sense in which reality matters, and good science as a way of investigating it matters, whatever the particular questions one chooses to address. Questioning dominant framings as a way of opening up to reveal alternatives, which might place different but important questions on the agenda, or support the social and political interests of particular groups, or allow for alternative normative stances, is not only a valid but also a vital exercise. The point is that once the alternative questions are asked, one can pursue the science and evidence associated with them. Different narratives and framings may be equally (if differently) associated with credible evidence. The problem is when positions fly free of reality, supported by an absence of evidence, or even by false 'evidence'. That's the realm of alternative facts – aka lies.

As global challenges like pandemics and climate and environmental change, complicated by urbanisation, conflict, migration and mobility, increase, we need to keep our grip on reality (and investigate it, rigorously and robustly) while also being constructivist about ways to frame it, and to understand its relationship with policy and politics. That's what I take from the Millstone school, which I believe is more important than ever. Thank you, Erik, for inspiring us and providing us with a legacy of realist constructivism to take into the future.

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Realist constructivist contribution to food policy on the safety of genetically modified crops and foods

Claire Marris, City University

I want to start by thanking Erik for his positive contribution to my life, as a fantastic teacher. I came to SPRU in 1991 to study for the MSc in Science Policy. Before that, I had been through an undergraduate degree in biochemistry and a PhD in molecular biology, which meant I had been immersed in a scientific and positivist worldview for many years. I had always been somewhat uncomfortable in that environment but until I came to SPRU I didn't really know why. The MSc, and in particular two modules led by Erik on the sociology of scientific knowledge and on environmental risk regulation, opened my mind to two decades of scholarship in the field of science and technology studies (STS) that helped me articulate the questions that had been troubling me. It was intellectually one of the best years of my life and I ended up becoming an STS scholar myself as a result. Nearly three decades years later, I am still very happy with that choice. So thank you for that, Erik!

I then had the pleasure of interacting with Erik in different ways, including working together on a research project where we applied his realist constructivist approach to understand how and why different regulatory regimes in different European Union Member states, at the European Commission and in the USA came to opposing policy decisions regarding the safety of biotechnology products, based on the same scientific evidence, and how this played out in trade disputes. This research project is one of many that, in my view, demonstrates that this approach is a distinctive policy-relevant approach to the sociology of scientific knowledge that illuminates scientific and policy debates (Millstone et al., 2004). We showed how a range of 'upstream framing assumptions' were embedded within scientific risk assessments. Erik's realist constructivist approach helped us to understand and explain how disagreements about the safety of genetically modified (GM) crops, hormone-treated beef and recombinant bovine somatotropin were not based on conflicting scientific facts, but on different answers to questions such as: What counts as a relevant risk? Which kinds of evidence are relevant? What benchmarks are being used? What is the (implied) chosen level of protection? How should uncertainties be dealt with? Can 'no evidence of risk' be equated with 'evidence of no risk'? Understanding trade conflicts around biotechnology agro-food products in this way suggested that what was needed was not simply more science, but a different kind of science policy that acknowledges the existence and importance of these upstream framing assumptions and renders them more transparent and open to democratic scrutiny. Delicate interactions with the European Commission during this project (very aptly negotiated by Erik!) and the delay encountered with the publication of this report illustrated how difficult it can be to get this message understood and accepted in the real world of policy-making.

That project was one of a wave of studies by science and technology scholars during the period 1990-2010 that employed some form of realist constructivist approach to analyse the controversy surrounding genetically modified crops and food. Other important contributors included Pierre-Benoit Joly, Les Levidow, Andy Stirling and Brian Wynne. Some NGOs were also inspired by the approach and started to delve more deeply into the construction of scientific facts in their anti-GM campaigns, notably UK's GM Watch. Some European Member States, and some officials within the European Commission, seemed to appreciate the value of the approach. The 2001 revision of the European Directive that regulates the environmental safety of genetically modified crops can be seen to some extent as a successful outcome of the influence of this wave of STS research, although policy change would never have happened without the influence of civil society activists. The new Directive explicitly acknowledged the need to consider a wider range of risks in environmental risk assessments, including indirect, delayed and cumulative effects. It also mandated that effects on 'agricultural habitats' need to be taken into account and not just 'natural habitats.' These kinds of considerations had been absent in the original 1990 version of this Directive and this had, in part, fuelled the GM controversy within Europe, and between Europe and the USA. The definition of 'What counts as a risk?' was transformed, and conducting risk assessments within this new framing would require new and different kinds of scientific evidence.

From the point of view of the realist constructivist perspective, it was hoped that the new Directive could lead to the development of new risk-assessment protocols and experiments that would enable us to investigate different and broader kinds of health and environmental risks, in a scientifically rigorous way. It would also lead to the reorganisation of scientific expertise. And indeed, around this time, many scientific advisory committees across the European Union were restructured to include a wider range of scientific disciplines beyond molecular biology, and 'even' experts who were not professional scientists but, for example, farmers or environmental campaigners with science PhDs. There have been interesting real-world experiments with different models for the organisation of interactions between 'scientific' and 'lay' expertise in these committees, with mixed results that would warrant more in-depth analysis.

Over the last 15 years, there have been some studies that have tried to investigate the risks and benefits of GM crops in ways that would be considered rigorous from a realist constructivist perspective; but far too few, and with very limited (intellectual and material) resources. Unsurprisingly, most have been inconclusive, in the sense that they are not able to provide 'yes' or 'no' answers to the generic question 'Are GM crops safe?' since this is, from a realist constructivist perspective, a question that we should not expect be able to answer categorically because we need to ask 'safe compared to what?' In addition, the realist constructivist perspective demands better symmetry between assessments of risks and benefits. STS research in this area showed how, all too often, claimed benefits were implicitly assumed during the risk assessment process whereas any claimed risks had to be 'proven', and how this affected policy outcomes. Failing to address this issue more rigorously has also contributed to the continuing public controversy around GM crops and foods.

So the regulatory deadlock for GM crops that some had hoped would be resolved by the 2001 revised European Directive continued. And rather than devoting their efforts to risk research of a kind that the Millstone school would recognise as more scientifically rigorous, the pro-GM lobby focused most of its attention on pushing for a positivist perspective based on a simplistic separation between what counts as 'science' on the one hand, and 'other factors' on the other (typically labelled as ethical, political, or economic), which was precisely what the realistic constructivist approach had been trying to challenge. This lobby was ultimately successful when, in April 2015, a European Commission Communication was passed that allows EU Member States to prohibit the cultivation of specific GMOs in their territory even after a GMO has been deemed 'safe' by the European Commission. Crucially, this can only be done on the basis of so-called 'other legitimate factors', meaning factors that are 'not based on scientific considerations' and 'do not only relate to issues associated with the safety of GMOs for health or the environment.' With this new policy, the policy framing for risk assessment of GMOs was pushed firmly back into a simplistic positivist frame (Wickson and Wynne, 2012). This does not bode well for human health, the environment, or the democratisation of science. It does not even bode well for those who favour the use of GM crops, since it fails to address the root causes of the public controversy.

As other contributions to this Festschrift suggest (such as that by Melissa Leach), those of us who identify with the Millstone school are likely to repeatedly face this kind of frustrating situation in our interactions with policy, where evidence we produce helps to shift the underlying policy narrative but is later ignored as dominant framings come to the fore once again, often pushed forward by the powerful vested interests of big industry and capital. Indeed, much of my research these days focuses on trying to better understand policy discourse, and why positivist policy framings are so pervasive and entrenched, in order to find ways to better challenge them.

I have also recently taken over running the MSc in Food Policy at City, University of London, set up twenty-five years ago by Erik's long-time colleague and 'partner in crime', Tim Lang. Our students are fantastic people and many remind me of myself when I came to SPRU: passionate about wanting to help change the world for the better, but sometimes unsure about how to formulate their concerns in ways that can be investigated in a rigorous way. I will be very proud if I can inspire some of them as much as Erik inspired me; and empower them to find their own ways to advance more integrated and inclusive food policy.

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Pastoralism in Africa: some (very brief) 'realist-constructivist' reflections

Ian Scoones, STEPS Centre and Institute of Development Studies, Sussex

Like Erik, for many years I remained in blissful ignorance of the field of the sociology of science. Like Erik, too, I had trained as a natural scientist (in my case a biologist, not a physicist), but I did not have the benefit of his rigorous philosophy training. For many years during the 1980s and '90s in the fields of range ecology, land management and pastoral development in Africa, we were engaging in 'realist-constructivist' debates – although I didn't know it at the time.

I blame Brian Wynne for my first encounter with the sociology of science. I had just read his classic 1992 paper on uncertainty and indeterminacy (Wynne, 1992), and I was discussing with Melissa Leach and Robin Mearns a conference here at Sussex on why it was that myths about environmental change in Africa persisted for so long (Leach and Mearns, 1996). Rashly, we decided to invite him, and the early connections (now nearly 25 years ago) were made between work in development and science and technology studies, later forming the basis for the STEPS Centre and the highly productive revitalisation of SPRU-IDS interactions at Sussex.

If the classic paper by Erik and Paddy van Zwanenberg from 2000 on realist-constructivism (van Zwanenberg and Millstone, 2000) had been available in the 1980s and '90s, then much of the debate I and others were having then in Africa would have been given a really useful framework. But back then, trained in methods of positivist science, I didn't have the tools to think in new ways. I wouldn't have had a clue what 'realist constructivism' was.

As I gradually morphed into a jobbing social scientist, I searched around for useful perspectives. I didn't have much truck with the trendy post-modern theories of the time, and needed something else. We were debating real things in real environments: precipitation patterns, livestock numbers, grass species composition, landscape ecology, patch dynamics, soil erosion and so on. These could all be measured by the ecological field techniques I was trained in, or latterly studied drawing new insights from satellite imagery.

Yet, at the same time, the debate about pastoralism and land degradation was stuck, constructed on a flimsy and partial basis. Of course, so the arguments went, pastoralists in Africa destroyed the land, of course they did not manage grasslands well, of course desertification was rampant and the deserts were advancing. These were the popular and scientific narratives of the time – and indeed they persist today (see Melissa Leach's article in this volume). But these claims did not match up with what we saw on the ground, and indeed what livestock keepers would tell us. The old rangeland management – developed in large-scale ranching systems in the American mid-west, and copied in Argentina and Australia, then imported into Africa through the circuits of colonial expertise – was not a good model for flexible, mobile pastoralism in Africa.

An alternative frame was needed. In ecology, there was also an emergent science that challenged much of this. This was in my view a usefully contrasting representation of the facts that provided a more credible perspective for the African context. This became known as 'non-equilibrium range ecology' (Behnke et al. 1993; Scoones 1999), where systems were appreciated as unstable and uncertainty was everywhere. Now there were competing framings and competing facts in the debate, opened up by asking new questions in different ways. Satellites showed that deserts moved in both directions, depending on changing conditions. Different management systems, based on contrasting normative positions of what rangelands were for and for whom, required different grass and tree species. Carrying capacity was not a fixed number, but was dependent on what users wanted, their values and needs: in other words, a political choice. And so on.

I now understand our debates depended on the 'framing assumptions', and how arguments were constructed – socially, institutionally and historically. Truths emerged through a process of negotiation and in a given context. Frames and facts had to be debated, with some being more convincing than others. In

other words (and without really knowing it), we were having a debate about ‘realist constructivism’. Like all of Erik’s hugely influential work on food additives, toxicology, BSE, GMOs and more, we needed to have a debate with the science, and engage fully with realist interpretations of the world. But we also had to engage with scientists and the scientific and institutional establishment in ways that laid out how different interpretations could emerge with the same data, and so address head-on the sociology and politics of how science was constructed, specifically in southern Africa (where I was then working) in the context of a history of settler colonialism.

What, then, is the essence of Erik’s approach that would have been so useful back then? What I especially value about Erik’s work is his ability to focus patiently on the detail; he really gets to grips with the science, asking how data are deployed, always questioning the credibility of arguments. But he also sees the bigger picture, and how such arguments are constructed, and particularly the political contexts for these processes. Recently, I have learned a huge amount from co-supervising a SPRU doctoral student with Erik. In the same supervision session, here is Erik the physicist leaning forward and puzzling over the (realist) detail, and here also is Erik leaning back, the philosopher (and sociologist of science), questioning how it was constructed.

How could this approach of realist constructivism be taken forward? How could it contribute further to the tricky challenges of science-policy interactions? In my view, the approach provides an ideal conceptual platform for the type of collaborative, collective science between scientists and publics that is often talked about – interrogating the facts, questioning the frames and narratives together. There are never singular answers, they must always be debated. Truths are inevitably emergent and context-specific. The role of the science-policy researcher may be less dispassionately to judge on the merits of one or other version (on what basis?), but more to facilitate the debate, providing a brokering and translating role, so helping to ‘open up’ alternative perspectives and pathways, encouraging a reflexivity in both science and policy (Stirling, 2005).

In his writing, in his teaching, in his interactions with policy and in his multiple contributions to the work of the STEPS Centre over the last decade, Erik has forged a practical, but also radical and political, intellectual way forward. As we seek to build on his work, we hope he will continue to be involved, even in retirement. So thanks, Erik!

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Millstone grit: putting meaning, courage – and fun – into 'realist constructivism'

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Over nearly thirty years now, Erik Millstone has been – for me – a greatly valued teacher, mentor, colleague, comrade, protagonist, friend and inspiration. His unique mix of forensic rigour, dogged determination, contagious good humour and deep humanity continues to build a lasting legacy among the many whose paths he has crossed, and also leaves memorably instructive marks on a fair few of the policy agencies and global corporations with which he has tangled. No-one among many candidates encapsulates better the buccaneering SPRU tradition in fearlessly independent interdisciplinary policy research and teaching.

Despite his gentle humility, the legendary force of Erik's intellect and courage in standing up to powerful industrial adversaries helped make him among the scariest of many intimidating figures that a new graduate student might bump into in the SPRU corridors of the early 1990s. Defying the latest in office soundproofing technology, Erik's regular explosive reactions to delinquent keyboards or software – and occasional incoming emails – expressed very well, a spirit that even the most overbearing political forces could not subdue. And all the more daunting for its quiet delivery is the ostensibly innocent query from Erik-the-philosopher (in one of his frequent "remissions of epistemology"): "What exactly do you mean by that?" Many are the averted embarrassments in my own efforts that I owe to this uncompromising – but deeply friendly and constructive – scholarship. (Of course, many more causes for my own embarrassment still remain).

It was only later that I realised these traits in Erik are facets of the formidable stamina and energy acquired over decades of struggle against overbearing corporate self-interest of a kind and strength all too often left unchallenged by the polite etiquette of policy research. Indeed, I learned in my own later work with him how the bullying tactics of the mainstream food industry can put those of the (more familiar to me, at least) nuclear, chemicals and biotech sectors in the shade. Yet whether the issues were about lead (Millstone, 1997), food additives (Abraham and Millstone, 1989), artificial sweeteners (Millstone, 2013), nutrition (Lang et al, 2005), veterinary medicines (Millstone and van Zwanenberg, 2001), BSE (Millstone and van Zwanenberg, 2005) other animal diseases (Millstone et al, 2006), sugar (Lang et al, 2017), food processing (Millstone et al, 2015), genetically modified crops (Kashani and Millstone, 2016), biosafety regulations (Millstone and van Zwanenberg, 2002) risk assessment (Van Zwanenberg and Millstone, 2001), the precautionary principle (Millstone and Van Zwanenberg, 2003), international standards (Millstone et al, 2004), trade disputes (Segal et al, 2015), global assessments (Brooks et al, 2009), agricultural strategies (Millstone, 1994) or innovation policy in general (Millstone and Van Zwanenberg, 2001), society as a whole owes a great debt to Erik and his colleagues' periodic major victories in upholding public interests against attempts at distorting regulation. Even in the simple drinking of beer, Erik has found opportunities to challenge possible skulduggery (Thomas and Millstone, 1993).

Over the years, it has been related to me a number of times by senior figures in business, the UK food and farming ministry, and the European Commission, that the name Millstone is both respected and feared among those who would put private profit (or just an easy life) before wider wellbeing. Likewise nearer to home, there are quite a few ill-fated 'consultants' or 'facilitators', who have found themselves on the receiving end of a devastating one-liner or a waving walking stick as Erik escapes with panache from an especially inane 'workshop'. Yet for hundreds of students and colleagues, Erik's fire and passion are among the main aspects of his charm that have earned him so many laughs and claps and cheers in his lectures over the years.

For what comes over most of all with Erik is his warmth, compassion and sense of fun. His massive ever-ready stock of set-piece jokes and anecdotes are a frequent tonic for the (multiple) Kafka-esque sides of university life – and can keep any seminar on the edge of its seats. And Erik's durable collegiality and disarming self-deprecation are qualities that universities could do with a lot more of: helping resist the creeping individualism, pomp and hubris of modern academia. For instance, there is the way in which Erik

himself pushed for the present discussions to focus on widely shared analytical and policy questions, rather than on a much-justified celebration of the lasting significance of his own personal contribution.

Nonetheless, I should respect Erik's wishes and wrestle briefly with the three – characteristically insightful – framing questions that he has suggested we reflect on in this discussion.

First, we are asked if what Erik calls 'realist constructivism' is a distinctive policy-relevant approach to the sociology of scientific knowledge that can illuminate scientific and policy debates? What Erik has done with this idea is to join together two jealously-divided (Abraham, 2002) academic silos (realism and constructivism), which should actually never have been separated in the first place (Millstone, 1978). And what Erik's work here has shown, arguably more than any other, is that by refusing to be constrained by such artificial disciplinary boundaries, a huge amount can be achieved, both intellectually and in policy and wider political terms (Millstone, 2007). As a result, what Erik and Paddy van Zwanenberg refer to as "a constructivist approach grounded in epistemological realism" (Van Zwanenberg and Millstone, 2000) has (by whatever name) come to be recognised not only as policy-relevant but also as the only truly effective way to articulate crucial insights of constructivism in sceptical corridors of power, where these critical messages can sometimes be most unwelcome (and yet at the same time, in the long run, most useful) (Millstone, 2000).

Second, Erik challenges us to reflect on the use and limitations of realist-constructivist approaches to the sociology of science, particularly in relation to policy analysis, critique and prescription. In short, this value – for me – lies in resolving together the twin pathologies of each of its component parts (Millstone, 2002). These are: the danger that constructivism can neglect the more coarse-grain structural features of power; and a tendency for self-proclaimed realism to be deeply unreflexive in the face of uncertainty. For evidence of the value of addressing these weaknesses together, one need look no further than the voluminous corpus of work by Erik and his many doctoral students on the myriad ways in which vested interests can warp the dynamics of science and emerging technologies – and how this can be countered (Millstone and Van Zwanenberg, 2001).

If such problems were tackled in a purely constructivist way, then it would be more difficult to challenge, as assertively as is often necessary, the most entrenched cases of regulatory stitch-up. The brutal ways in which the sugar industry, for example, not only shapes its captive markets, but also seeks to obstruct and shut down inconvenient research, can be too clumsy and mechanical for some of the more subtle vocabularies of constructivism. However, if science is treated in unqualified 'realist' terms – with too little attention to the profound conditioning of knowledge by its social context – then one of the most important kinds of insights is side-lined. We might miss how concentrated gradients of power can (like a gravitational field) always bend the content of even the most apparently disinterested science, in order to represent the world in the most expedient ways (Millstone et al, 1999). Combining the best of both approaches, then, the finely-honed methods that Erik has helped develop over so long, allow much more systematic challenging of some of the main warping effects in regulatory science, by upholding those very standards of rigour that have been most captured (Millstone et al, 2008).

Erik's third point for this discussion is to ask for suggestions on issues and debates where a realist constructivist approach could be most effectively applied in future. Here my answer is that Erik's own work again shows the way (Millstone 2009). His distinctive mix of insights has never been needed more than is the case now. With globalising institutions like the World Trade Organisation intensifying their stranglehold on the way science is interpreted, with authoritarian populism strengthening the hand of big business, and with academia ever more pressurised into sycophantic alignments with power, the world needs like never before the combination of rigour, tenacity and bravery that has always characterised Erik's work (Millstone and Lang, 2008).

So, thanks Erik! I've learned an awful lot from you – more than I can say. And we've had a lot of fun, which I am looking forward to continuing. All I can conclude is that, though I wish you a little more well-earned time with your beloved family, Vienna and the Alps, I trust you're still going to be regularly around in the corridor, helping to keep SPRU attention firmly fixed on one of the most important and difficult strands of our mission – and sometimes helping to save us from ourselves!

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Some brief reflections on the past and future of Erik's contribution to policy-relevant sociology of science

Patrick van Zwanenberg, Argentina National Council of Sciences (CONICET), based at the Centro de Investigaciones para la Transformación (CENIT) and STEPS América Latina.

Like many of Erik's graduate students, I arrived at SPRU with a background in the natural sciences, curious about the politics of knowledge but for the most part ignorant of the literatures on that topic. Erik introduced us to the philosophy, history and sociology of scientific knowledge through his MSc course on the social institution of science. He did so insisting that we think hard about what a range of different ideas about the social nature of scientific knowledge might mean for how we conceive of, research and intervene in science-based policy-making processes. I had taken a short undergraduate course in the philosophy of science a few years earlier at a different university but it had been taught entirely in the abstract. This was refreshingly different.

I subsequently began a doctorate with Erik, exploring how a realist-constructivist sociology of science could illuminate the nature, basis and politics of institutional decisions about pesticide safety. At that time Erik was the only source of decent coffee on the entire campus. High quality caffeine, combined with 1930s jazz and blues on his office tape recorder, wide-ranging conversations, a line in slightly juvenile jokes, and an ability not to take the academic enterprise or himself too seriously (Erik had, and may well still have, a large collection of rubber stamps for university correspondence, including one that stamped the word 'bullshit' on offending documents) meant that supervisory conversations were always a pleasure. At the same time, Erik took very seriously what several of us were trying to advance – in terms of developing and experimenting with critical, policy-relevant STS approaches. Supervision involved long conversations, often spilling over to a walk on the Downs, about my various ideas, plans and drafts. Amongst other things, Erik tried to instil precise, unambiguous modes of thought and writing, no doubt a reflection of his philosophy training. Particularly helpful were his efforts to persuade me that I did have important and relevant things to say. I want to offer a heartfelt thanks to Erik, for all of this process of learning, and subsequent collaboration, and for his company and friendship over the years.

Pondering on the future possibilities for realist-constructivist approaches to the sociology of science, and in particular for analyzing technology and environmental regulation, it is striking how far UK and continental European regulatory cultures in fields such as chemical and food safety have shifted over the 25 years since I began working in this field. In the early 1990s, British pesticide regulation was an almost entirely opaque affair. I was able to find out far more about how scientific and regulatory decisions had been made by UK regulators about the safety of a group of pesticides during a visit to the archives of the US Environmental Protection Agency in Washington DC than I ever discovered from the very limited documentation available in Britain or from interviews with British civil servants.

Nowadays, a combination of digital infrastructure, institutionalized Freedom of Information, and other reforms stemming from the fallout over BSE (as well as, I suspect, the influence of Erik and others in shining a critical light on science-based regulatory policy-making) have meant that far more detailed information on the construction of evidence and policy is available in the public domain. It is, as a consequence, far harder to continue, plausibly, with the orthodox technocratic representation of this area of public policy, familiar to those of us who worked in this field in the 1990s and earlier, whereby regulation was supposedly an entirely a-political, technical issue, based on reliable 'sound science', and which social science researchers had no business poking around in and asking awkward questions.

The on-going saga over the safety of the herbicide glyphosate illustrates well the changing context. In 2015, the International Agency for Research on Cancer (IARC) concluded that glyphosate was a 'probable' human carcinogen, in stark contrast to the conclusions of every national and pan-national regulatory institution (including a recently completed review by the German government on behalf of the European Commission). The traditional response to such dissenting minority opinion – that IARC's

analysis must be scientifically flaky or consciously politically biased – just didn't seem to wash, despite (on-going) attempts by the agrochemical industry and some regulatory scientists to make just those insinuations. Instead, it rapidly became clear that different institutions were asking slightly different questions, drawing on different bodies of data, applying different standards of data relevance and reliability, adopting different approaches to the overall assessment of evidence, and even examining slightly different substances (German regulators assessed tests on just the active ingredient for glyphosate whilst IARC also included studies on the commercial formulations). Furthermore, it soon transpired that the manufacturers of glyphosate had been intimately involved, for decades, in the process of regulatory assessment in both the USA and Germany. That involvement raises important questions about where the numerous subjective, legitimately contestable judgements necessarily embodied in regulatory assessments about the safety and acceptability of glyphosate (and in pesticide regulatory assessment methodologies more generally) had originated, and about how socially or empirically robust they actually might be.

Thus, at least in some jurisdictions, and for some issues, the otherwise extremely difficult, painstaking 'deconstruction' of regulatory decision-making – a task that would once have taken months or even years of work – is (partially) occurring in front of our eyes. Opportunities for realist-constructivist analyses of issues such as glyphosate regulation, and the important regulatory and wider politics that such analyses can help illuminate, are there for the taking these days, in ways that would simply not have been imaginable in the UK a couple of decades earlier.

A promising future line of activity for realist-constructivist sociology of science lies, in my opinion, in working more closely with people in the disciplines that we study. In doing so, we could go beyond identifying key indeterminate or underdetermined decision points in the construction of knowledge, the alternative options and pathways that were potentially available at those junctures but were not taken, and an exploration of why, and with what implications. Working with practitioners, we might also help to construct some of those alternative knowledge pathways.

To take a brief example, we recently scrutinized a series of influential analyses of the impact of plant genetic engineering and other agricultural innovations, over the last two decades, on soya production in Argentina [Marin et al, 2014]. One of the things we noted is that the assessments tended to ignore the impact on the performance of conventional plant breeding on soya production. Some analyses also effectively conflated the indirect impacts of conventional plant breeding on soya performance with those from genetic engineering (GE), but then assumed that all those indirect impacts of seed innovations were solely a product of GE technologies. The most charitable interpretation is that prior assumptions that GE technology must be the key seed-based driver of changes in performance shaped the analyses in ways that made it inevitable this would be the conclusion. The knowledge pathway, potentially available but not taken in this case, would have been to produce a comparative estimation of the impacts of conventional and GE seed innovations. Working with some economists, we therefore decided to try and partially conduct just such an assessment. Our preliminary analysis indicated that conventional breeding seems to have had a far greater impact on overall soya performance than GE technology (Marin et al, 2014).

Collaborating more closely with natural and social scientists – from the particular disciplines that we study – to produce what is sometimes referred to as 'undone science' is not in itself novel. However, the opportunities for practitioners of realist-constructivist sociology of science to do more in this vein, drawing and building upon Erik's pioneering contribution to policy-relevant sociology of science, are surely considerable.

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Speaking Truth to Power: a philosopher brings Truth, and Science, to Earth

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Preface

Erik invited us to participate in this retirement occasion with some questions on which we might reflect. One of his many inspirational qualities for students and colleagues has been that he never flinched from difficult questions. In this brief appreciation of Erik's work, I start with some recollections as to the context in which he and I began, and subsequently worked both together and independently over our careers in STS, spanning nearly 50 years. Then I will describe some of the special qualities I have seen in him and his work. I will finish with some questions – ones that Erik has not yet answered; but then, nor has anyone else – which remain for anyone, most likely as ones with which everyone should just continue to struggle, and share.

Historical beginnings

My own career in STS¹, or rather for me in SSK, began like Erik's in the early 1970s, though I think he began here at Sussex as a PhD student in philosophy of science, whereas I didn't start looking at science, and specifically scientific knowledge, from a historical, philosophical and sociological angle, until after my PhD (1971) in materials science. Both of us came to STS as scientists by training, in my case including research and publication, thus deepening my illiteracy and naiveté about the social, political, historical and philosophical dimensions of science. Erik turned to philosophy for his PhD and took up what was the rapidly emerging SISCON (STS – see fn.1), whereas I only began to see the point of looking at science in a systematically reflective way, including politically, when I was thinking of accepting the offer of a post-doc research contract in my department, Materials Science at Cambridge. The very experience of trying (and failing) to negotiate the focus of that scientific post-doc was what began to remove the scales from my eyes about the scientific research trade I had enjoyed in a free-rolling but wholly unreflexive way, and was happily contemplating spending my life in – but that is a story for another occasion!

It was the uncompromisingly clear and challenging early work of SPRU from the late 1960s, led by Chris Freeman, and of the articulate activist David Dickson and others (like Pete Chapman) launching the British Society for Social Responsibility in Science, BSSRS, which made me realise belatedly that there was – and remains – a huge politics (as well as unspoken and unresolved epistemic questions) involved in the ways governments spend money like water on certain domains of science, technology and innovation, while starving others altogether². In the early 1970s the emblematic issue on this was energy, with military and civil nuclear developments taking virtually all the resources, while alternative supply options and energy demand-management policies and innovations were starved. Consequently, the corresponding social and political questions were left unaddressed.

It was some early sense of this, but still completely naïve, that led me to decline the materials science research post-doc, and instead to seek a new trajectory that would allow me to explore these bigger life-questions embedded within my science. Thus on the advice of Pete Chapman, a fellow PhD student friend in the Cambridge University Cavendish Lab, I went to Edinburgh and the Science Studies Unit under David Edge, with Gary Werskey (soon to be replaced by Steve Shapin), David Bloor, and Barry Barnes as three lecturers in history, philosophy, and sociology of science, respectively. I might easily instead have found myself at SPRU, with Chris Freeman, and HSSU, under Roy MacLeod, with such charismatic figures as Brian Easlea also doing their colourful STS thing³. At Sussex, Erik developed in a more determinedly activist science-critical environment than I did at the Edinburgh SSU, which presented itself as strictly academic and 'scientific' in its investigation of science, and which was indeed criticised for this by the growing and more activist network of scientists, social scientists and humanities scholars involved in our field.

Because in the early 1970s there were only a handful of places and people developing teaching and research in what was then called Science in a Social Context, it seemed as if we met each other quite a lot, reading and discussing each other's work as well as the Kuhn-Popper-Lakatos-Feyerabend exchanges. Inter alia, the annual SISCON summer school at Coleg Harlech involved intense week-long "getting to know you" debates and arguments, keeping us awake into the early hours and constituting key parts of the collective STS learning curve.

I recall getting to know Erik during these early days, including at the Harlech discussions, and recognised right from the start his burning commitment to getting science to do the right things, and also to get it to do those things, whatever they might be, in the right ways. This was a normative commitment to proper science, but with three further, key elements that almost defined 'SISCON', or what came to be known as STS:

- (i) it was emphatically not just a philosophical indulgence in epistemology alone – it went far beyond this disciplinary focus alone;
- (ii) it involved making science, and the economic and political forces that controlled and shaped science in both its productive (innovation) and protective (risk, regulation, etc.) modes, more democratically accountable; and
- (iii) this represented a practical challenge not only for science per se, and for scientists, but also for the economic and political forces that controlled and shaped science – that is, for 'society'.

As Erik has demonstrated consistently and persistently throughout his long-standing and varied body of work, one cannot do these things without getting stuck into the prevailing scientific knowledge itself. This includes exposing its unstated assumptions and framings. In so doing, Erik avoided the fashionable promotion of 'alternative sciences', and instead through his own critical research uncompromisingly challenged science to conduct (and shape) itself properly. Back in the 1970s when he began this programme, Erik was almost unique in eschewing fashionable 'big theory' in favour of carefully selected, politically salient, detailed empirical investigations of how scientific knowledge was constructed and deployed in societal decisions about risk assessment and regulation of economically important new chemical technologies such as food-additives, artificial sweeteners, and pesticides. So much of the more recent public engagement with science work (including some associated with my name) has failed with respect to this crucial requirement to engage with the technical-scientific knowledge in play. With all this kind of fashion-modelling going on around him, Erik has stuck to his last and continued, patiently and carefully, to show in evidential detail how and why it matters to conduct this kind of interrogation of the scientific knowledge defined – and exploited – as public authority.

So here I come to my chosen title to describe Erik's outstanding and seminal lifetime contribution to the field of STS and to policy, especially public regulation done in the name of science.

Speaking Truth to Power

This phrase has become something of a cliché since the post-war days of debate on expert advice to policy, with relentlessly if unevenly proliferating public controversy and confusion over expert disagreement. Conventionally, it is 'policy' or 'publics' that have taken the blame for a lack of scientific authority in public arenas. Moreover, in the days of Trumpeting "post-Truth" and "Fake News", with barefaced lying as an almost-normalised mode of public life, it might be described as an obsolete concern. Erik has shown – indeed he continues to show in every aspect of his work and commitment – why this cannot be so.

But notice here an important and distinctive dimension of this, in Erik's case. The Truth that Erik's work seeks to bring into being and uphold as Public Truth – and to speak it to Power – is not just composed of Facts, and Evidence. As with all good STS-SSK scholarship, it is both *about* scientific knowledge and its conditions of authority, and also a contribution *to* scientific knowledge and its (conditional) authority, as this should and could be. In contradiction of the dominant, blithely imposed assumption that it is about factuality alone, truth involves, and is, more than mere facts, however strongly or weakly these may have been validated; and it is about more than only factual questions. Truth is multi-dimensional, more than

just epistemic; and it is a condition, not only a propositional verity. It is also a collective *process*, or an historical way. As such, it is never complete but always – if the proper conditions are in play – emergent; it is never completed nor final, which is the typical (but ultimately fragile, and inviting its own come-uppance) way in which it is communicated.

Thus, truth in its real and important form is not just a comforting reference to ‘facts’. It is a relentless struggle – something to which Erik’s exemplary activist-oriented research and publication-record, and crucially his mundane daily practice, bears witness. In this more complex version of *Speaking (and Seeking) Truth*, I also want to include, for our collective gratitude, not only his many genuinely influential research-based publications, his numerous policy-focussed and thoroughly researched reports on so many important policy issues involving science, and his willingness to commit his special expertise to necessary public causes. In addition, I want to acknowledge gratefully his unrivalled and fabulously extensive teaching and supervision. This latter alone represents a major contribution to the global STS and policy field. Indeed, I find it hard to think of anyone in our world who enjoys such a list as does Erik, of ex-students as now-distinguished STS and related scholars. This is also an essential as well as a revealing part of the Truth that Erik speaks to power – bringing that Truth down to Earth through a practice which, in all its diversity, variation and sophistication, remains an integrated whole. We also hope this will be a continuing one!

Realism and constructivism à la Millstone

In their (van Zwanenberg and Millstone, 2000) *Science, Technology and Human Values* paper, Erik and Paddy van Zwanenberg address a prominent and lasting school of thought in STS, namely (social) constructivism, and the debates which had existed for about a decade between its adherents and realists or critical realists who rejected what they understood to be constructivism’s lack of respect for realism as the grounding for legitimate truths and scientific knowledge-claims. Many scholars clung to one side or the other of what was a highly polarised and too-often *unconstructive* debate – either reality was socially constructed in discourse alone, or it was ultimately shaped by reality (however obscure that was). As usual through detailed empirical case-analysis, Erik and Paddy instead proposed “Constructivist Realism”. This went beyond the dogmatically entrenched positions then predominant, and explained why other more complex options are available indeed necessary for interpreting scientific conflicts in public arenas.

I had spent several previous years addressing the same issues but with a sustained and developing interest in hermeneutics, “reflexivity” and risk, as well as in the realist-constructivist stand-off⁴, developing what I called (Wynne, 2002) a “Realist Constructivism” perspective⁵. We soon realised that these were essentially the same position. Reality is only ever observable indirectly, even though valid scientific knowledge reflects (selective) real conditions. Moreover, it is normally more multiply contingent, situationally conditional, and forever under intervention and change – and thus emergent and unpredictable – than is usually understood. To be true, scientific knowledge must recognise these points, in a reflexive way. Yet to do so is feared – by scientists and their political clients or patrons – on the grounds that it will undermine public authority and trust. Moreover, to state that scientific knowledge is more contingent or conditional than is usually recognised is not to abandon legitimate truth or authority as social aims or goods, but to recognise that these are more demanding of our political (and scientific) institutions and actors than anyone - in science, science policy, regulatory and public policy, commerce and industry, politics and the media - has yet recognised. I believe this to be common ground between realist constructivism and constructivist realism; and it is a ground from which serious challenges are posed to democratic politics and modern culture, as well as to science, when scientific knowledge is increasingly controlled by less-than-impartial political-economic forces.

Some questions

My first question here is more of a hypothesis, or a provocation, which follows from the foregoing:

- As a social institution, science has always needed patronage. Nowadays it is so deeply and inescapably dependent upon often competitive financial and related patronage, as well as upon uncritical media adulation and abstract secular worship. Does this mean that science does not understand its own epistemic (and hermeneutic) contingencies, nor how to express (let alone

communicate) its own contingencies, conditions of non-knowledge, or pre-empted (because they are ignored or avoided by patrons) domains of potential knowledge?

- Does this “*unreflexive*” condition of science engender public discomfort and incipient mistrust in or ambivalence about science and its claims to public authority? And is this something that is exacerbated by the existing political economy of science? How can contemporary science even imagine, let alone expect, it has some claim to public authority, if it is structurally (and perhaps even constitutionally) incapable of admitting, articulating and addressing its own multiple intersecting contingencies?

My further questions, also inspired by Erik’s work, are as follows:

How can STS break loose from its own comfort-zone of critical work exposing the regular failures of science to live up to its own supra-social ideals or norms (as demonstrated by post-Kuhnian empirical sociology of scientific knowledge and culture in the 1970s, followed by lab studies such as those by Knorr-Cetina (1981) or Latour and Woolgar (1979) to suggest more positive or better trajectories for STS than those we inherit and criticise?

More overtly in the fields of science that intersect with and influence policy (as Erik’s work has done), how can we move forward from sophisticated diagnosis of public conflict over policy-relevant scientific knowledge, much of which involves critical analysis of powerful political manipulation of scientific expert knowledge? Can we show more constructively how scientifically informed (which must include appropriate STS-SSK analysis, for instance of the tacit framing choices shaping scientific knowledge) policy options can be identified and argued through legitimately conflictual democratic political processes?

Given that so many if not all such scientific conflicts involving policy questions over important things like the acceptable risks of glyphosate pesticide, end up in such an unresolved state (even in the science, let alone in the policies and politics which are supposed to flow from that science), what can we STS folks say should be done next? More but better (more honest, more independent, more searching) science? That might be one way of reading Erik’s long-term stance, representing a challenge to both policy and science – to enforce Merton and the CUDOS norms, if you like. Yet in the currently extant glyphosate case, to take one example from many, we have had about as much science as can be had, but we are no closer to a resolution, scientific or political.

Andy Stirling’s challenge is more directly to policy-makers in the face of such impasse. It might be paraphrased as follows: “take responsibility and speak truth to the public, namely that science can’t tell us whether or not, nor under what conditions, it is safe or harmful; it depends on the conditions that prevail in multiple real situations – of production, of use, and of the risk research itself.” This requires that not only science as expert authority, but also policy as recipient and ultimately responsible agent, should be required to up their ante, and develop some further legitimacy-criteria for decisions on our behalf.

My experience of policy (and I mean, policy-actors, as distinct from scientific) actors in fields involving scientific knowledge is that they are scared to ask publics to cope with radical contingency and plurality in the development of public commitments involving (but not reduced to) scientific questions. They have become habituated to pretend that no such contingency and plurality, nor any normative framing questions for the science, exist at all. Instead of honestly contradicting such entrenched policy cultural habits, scientists involved in policy advice typically comply with this institutional myth-making – one might even say lying – and instead look the other way, often blaming public inability to cope with scientific indeterminacy. This could be tested empirically, yet this is never done, at least not as a public social experiment.

Other parts of this potentially more constructive STS shift, amplifying the essential critical parts to which Erik has contributed, include questions as to where the burden of proof of harm is set, and how Risk Assessment Policy (Millstone *et al*, 2008) can be done *democratically and accountably*, not privately by administrative agencies acting as if they had a democratic mandate. Erik has already begun to lead

us towards this key future domain of STS and related analytical work. His work illustrates how and where unaddressed democratic questions in the key ‘scientific’ field of risk assessment of thousands of commercially produced chemicals and artificial foods are being pre-empted by the pretence that parliaments have decided such risk assessment framing questions, when there has actually been no such democratic debate and decision. Erik’s 2008 report on Risk Assessment Policies for the EU Institute for Prospective Technology Studies compared international practices in the normative framing of risk assessments of new products aimed at global markets, and showed that important, even crucial, political choices in framing such risk assessments were being obscured by portraying them as solely expert scientific issues. This pioneering work is likely to lead to further STS policy-engaged research and to associated policy developments, of which Erik can be proud to have been the intellectual godfather.

One way of summarising the above discussion is to suggest that Erik has all along been reflecting the reasonable demand that science live up to its own and Merton’s grandiose claims for itself, exemplifying in practice the institutional norms of CUDOS (communalism, universality, disinterestedness, and organised scepticism). As Mulkey (1979) suggested, following the empirical falsification of those ideal norms as a descriptive account of science: perhaps the Mertonian ‘description’ should be seen more as an ambiguous constructive heuristic, whose false descriptive discourse nevertheless in its propositional *idiom* actually exercises prospective material influence on emergent, scientific practices. It may thus regulate future science by bringing otherwise divergent practices more into line with those Mertonian ‘descriptive claims’. Thus we might speculate, the rigorously founded challenges that the body of work by Erik and those he has led, represent a challenge to science to live up to its own (Mertonian) claims and promises – “Put up, or shut up”, as it were.

A personal conclusion: unassuming and principled leadership

I want to close by sharing something on Erik, his work and his influence on new recruits to the field, which I found from my own archives when preparing to give this talk. Searching for anything on “Erik Millstone”, I found a reference letter that I had composed for Sussex University in 2001, when Erik was being considered for promotion to the elevated rank of Reader. Looking back all those years, I am quite proud of this small paragraph which formed part of my letter of support for Erik:

“One striking – and relevant - aspect of Erik’s work in often controversial scientific and policy areas, is the way in which people who have been his adversary in debates in those settings, express very great respect for two key things – Erik’s honesty and integrity, and his rigorous principled approach to his work. Even when they have disagreed with his normative stances on whatever specific issue was on the agenda, they have seen and respected these qualities. They are ones which Erik has also passed on to younger colleagues as a matter of real intellectual and indeed moral leadership in this field. This deserves proper recognition.”

Well, confidentiality norms of 16 years ago being suspended for a more deserving cause now, these are the principled qualities that Erik has relentlessly lived throughout his professional life, adhering to them with modesty and with self-deprecating humour. This will be one further legacy of Erik’s work and his leadership, besides the important and distinctive intellectual contributions that Erik has made to the fields of STS and Science and Public Policy.

Endnotes

¹ I do not know when STS became the accepted name of our field. Certainly in the early 1970s, STS did not exist as such, but the academic (and political) work that later became known as STS certainly did. It was then focused explicitly on science, as “Science Studies”, and in the UK was loosely known as SISCO, after the 1970s Nuffield Foundation-supported higher education teaching programme for mainly undergraduate scientists entitled Science in a Social Context. Other private foundations supported such teaching but also related research, such as the Wolfson Foundation’s funding from 1968 onwards of the Science Studies Unit at Edinburgh University. Significantly, SSU was then in the Science Faculty, its mission being to teach SISCO to science undergraduates. This was also true for most, though not all, of the equivalent programmes at other UK universities.

² This was first expressed in published form as a novel policy issue, by Alvin Weinberg, Director of the US Oak Ridge National Laboratory, and an articulate proponent of nuclear technologies, in an article in *Minerva* in 1963.

³ Another member of the SPRU firmament from whom I later learnt a lot was Melissa's father, Gerry Leach, whose 1970s work on end-use efficiency of electricity was (with Chapman's) way ahead of its time.

⁴ This was well represented at that time in Lancaster, with colleagues such as Alan Holland, John O'Neil, Andrew Sayer, Bron Szerszynski and Mick Dillon in regular seminar discussions, and Roy Bhaskar, Sheila Jasanoff and others as visiting speakers.

⁵ Confusingly, there is also a quite unrelated strand of debate in the field of International Relations, which also witnessed a debate between "Realism" and "Constructivism", albeit with different meanings. Even more confusingly, this difference was also resolved (Barkin, 2003) with the term "Realist Constructivism", again used in a totally different sense to that of STS.

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