Nordic Journal
of Science
and Technology
Studies

2013 | Volume 1 | Issue 1

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NORDIC JOURNAL
of Science and Technology Studies

Volume 1, issue 1

Photo in this issue:
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Design: Black Moose

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Published with the support of the Nordic Research Councils for the Humanities and the Social Sciences and the Norwegian University of Science and Technology.

ISSN: 1894-4647

nordicsts.org

NORDIC JOURNAL
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EDITORIAL

Making sense of Nordicness, or making Nordicness?

by Marie Antonsen, Kristine Ask, Anja Johanssen

What is Nordic? Does it make sense to talk about Nordic Science and Technology Studies (STS)? If so, what kind of contributions could Nordic perspectives give to global STS and other disciplines? And what elements of the international field of STS are being developed and honed by Nordic scholars?

These are some of the questions we are opening up for debate with the first issue of Nordic Journal of Science and Technology Studies (NJSTS). The first paper by Henrik Karlstrøm and Terje Finstad looks specifically at these questions. They use the Norwegian word 'stedegenhet', roughly translated as 'place-ownness', to discuss the implications of geographically anchoring a discipline. They play with the linguistic content of a word that simultaneously means uniqueness of place, the changeable nature of places and their stubbornness to change. Karlstrøm and Finstad conclude that STS is theoretically well suited for handling interdisciplinary challenges and that the Nordic region is ripe with examples of this. However, the exact nature of 'Nordicness', and whether it is even useful category, is a question that should be investigated empirically, debated, and re-visited during the life of this journal.

At the first Nordic STS conference at Hell, Norway in April of this year, we hosted a panel titled *What is Nordic STS?*. Answers ranged from interest in the Nordic welfare model to a more general Nordic inferiority complex. Some noted a fascination with technologies that produce heat (rather than let's say food), and a research approach that was either highly pragmatic - or maybe just ahistorical. While others hypothesized that nothing was simply 'nordic'. The diversity of themes in the responses demonstrate the potency of such a question. When we now launch NJSTS it is to be a place where such questions can be deliberated on. We aim to strengthen the standing of STS in the Nordic region, to invite Nordic scholars to showcase their work and position Nordic STS research internationally.

The scope and focus on NJSTS is application and/or development of theory in relation to the study of science and technology, translation and/or rewriting of STS theoretical concepts for a Nordic audience, showcasing theoretical and methodological developments in STS, as well as presentation of new empirical data from a Nordic context. However, the journal will also be of interest to an international audience and we encourage international scholars to contribute with comparative cases and perspectives from other contexts. Journal articles are published in either a Nordic language (for pragmatic reasons only Norwegian, Swedish, Danish) or in English. The editors of *NORA - Nordic Journal of Feminist and Gender Research* has labelled this strategic Nordicness; "an inclusive strategy of incorporating extra-Nordic articles that compare with, or have direct bearing on, Nordic matters" (Åsberg, Rönnblom, Koobak 2012:2).

While we use the term Nordic freely, and somewhat frequently, it is a concept that holds different meanings depending on topic and field. Nordic refers on one hand to a socio-political reality of the Nordic Council and the Nordic Council of Ministers; formalized cultural and political collaborations between the Nordic countries established after World War II, including Norway, Sweden, Denmark, Finland, Iceland, the Faraoe Islands, Greenland and Åland.[1] For international audiences it would be relevant to point out that the Nordic countries are looking back at a history of turbulence with wars, alliances and shifting power relations. This includes a union between Sweden and Norway, as well as periods of Danish rule in Norway, Greenland and the Faraoe Islands. In contrast, the contemporary political co-operation is said to be built on "common values and a willingness to achieve results that contribute to a dynamic development and increase Nordic competencies and competitiveness."[2] One outcome of this is joint research councils, like the Nordic Research Council which has supplied funding for NJSTS.

1 http://www.norden.org/en
2 http://www.norden.org/en/about-nordic-co-operation/nordic-co-operation

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In our first issue we have explicitly themed the Nordic, with contributions from both Nordic and international STS scholars. In the first article Henrik Karlstrøm and Terje Finstad looks at the state of Nordic STS research. They provide an overview of STS related institutions and activities in Nordic countries, and how different adaptations of STS theories is linked to institutional practices. In the second article Knut Sørensen draws on of four empirical studies on energy policy in the Norwegian context; cars, wind power, hydrogen for transport and carbon capture and storage (CCS), in order to link socialization and domestication to innovation studies. We also present interviews with Harry Collins and Brian Wynne, two prominent figures in the controversy regarding the 'third wave of STS'. They deliberate on and fruitfully disagree about the role of politics within STS and the relationship between established expert knowledge and less publicly verified types of knowledge.

The book reviews cover both theoretical issues (practice theory and the relevance of gender in Information and communication technologies) as well more empirical studies; of environmental politics in Norway and Scandinavian design, respectively. Design historian Kjetil Fallan in *Scandinavian Design* (reviewed by Maija Mäkikalli), challenges the usefulness and relevance of a term like Scandinavian design, described as a "cleverly crafted concept [which] has led to a disturbingly narrow understanding of Nordic design culture"[p. 1.], meaning characterizations such as "'humane', 'democratic', 'organic' and 'blond'." More than anything, he argues, Scandinavian design is something performed. We could say that goes for "Nordicness" as well.Thus, positioning the Nordic in a journal like NJSTS may be more about performing the Nordic than it is establishing a definition once and for all. Our aim for the inaugural issue is to be a conversation starter about both Nordicness as well as a contribution to ongoing controversies within STS.

NJSTS is an online Open Access Journal published under the Creative Commons License, meaning that all content is free and available for reuse and remixing (presuming correct attribution takes place). Our choice to use an Open Access format is threefold; first and foremost it democratizes scientific knowledge. In the traditional model scientific knowledge becomes intrinsically linked to finance, limiting access to those affiliated with financially strong institutions. In a postindustrial society this exasperates differences between inside/outside, and between north/south and east/west. Secondly it ensures that authors retain the right to distribute and use their own research as they see fit. Thirdly, it is about the communicative element of scientific publishing: "Granting readers full reuse rights unleashes the full range of human creativity to translate, combine, analyze, adapt" (Carrol, 2011:1). This last element in particular is something we encourage in our readers and future contributors, and in line with this we wish to invite responses, recommendations or rebuttals to any of our articles.

With that we welcome you to join our performance of Nordic STS as we present to you the inaugural issue of NJSTS.

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THE *STEDEGENHET* OF NORDIC STS

by Henrik Karlstrøm & Terje Finstad

The increasing formalization of STS research networks in the Nordic countries prompts a discussion of how research and academic work in the region is constituted – what makes something 'Nordic' STS as opposed to just 'regular' STS? Similarly, the degree to which international STS theories can be translated into a Nordic institutional context is a matter of importance for assessing the type of work that is being done by Nordic STS researchers. The article provides an overview of STS-related institutions and activities in the Nordic countries, and discusses the diffusion and diffraction of STS theory across national and institutional barriers.

Keywords: STS Theory, Nordicness, institutionalisation, stedegenhet

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Introduction

In April 2013, the first Nordic conference specifically oriented towards science and technology studies (STS) was arranged in Hell, Norway. The idea of the conference was to have a meeting place for people working in STS or on STS-related topics, a platform for discussion that was amenable to the growing community of STS in the Nordic countries. With more than 130 participants – a sizeable share of the active STS community in the Nordic countries – it attests to the strengthening position of the field of STS, a field which may not be as institutionally strong anywhere else than in just this region. With room made for national meetings of the constituent STS networks in the various countries, the conference seemed to confirm that there was need for a further integration of STS research between the Nordic communities, as well as a need for a place where young scholars could present their projects and research and where established scholars could convene to network and plan projects, all within a setting which was not overwhelmingly large.

At the conference, a panel debate was arranged to discuss if such a thing as 'Nordic STS' could be said to exist, and what it might even be. The four panel participants, one from each Nordic country present except Iceland, were reluctant to define what Nordic STS could be or constitute. At most, it might consist of a set of research priorities, tied to the specific historical and political context of the Nordic countries – for example, the largely public system for care for the elderly has led to investigations into so-called 'welfare technologies' in Denmark. The sentiment seemed to be that it makes little sense to try and distinguish STS in these countries from what is going on in other places, since STS is in its nature a global and cross-national field of inquiry. The theories and to some extent the empirical investigations travel between countries and look more or less the same everywhere. At most, Nordic STS can be summed up as a sort of communality grounded in shared research interests and a mostly shared language base. This view is echoed by Sheila Jasanoff, who in a talk given at the University of Oslo in September 2012 entitled "A field in spite of itself" discussed various ways of conceptualizing cross-disciplinarity.[1] In contrast to the more standard way of looking at disciplines – as territories separated by clearly demarcated borders closely guarded by jealous gatekeepers – it might be better to see them as islands in a large sea, with the uncharted territories between the islands representing the space for interdisciplinary explorations. In her version of the story, STS researchers can be likened to seafarers, charting the waters between established disciplines and establishing new connections for the exchange of knowledge.

What we wish to do here is to investigate this claim a little closer. Not because we think the metaphor of intrepid disciplinary Argonauts is necessarily wrong (although it is perhaps more of an ideal to strive towards than an accurate description of today's STS field), but because it opens up for some interesting avenues of investigation, of which we will mention two. Firstly, there is reason to ask whether STS as a field can be said to be uniform, and even whether this is something to strive for. One argument against this could be based on STS-theory. Many of the STS-theories utilized across the world have significant things in common and make it possible for STS-scholars to understand each other even if they are studying vastly different contexts. As many of these theories say, however, there can be no doubt that travelling theories and perspectives have to be appropriated, integrated and domesticated into local context. Secondly, the consolidation of a Nordic STS community is in itself a reflection of a specific institutional context tied to a set of priorities dictated by the needs of the funders of social research in the Nordic countries – mostly the nation-states of Denmark, Finland, Norway and Sweden – which again ties into notions of shared history and an attempt to stake out a third course between the two dominating post-WWII political blocks.

This consolidation, most recently demonstrated through the establishing of a new pan-Nordic network of STS research and a biannual conference of research done by Nordic scholars, also marks the demand for a platform for the further dissemination of this work. This is where a journal for Nordic STS research can find its *raison d'etre*, both as a home for empirical investigations that might not be deemed of general enough importance for the larger STS publishing channels and as a window in from outside showcasing the academic work done in the region.

In this article, we want to point to the aspects of STS work done in the Nordic region that could justify using a term that encompasses five countries and hence five different institutional settings. We believe that many of the concepts that have been developed within the STS discipline since its inception, such as the notion of translation and intermediaries (Callon, 1986), the existence of parallel types of expertise (Collins & Evans, 2008) and coproduction (Jasanoff, 2004) to name just a few, are exactly the approaches that allow us to analyze critically the movement of these very theories. In the following sections, we will discuss how both theory and empirical work can be understood as arising within specific institutional contexts. Then we see how such an understanding can shed light on developments by examining the types of STS research done in Nordic institutions. Finally, we point to ways this work can be made relevant to the larger, global STS context, both by providing interesting empirical examples and by highlighting the flexibility and adaptability that current STS thought allows for.

1 The talk can be heard here: http://www.uio.no/forskning/tverrfak/kultrans/aktuelt/konferanser/demarcations/program/jasanoff-lecture-edited-full-version.mp3

The shaping of theory

The question of theory in relation to a place-specific instance of STS can be seen from two vistas, roughly corresponding to the difference between theoretical and empirical investigations. On the one hand, there is the question of the *form* of theory that can be adopted or developed in any given context, meaning the ways in which the theory is enabled or constrained by external factors. To give an example: to what degree do the politics of research funding – the priorities of the funders – dictate the types of scholarly inquiry that are considered legitimate answers to specific societal needs? And in what way does the historical shaping of political institutions, for example an expansive welfare state or the specifics of a perhaps surprisingly successful post-war class compromise, give rise to specific theoretical perspectives that arouses the interest of researchers? We are thinking here of theories employed by STS scholars working in Nordic countries and adapted to Nordic empirical realities: what perspectives have been taken up, and how? On the other hand, there are the various *topics* embedded in the empirical questions that Nordic scholars have focused on. Given that the Nordic setting for politics, scientific focus, technological development and many other fields is empirically different from the European continental or American ones, there are other types of factual investigations that can be made. Here, we will discuss how differences in institutional organization have given Nordic STS research a slightly different focus from many of its continental or Anglo-Saxon peers.

Before that, some words about the word 'Nordic'. It is potentially a problematic one. It presupposes a shared affinity between people working in different institutional contexts and languages, solely based on a politically/culturally constructed idea of similarity. To insist that there is something more than that, a sort of quintessential 'Nordicness' which influences all Nordic STS scholars and somehow sets us apart from others, carries with it certain questionable implications, not least what this entails for the communication between regions and the movement of scholars across institutional borders. However, it is a fact that there is a large degree of cooperation within the Nordic region, with both strong historical precedence and political basis. To the degree that we are only dealing with professional cooperation and a tendency to talk to each other about what we are doing, we do not see a big problem in talking about Nordic STS as a sort of entity in itself.

One reason for choosing to stay with the notion of Nordic STS, would be to discuss what happens as theory travels into our regional and national contexts. Even though they are often treated as such, theoretical concepts do not enter the world from a vacuum, but rather arise out of a particular time, place and institutional setting. So, concepts used by Nordic STS-scholars can be expected to be shaped by the particular institutional context in the Nordic countries in one way or another. This might be a somewhat banal statement, but as Nordic STS seems to be in a process of increased institutionalization, it's important to remain sensitive to how contexts shape our readings and uses of theory.

We can take Jasanoff's work on the American justice system as an example (Jasanoff, 1995), and compare it with Bruno Latour's on the French (Latour, 2010). Jasanoff studies the making of law through the lens of scientific evidence and the appeals of lawyers and judges to scientific facts in order to produce 'truth', all with clear political implications. Already on the very first page Jasanoff refers to the 'distinctive flavor' of the tendency in American politics to resolve social conflicts by means of the legal system. Latour deals with the way legal authority in the French system relies on a complex mesh of historical precedence embedded in the very make-up of its supreme court, "entirely fabricated, over two centuries, by the judges themselves", as he writes in the preface. It is not that the American system could not have been analysed using Latour's reference points or vice versa, but rather that it would look different due to the different context in which French and American law have been produced in and produces. The legal system of the Declaration of Independence does not operate in the same way as that of Napoleon and the *Conseil d'Etat*.

Another, similar example of how differing contexts can play into our theorization in the field of STS can be drawn from the fact that Nordic countries are often portrayed as the perfect example of how a sustainable modern market economy can be produced and maintained – the so-called Nordic Model (Andersen et al., 2007; Christiansen, 2006) which seeks to limit the purview of markets in favour of an extensive social security net and has been held up as an alternative for reform in countries like the United States (Jantti et al., 2006). However, it would be false to pretend that the market is the same thing in the US, France and the state-dominated economies of the Nordic countries. When the state is a major player in most spheres of the economy – owner of some of the largest companies in most sectors, partner in annual wage negotiations vis-à-vis the private sector, provider of health care, arbiter of gender relations, to name a few – could this not mean that the analysis of the economy, labour relations, consumer patterns, must look different too?

Bruno Latour once wrote "Give me a laboratory and I will raise the world" (Latour, 1983). This was during the height of laboratory studies, before STSers started following lawyers, bureaucrats and politicians through society. However, STS has shown us how the specific meetings between sectors transform the world, and our theories about the world, in unexpected ways. This insight should of course be brought into a discussion of meetings between different flavours of STS – reflexivity is, after all, part of the DNA of the history of STS (Wynne, 2007). Could it not be that the development, introduction and domestication of central STS theories are reliant on the institutional arrangements of the contexts where these theories were produced, and that this reliance can in turn end up reflecting very specific notions of how society or politics should work, and hence, how research is done? Looking at exactly how a field is institutionally composed and re-composed could also reveal something about these notions.

The world we study

In his history of the rise of STS in the UK, John Law (2008) traces the origins of the field to 1960s sociologists with a special interest in technology, noting that the field of science studies branched off from mainstream sociology during the 1970s, taking up impulses from anthropology, geography, history of science and philosophy of science. In his telling, STS is a happy amalgam of other disciplines which seems to be a way forward for the social sciences. He also stresses the strength of 'arguing through case studies', which can be seen as an antidote against sweeping generalisations. The importance of case studies is also recognized by Peter Dear and Sheila Jasanoff in an essay discussing the relation between STS and the history of science. Here, they claim that STS is more an object centered field of study than a discipline in the narrow sense (Dear & Jasanoff, 2010), meaning it consists more of a set of perspectives that can be mobilised in the analysis of a given phenomenon, for example through metaphors of networks, controversies or materiality, than a prescribed sequence of steps to follow. This is a reasoning common for other interdisciplinary fields such as visual culture studies, social geography or gender studies as well, all of which share a certain affinity with STS.

The distinction between a field and discipline can be useful for thinking about the meanings and implications of the term 'Nordic STS'. If STS is an interdisciplinary, object (or case) centered field, then the objects approached must be firmly placed in context. Thus, while the objects of STS might very well be global or universal, they are also inherently local. If cars are shaped by its cultural, political and economic environments, then there is all the reason to insist on that the history of the car in Norway followed quite different lines than that of the American car (Østby, 1995). This object-centeredness might again be a fruitful entrance to the question of empirical studies. In the following section, we give a brief overview of some of the types of empirical research that have been undertaken in a Nordic STS setting. Could it be that the types of large institutions that are prevalent in the Nordic countries play a distinct role in the types of topics that have been and continue to be discussed within Nordic STS? That the welfare state does not just provide the type of state-sponsored support which makes so many outside the Nordic countries somewhat skeptical, but also provides STS scholars with a wealth of interesting subject matter for further study?

One example can be drawn from the debate at Hell, where Kristin Asdal used the term 'science-state nexus' when pointing to an important difference between STS in a Nordic context and the U.S. Where much American STS-research has centered on the interface between science and industry, much Nordic STS-research centers on the crossroad between science and the state.[2] The Nordic welfare states are highly involved in the shaping of both scientific research and technological development, and the involvement seem to take different shapes as they both initiate, fund, shape and respond to much of the research being conducted. There are however significant differences between the Nordic countries. Whereas Sweden has large industrial funds going into research, the private funds available to researchers in Norway are microscopic compared to state funds (Skoie, 2005). In addition, there was a significant build-up of state ownership in Norwegian industry after WWII, a trend that seemed to strengthen as Norway struck oil (Sejersted, 2005).

This acknowledgement of the importance of contextualizing our objects of study and the contexts they change prompts a further question: Are there local flavors to STS-research in the various centers and departments in the Nordic countries? In order to give a brief sample of research going on in the Nordic countries, and given that Nordic STS research is highly institutionalized, it is reasonable to use some of the dedicated institutions as a point of departure.[3]

Nordic institutions

Starting with Sweden, we see that according to Aant Elzinga, Swedish STS grew out of a discussion about research policy in the 1960s and that centers investigating questions related to science, technology and society was established in the 1970s. He identifies Lund and Gothenburg as universities that had significant groups. However, writing in the 1980s, Elzinga concluded that in Sweden, policymakers and bureaucrats showed little interest in the field (Elzinga, 1980). Since Elzinga wrote his article in 1980, this seems to have changed, and the Swedish STS-community has grown considerably. The Center for Science and Technology Studies in Uppsala has become a hub for associates from a range of departments and disciplines and focus on two broad research programs: Science, technology and business, and science, technology and research policy, respectively. Several of the projects focus on the sectorialisation of research and the role of the university in the new innovation economy. At Sweden's Royal Institute of Technology's Department of History and Philosophy of Technology, the research is more focused on technology and infrastructures. Thematically the research includes energy systems, technological systems and European integration, ICTs, and the infrastructures of arctic knowledge.[5] At the University of Linköping, the Department of Thematic Studies contains the research unit for technology and

2. She seems to play on the work of Creager et al. (2004), but the literature on the triple helix of science, state and industry also spring to mind (Etzkowitz & Leydesdorff, 2000).
3. This does not imply that these are the only places where interesting research is conducted. Neither will the overview consider all the research conducted in every STS-institution in the Nordic countries as even these have outgrown the scope of this article. Any attempt to describe Nordic STS, must therefore be considered a taster rather than a full meal. Even so, we will attempt to do just that hoping that those disagreeing with our description will vent this in future opinion pieces in this journal.
4, http://www.sts.uu.se
5 http://www.kth.se/en/abe/om-skolan/organisation/

social change. The research at the unit focuses on energy infrastructures, as well as environmental and medical STS and everyday life in past and present societies.[6]

The Danish organization of the STS-field is somewhat different from the Swedish. First of all, the Danes have established a national association for science and technology studies. In keeping with the science-state nexus, the Danish association for science and technology studies (DASTS) grew out of a research council initiated network for the history and philosophy of science that was established in 1994. DASTS was established to stimulate quality, breadth and cooperation within the STS-community in Denmark and to promote Danish STS in the national and international community. DASTS has about 400 members from a range of disciplines and academic institutions and tie these together with the help of a mailing list, the electronic newsletter "Hugin and Munin" and the journal *Encounters*.[7] That STS in Denmark is institutionalized through a national association may signal that in Denmark there are rather few dedicated STS departments or centers. The Center for Medical Science and Technology Studies and the Center for Science, Technology and Society at Aarhus University are two examples of the opposite. Still, many Danish STS-groups seem to be situated within thematically oriented research groups, centers or departments rather than field demarcated institutions. This makes the STS-scene quite diverse, but at the only dedicated centers medical STS is prevalent. Other important research themes for the Danish community are sustainable transitions, media and innovation.[8] Compared to the Swedish case, historical STS seems to have a weaker standing than anthropological and sociological perspectives in Denmark, which is illustrated by the newly established research group "Technoanthropology" at Aalborg University.[9]

Finland is the other Nordic country where a national organization exists to gather all the animals of the STS-forest. The Society for Science and Technology Studies was established in 1985 and gathers just over one hundred members.[10] Finland is also home to one of the larger STS journals in Europe, Science and Technology Studies, now the official journal of the European Association for the Study of Science and Technology (EASST).[11] Furthermore, Finland houses the Research Center for Knowledge, Science, Technology and Innovation studies at the University of Tampere. This center does research ranging from the politics of knowledge, institutions and research community, via technology and everyday life and to the study of innovation systems.[12] As in the other Nordic countries, there are significant STS groups situated in other institutions than the dedicated STS-centers, and many of the groups are very active in educating PhD-students and conducting research.[13] Also, the University of Helsinki and Aalto University has established the network unit Helsinki Institute of Science and Technology Studies (HIST). This institute is to strengthen the research and education and the institutional basis for Finnish STS. Research at this institute includes research on green economies, innovation, risk governance, nanotechnologies and climate policy.[14]

The Center for Technology, Innovation and Culture (TIK) is one of three established Norwegian STS research centers. As the other STS-institutions in Norway, this center was established in the 1980s in the aftermath of discussions about the social consequences of new science and technology. TIK has two main foci of research: Innovation studies and science, technology and culture. Whereas the first group is oriented towards the study of innovation systems, the latter approaches science and technology with a focus on policy and politics. Of research topics, we can mention that TIK-researchers are investigating the politics of nature, risk, expertise and consumption.[15] The sibling STS-institution of TIK, is the Center for Technology and Society (CTS) located at the Department of Interdisciplinary Studies of Culture in Trondheim. Here, the focus of research tends to be on STS related to ICTs, energy and climate change, biopolitics and consumption.[16] In addition, TIK and CTS cooperate through the Center for Sustainable Energy Studies.[17] In Bergen, you find the Center for the Study of the Sciences and Humanities (CSSH). They focus on philosophy and theory of science and research topics include ethics of science and technology, uncertainty and risk as well as the evaluation of the social impact of science and technology.[18]

So far, Iceland does not have an established STS institution in itself, but there are clusters of related work being done, for example in the Center for Equality, Diversity, Development and Advancement (EDDA)[19] at the University of Iceland, which does work on questions of sustainability, citizenship and transition theories among others.

A place of one's own

So far a quick look at STS institutions in the Nordic countries. What can we make of this? First of all, it's clear that many of the scholars identifying as working within STS are not situated at dedicated STS-departments or centers. Some are located at disciplinary units such as departments for sociology, history, anthropology, while others are working in what is termed the institute sector. However, this is more or less the *modus operandi* for STS all over the world. As stated in the introduction of this article, Nordic STS does enjoy a high degree of institutionalization and the various institutions do have

6 http://www.tema.liu.se/tema-t/forskning_t?l=sv
7 http://www.dasts.dk
8 http://www.dasts.dk/?page_id=23
9 http://vbn.aau.dk/da/organisations/pp_5a5ba97a-6f42-47c2-827d-226202ed66f8.html
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19 https://edda.hi.is/

a different flavor of STS-research. This is perhaps best illustrated by the strong position enjoyed by historical STS in Sweden and the strong position of anthropology in Danish STS. Also, Norwegian STS communities can be said to have an interest in the integration of science and technology in common, while the Swedes seems to be more geared towards infrastructures and institutions. This might have historical reasons that involve the shape of the R&D-systems in Norway and Sweden, but also the structures of economic life: Where Norwegian economic life centers on raw materials and have imported most of its technologies outside the specific petroleum related ones, the Swedes have a strong industrial-innovation legacy.

In line with Jasanoff's injunction to act as explorers between and within fields, we have tried here to give a very rough sketch of what STS in the Nordic countries might entail. Of course, providing a more detailed picture will require more extensive work and space than available here, as the STS-jungle in the Nordic countries has grown so wild that mapping it completely would be almost impossible.[20] However, the ways in which Nordic STS researchers have pursued research into the "science-state" nexus might give us some insight into the questions posed in our initial discussion of the ways a Nordic STS might merit attention. Nordic STS has for the most part relied on using concepts from the general, international STS literature, even though exceptions exist, for example the work done on domestication theory (Williams & Sørensen, 2002). Still, the process of translation and domestication has resulted in a highly diverse field, which speaks to the general usability and malleability of theory. In light of this, we have tried to show how STS in the Nordic countries both carries on work within a clear tradition, and gives that tradition as it is expressed in those countries a flavor of its own.

A way to talk about this flavor might have been through the concept of cultural de- and re-territorialisation, which posits a relation between culture[21] and geographical and social territory (Canclini, 1995). Could the same hold for theory? One problem is that deterritorialisation mostly applies to situations of the margin. While it is true that the Nordic region is not the most central in the world, it would be disingenuous to make use of the language of marginalization from a position which is so clearly privileged as that of scholars in some of the richest countries in the world. There is, however, a perfectly usable Norwegian word for what we are trying to convey, which should be legible for at least Swedes and Danes as well: Stedegenhet – literally, "place-ownness". Stedegenhet points to the way a place-specific setting might influence the way a question is asked or an investigation undertaken, all the while avoiding the somewhat uncomfortable connotations of a concept like *terroir*, which carries with it a sense of essentialism. Stedegenhet also has the fortuitous feature of having a double meaning – egen meaning both "own" and "stubborn" – correlating to the partly independent and outsider status of Europe's northern extremes.[22] When considering that every place, *sted*, is in constant development, it should be obvious that it's not clear what Nordic STS is. What we need is a constant discussion of what it might become over and over again.

Conclusion

As stated initially, one impetus for this text is a slight unease with the way STS theory is often presumed to be global, with universal concepts applicable to all sorts of different contexts. The question is: how should we deal with this unease? Our asking this question might make it seem that we think STS is in some sort of crisis. This is not our position at all (indeed, if this was the case, why launch an STS journal in the first place?). To the contrary, we believe that it is precisely because STS is doing so well both theoretically and institutionally that it is time to ask these types of questions. We see in STS the potential for a cosmopolitical type of theory,[23] one that disseminates across borders, languages and epistemic cultures and simultaneously morphs and incorporates local impulses. The analogy of a rhizome might seem a tired one, but if there is one type of thinking that has the ability to move and grow rhizomatically, it has to be STS, a point already made in a discussion of Norwegian STS (Sørensen, 2012). Drawing on this, we see many possibilities for Nordic STS to spread out in the future, while still exhibiting some of that strange stedegenhet which ensures that just this work will not be done anywhere else in the world, at least not in exactly the same way and form. We also believe that the examples we provided earlier of how STS research has been conducted in the Nordic countries demonstrate that this potential always has been present in the discipline. This can be nothing but a strength. After all, why is science and technology studies oriented research coupled with gender studies in Trondheim yet located in a business school in Copenhagen? Why do STS scholars combine so well with history in Sweden but move in the field of innovation studies in Finland? Don't these examples demonstrate that STS is uniquely capable of handling the interdisciplinary challenges of modern social research? We think so, and welcome the opportunity to contribute to the continued messy growth of the roots and shoots that stem from what Donna Haraway has called the 'fertile compost pile' of science and technology studies.

20 As we see it, this is in itself a good reason for establishing a Nordic STS journal.
21 In our case epistemic cultures (Knorr-Cetina, 2007), although that concept doesn't capture place as much as institutional culture.
22 Here we are conveniently skipping over the fact that the Nordic countries remain some of the least self-sufficient and most interconnected countries on Earth.
23 As suggested by Stengers and Bononno (2011)

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Acknowledgements
Both authors are funded by the Norwegian Research Council. We are grateful to our two reviewers for useful comments.

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NORDIC JOURNAL
of Science and Technology Studies

BEYOND INNOVATION

Towards an extended framework for analysing technology policy

by Knut H. Sørensen

This paper analyses technology policy as a scholarly concern and political practice that needs to be taken beyond the present somewhat singular focus on innovation and deployment. We also need to include an interest in the making of infrastructure, the provision of regulations, and democratic engagement. Consequently, this paper introduces the concepts of socialisation and domestication to overcome the instrumental, economic framing of technology policy. These concepts highlight the importance of embedding and enacting new technology. The suggested conceptual framework is used in a brief synthetic analysis of four examples of technology policy and technological development in the Norwegian context: cars, wind power, hydrogen for transport, and carbon capture and storage (CCS).

Keywords: Technology policy, innovation, deployment, socialisation, domestication

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Introduction: what is technology policy?

Technology plays a prominent role in many kinds of discourses concerned with improving human conditions and the political management of challenges like global warming, sustainability and employment. In particular, this is expressed through widespread use of concepts like 'innovation' and 'knowledge-based society', which form the basis of much of today's public policies and governance. Arguably, the development of technology has become a sublime that focuses the hope for a better future in a particular manner. This paper is concerned with how we may conceptualise the scope of policy issues involved in pursuing technological development as a way of improving modern societies. Presently, many scholars agree about the need to supersede the present dominance of a fairly singular focus on technological innovation for economic growth, albeit for different reasons, like the need for sustainable transitions (Schot and Geels 2008, Steward 2012), the impact of non-technological regulations (Paraskevopulo 2012), concerns for the role of activists (Hess 2007), the need to include broader political economy perspectives (Tyfield 2012), or the importance of pursuing public engagement and perceptions of risk (Felt et al. 2007).

In early science and technology studies (STS), the analysis of science and technology policy was a main concern (Spiegel-Rösing and Price 1977). However, the main focus of these efforts was science-government relations centred on R&D, in particular the analysis of how social interests shaped such policies (Cozzens and Woodhouse 1995, Elzinga and Jamison 1995). While these are important issues, this paper moves in a different direction. Rather than emphasising the role of science policy as an articulation of social interests and power to influence innovation, I want to pursue what may be considered "downstream" issues arising from efforts to integrate technologies in society. Thus, the intention is to complement the efforts of broadening science and technology or innovation policy analysis by developing an inclusive concept of technology policy. This concept should help providing a comprehensive agenda with respect to what the analysis of policy-making with respect to technology may involve.

As a scholarly term, technology policy is not widely used in the social sciences, including policy analysis. The concept is not common in public political discourses either. For example, using 'technology policy' (in Norwegian: 'teknologipolitikk') to search Norwegian news media through the comprehensive database Retriever, we find that the term is rare – in striking contrast to 'science policy' or 'innovation policy'. Maybe 'technology policy' triggers unpopular images of governmental planning and thus runs counter to the present dominance of neoliberalism and the belief in the all-powerful market? Or is it that the concept does not fit the heralded visions of globalisation since it seems to refer to the nation state?

What should we mean by 'technology policy'? Lewis M. Branscomb (1993:3) provides the following definition:

> A technology is the aggregation of capabilities, facilities, skills, knowledge, and organization required to successfully create a useful service or product. Technology policy concerns the public means for nurturing those capabilities and optimizing their applications in the service of national goals and the public interest [...]. Technology policy must include not only science policy ... but also all other elements of the innovation process, including design, development, and manufacturing, and the infrastructure, organization, and human resources on which they depend.

In a similar vein, Charles Edquist (1994:68) defines technology policy as "all public intervention in the process of technical change. More specifically technology policy is implemented by a number of public policy-making bodies that use specific instruments to influence the process of technical change". Further, Edquist makes a distinction between direct and indirect technology policy. The first is expressly intended to influence technical change, while the latter includes policies that are not primarily designed to shape technical change, but still have such effects. This includes trade policies, military policy and industrial policy.

Thus, supposedly, technology policy is a comprehensive scholarly concern, but how comprehensive in practice? Both Branscomb and Edquist frame technology policy as primarily an economic issue. Branscomb (ibid.) states that: "Technologies are created for economic reasons and the investments they call for must be economically justified". Edquist (p. 70) claims that: "The most important goal of (civilian) technology policy is in practice increased productivity growth and competiveness". This suggests a limited and fairly instrumental interpretation of technology policy as mainly science and/or innovation policy to serve economic interests.

A twist on such an interpretation is found in Mowery et al. (2010). They propose developing a technology policy approach aimed at managing the threat of global climate change. Mowery et al. argue the need for a large-scale, concerted effort to develop and deploy energy technologies that can be tools for climate change mitigation, and they criticise suggestions that such efforts can modelled after the Manhattan project or the Apollo programme. They call their alternative a R&D support programme, making R&D the core of the effort of climate change mitigation. The problem of deploying technologies for sustainable energy is mainly conceived as a challenge for governments to stimulate the demand for such technologies. Again, we observe the dominance of an economic framing, even if Mowery et al.'s main concern is global warming.

If we turn to STS, we find a number of studies that are relevant to the understanding of technology policy, like work on standards (Bowker and Star 1999), genetics (Wright 1994, Jasanoff 2005) or gender (Sørensen, Faulkner and Rommes 2011, Wajcman 2004). Arguably, the co-production approach of STS (Jasanoff 2004) could be useful, for example by considering technology policy as a co-production of technology and policy or of development and deployment. However, the concept of technology policy is usually not part of these scholarly contributions.

Some efforts have been made to provide a more policy-oriented version of STS (Sørensen & Williams 2002, Hommels et al. 2007, Raven et al. 2009). One way of doing this is to extend the concept of technology policy to be more concerned with downstream issues, like use or domestication of technology (Sørensen 2002a). Sørensen (2002b) suggests that studies of technology policy should have four main concerns to transcend the dominant economic framing and focus on R&D: (1) Support for innovation, (2) The provision of infrastructure, (3) Regulation, and (4) Public engagement.

This paper will use the latter effort as a stepping stone to develop a framework for conceptualising and analysing technology policy. In doing so, there is a need to be reflexive with respect to the relationship between technology policy as an analytic and as a normative concept. Since we find relatively few instances where policy-making efforts are characterised by the practitioners as technology policy, relevant efforts have to be re-assembled (Latour 2005). Scholarly contributions have to be treated in the same manner. Using 'technology policy' as a generic term for issues of governance with respect to technology and technological development is meant to emphasise the need to study such governance as a set of possibly interrelated activities. This is intended to provide analytic benefit but it is also a normative stance in the sense of an implied critique of policy-making efforts that appears to be split up or are rendered invisible.

As suggested above, technology policy issues related to research and innovation have been fairly thoroughly researched. This is above all true with respect to the literature on innovation systems (Archibugi and Lundvall 2001) but also through the concept of triple helix (Etzkowitz 2008). To go beyond innovation-centred perspectives, this paper starts by moving downstream to consider what Mowery et al. call deployment issues, the rate of adoption of given technologies. I argue that from an STS perspective deployment is closely related to the processes of socialisation and domestication of technology, and thus to sense-making and use. However, as we shall see this is not a one way trip, but rather involves complex navigation upstream, downstream and sideways. The next section introduces some relevant theoretical perspectives that may help in the navigation. Then, I turn to some empirical examples mostly related to sustainable energy to demonstrate potential achievements from drawing on an inclusive concept of technology policy.

Technology in use: deployed or domesticated?

It is a truism that demand plays a crucial role in successful innovations. This is considered to be related to understanding user needs as well as user experiences and the related processes of learning (Andersen and Lundvall 1988). Kline and Rosenberg (1986) introduced the chain-linked model to transcend linear understandings of innovation by emphasising how knowledge and information moved through a variety of chains involving a diversity of actors. Lundvall (1988) proposed an interactive learning model, where innovations were shaped by producer-user interactions. The more recent national system of innovation literature integrates these and supplementary perspectives (Lundvall et al. 2002, Fagerberg and Sapprasert 2011) as do triple helix-oriented research (Etzkowitz 2008). Still, the innovating company or organisation is at the centre of attention, in some ways similar to classic actor-network theory's understanding of translation as being performed by entrepreneurial scientists or engineers (Latour 1987).

The concept of deployment transcends this focus through the acknowledgement of the need for policy actions to bring new technologies into use. Deployment policies are concerned with changing the premises of demand as well as users' engagement with given technologies, rather than with analysing consumption and use. Such policies may of course affect innovation efforts, for example by leading to increasing investments in innovation (Hoppmann et al. 2013), but that is not the prime target. The main aim is getting new or existing but underutilised technologies in place so that they can contribute to, for example, production of energy without emissions of CO_2.

Müller et al. (2011) perceive this aim above all as a need to remove barriers of deployment. They classify such barriers in the following way (p. 32-33):

1) Techno-economic barriers related to relative costs compared to competing technologies.
2) Non-economic barriers that related to factors preventing deployment or increasing costs
    - o Regulatory and policy uncertainties
    - o Institutional and administrative barriers
    - o Market barriers, for example inconsistent pricing structures
    - o Financial barriers
    - o Infrastructure barriers
    - o Lack of awareness and skilled personnel
    - o Public acceptance and environmental barriers.

This list of barriers covers a broad spectrum of technology policy issues, which makes the thinking with respect to deployment pretty comprehensive. Nevertheless, there is an ontological problem in the identification of deployment and barriers. The concern of Müller et al. seems to be to identify and remedy features that may curb the diffusion of renewable technologies through the stages of initiation, take-off and consolidation (see for example p. 50-51). The resulting frame of interpretation largely black-boxes technology through the use of quantitative indicators. Deployment is measured by counting the number of installations, energy production, investment levels, etc. Thus, the concept becomes predominantly economic with a singular focus on market competition. The actual dynamics of the appropriation processes are overlooked, like what happens when new technologies are moved into "the real world", where the concern for demand might be extended into a concern about use. In a sense, the deployment perspective also black-boxes demand by making it into an issue only of accounting, overlooking the potentially dynamic and reinforcing effects of creative use.

As already suggested, an alternative to the fairly instrumental deployment thinking is to be concerned with processes of appropriation of technology – the ways in which technologies embedded in society, and how technologies are affected by the processes of embedding, including cycles of embedding, dis-embedding and re-embedding (Giddens 1990). This would be in line with basic tenets of STS. How may we theorise such processes of technological change, focusing on use and the ways in which a diversity of publics engage with new technologies?

STS offers a host of overlapping possibilities. In the light of the focus on R&D, so prevalent in technology policy studies, an interesting proposal is to study the socialisation of scientific and technological research (Bijker and d'Andrea 2009). This could mean reframing the policy issues related to innovation and deployment as a need also to develop specific socialisation policies to provide what Mowery et al. (2010) call R&D support programmes. Actually, the socialisation perspective goes further in its insistence that the embedding of new technologies potentially implies a very comprehensive set of tasks, distributed over many areas.

Bijker and d'Andrea identify six such socialisation areas: (1) scientific practices, (2) scientific mediation, (3) scientific communication, (4) evaluation, (5) governance, and (6) innovation. Consequently, potentially, there are a manifold of agents of socialisation, which should be found in scientific institutions, NGOs, government agencies, etc. The problem is, according to Bijker and d'Andrea, that the work of socialisation is not done: "(I)n Europe, the "agents of socialisation" seem to be few; they often work in hostile environment, where resistance and hindrances limit the "systemic" impact of their action; the degree of acknowledgement that they receive from public institutions varies country by country, but overall it appears to be limited; they prevalently act in an "atomised" way, or create short and scarcely visible operation chains" (ibid, p. 22-23, emphasis in the original).

Compared to the deployment perspective with an ontology characterised by an economic framing and a focus on barriers, the socialisation approach as outlined by Bijker and d'Andrea is broader and more concerned with the potential for facilitation of societies' and social communities' appropriation of science and technology. Their concept of 'agents of socialisation' is helpful in identifying who should be expected to do the work of bringing science and technology out of scientific institutions and into use.

Of course, the idea that scientific and technological research or technology needs to be socialised is a basic STS tenet. Technologies only exist as sociotechnical entities. They are developed through reflections about achievements and use, including commercial intentions. As Latour (2005) reminds us, there is a lot of work by a diversity of actors involved in the translation efforts through which new embedded technologies emerge. Thus, actually, much socialisation is and has to be done. However, this work as well as the technologies involved are often rendered invisible and forgotten (Winner 1977). This means that the efforts of the agents of socialisation are easily overlooked. Bijker and d'Andrea are correct in their call for more and improved socialisation efforts. Still, if we are aware of the lack of visibility of the efforts of agents of socialisation, we may be able to observe more of it. This is important when we are concerned about the potential scope of technology policy.

We should also recognise that non-human actors too may be important agents of socialisation. While we may discuss how we should understand the ways in which humans and non-humans interact (Pinch 2012), we should not overlook the importance of infrastructure in shaping and facilitating the shaping as well as embedding of new technologies, including how new technologies are interpreted (Bowker and Star 1999). For example, fuel-cell cars will not be socialised without a network of hydrogen filling stations, which facilitate the practice of refuelling hydrogen as well as signifying that fuel-cell cars are a viable alternative to petrol-powered cars. Equally important are regulations, which set standards and provide risk management that are vital socialisation efforts. Thus, we need to multiply the number of socialisation areas that Bijker and d'Andrea identify.

To summarise, the paper has argued an extended conceptualisation of technology policy to include concerns about socialisation, together with innovation and deployment, as well as the interaction of these sets of activities. However, we need to explore the processes through which new technologies are embedded in society; how they may be enacted and made sense of by users. This concern points towards domestication theory as an approach to study such enactments and sense-making (Sørensen 2006). Domestication takes place in many areas and involves a multitude of actors. It results in practices with regard to use, provides meaning to the technology in question, and depends on users managing cognitive challenges related to learning and understanding the technology. Some technologies are domesticated swiftly across a broad

spectrum of the population, while other technologies become domesticated slowly and/or by small communities, and some technologies are not domesticated at all. Arguably, socialisation efforts should help technologies, or scientific knowledge for that matter, become domesticated. Domestication of a given technology means that it has been deployed, but the observation that the technology has been deployed tells us nothing about sense-making and the development of practices. To get such knowledge, we need to study the actual process of domestication.

This means that the study of domestication provides measures from which we may assess innovation, deployment and socialisation. With respect to innovation, the understanding of user needs is vital. Technologies have to be domesticated to be considered employed, and domestication failures may indicate socialisation flaws. However, these relationships may be contested, competitive and filled with conflict. Technology policy is a field of articulation of interests and thus of controversy. Thus, it has to be approached with this in mind. There may be good reasons that some technologies do not become deployed, socialised and/or domesticated, and anti-deployment and anti-socialisation strategies may be acceptable, even fruitful, for a host of reasons.

In a concept of technology policy concerned with innovation, deployment, socialisation and domestication, it is important to note that in relation to new technologies the public may play a complex of roles, as consumers, citizens and users. Technology policy may address these roles more or less explicitly, depending on scope and focus. If we are to improve our understanding of technology policy, we need to study how the different roles are catered for – if at all.

Let us briefly consider some examples. Today, nearly everyone in Norway is familiar with SMS (short message service), which is an integral part of mobile telephones and developed as part of the telecommunication standard called GSM (General System for Mobile Communication). The domestication of SMS happened incredibly swift through young mobile phone users who discovered this application as a cheap, quick and handy way of communicating with each other. The emergent practices, including shorthand and symbols, were produced by the collective of users in a distributed fashion where nobody credibly may claim intellectual property rights. This collective of users socialised SMS without any policy effort outside standard regulation of mobile telecommunication. In this case, technology policy with respect to mobile telecommunication did not really address any of the three potential roles of the public.

This may be contrasted to electric vehicles, where current technology policy in Norway includes comprehensive socialisation efforts to make such vehicles attractive as well as to facilitate an interpretation of them as environmentally and climate friendly (Ryghaug and Toftaker forthcoming). In this way, the population is addressed as citizens (to understand and accept the special conditions provided to electric cars), as consumers (making electric cars attractive) and as users (providing meaning to as well as some suggestions about the use of electric cars).

An interesting example of a non-embedded technology in Norway is nuclear power. Norway got its first atomic reactor in 1951 as the fifth country in the world. The reactor was primarily intended for research and experiments, and the director of the Institute for Atomic Energy (today, Institute for Energy Technology), Gunnar Randers, made a very substantial effort to socialise atomic energy (Randers 1975). However, Norway and Norwegians never domesticated nuclear power, and the Parliament eventually decided against the construction of nuclear power in Norway. Relatively speaking, no other technology has received as much funding as atomic energy in Norway, but as a technology policy object it became a failure because neither the practices involved in producing nuclear power nor the meaning attributed to the technology was considered attractive. The anti-socialisation efforts of the anti-nuclear movement (the public enacting the role of citizens) stopped innovation and deployment and thus made the roles of consumers and users unavailable.

These examples also nicely illustrate some consequences of domestication with regard to technology policy. In the case of SMS, a quick, successful domestication based on a distributed, collective user-driven socialisation effort, made any form of policy intervention superfluous. With electric cars, policy-makers saw a need for facilitating actions and launched an active technology policy for deployment and use, leaning on explicit socialisation efforts. Nuclear power exemplifies the potential role of conflict in rendering technology policy ineffective. The comprehensive socialisation efforts, in particular by the research community throughout the 1950s and 1960s failed when confronted with strong anti-socialisation actions. Thus, nuclear power did not lend itself to be domesticated by the general public or even by energy companies.

So far, this paper has provided an argument for analysing technology policy in a comprehensive manner by going beyond innovation and adding the issues of deployment, socialisation – including infrastructure and regulation – and domestication. Deployment should be considered because, often, policy efforts are made to get technologies employed. Socialisation is similarly important as a set of actors and activities that may or may not be mobilised in order to embed new technologies in society, while the analysis of domestication throws light on the effectiveness of policy achieving employment and embedding. Above all, socialisation efforts should be thought of as means to facilitate domestication.

In the next section, the aim is to demonstrate the potential of the proposed framework to analyse technology policy activities, with an emphasis on socialisation and domestication. We shall also see that such policy-making is complex, multi-sited and multi-actor. Such observations are not new to policy analysis, but this is still important to observe.

Exploring technology policies in a Norwegian context

In this section, I analyse four examples of development of technology in Norway from a technology policy perspective: (1) The appropriation of the car in the 19th and 20th century, (2) Wind power development, (3) The so-called Hydrogen Road as an experiment in supplying hydrogen for transport, and (4) The development of carbon capture and storage (CCS) technology. The choice of these examples is partly a pragmatic one; I have been involved in studying them. However, as hopefully will become clear, they display interesting diversities with respect to scope, aims, achievements, timing, and policy efforts. The analysis is synthetic and draws on printed sources to explore theoretical considerations. I do not present full-blown empirical accounts but try to demonstrate how the extended concept of technology policy brings forward observations that are more difficult to make with a singular economic focus on innovation and deployment. Thus, as a consequence, the paper highlights socialisation efforts. This is done by identifying areas, actors and strategies involved in the socialisation as well as considering domestication activities and their effects. First, we turn to a fairly long-term historical example, that of the motorcar in Norway.

The embedding of the car in Norway[1]

The appropriation of the car in Norway during the 19th and 20th century provides many lessons with respect to the role of socialisation in technology policy as well as regarding deployment and domestication. Also, it points to the possible problem of conflicting policy aims. Initially, the story of the introduction of the car in Norway was initially very much about development of regulation and provision of suitable infrastructure, neither of which really predated the automobile. The first legal term for a car was 'a rail-free vehicle', contrasting it to the railway. Thus, the making of non-human socialisation actors was critical and the main element of technology policy with respect to cars.

Initial regulations meant cars could only be considered to be a hobby for the wealthy, since the expensive vehicles were slow and not very comfortable, while the rules for driving them were very strict. This changed, and a main socialisation actor was the Directorate of Public Roads whose managing director Hans Hagerup Krag in the late 19th century publically demonstrated car driving and sent employees abroad to learn about making roads suitable for automobiles. Regulatory efforts were developed to become more accommodating; including the making of traffic rules as well a system for certification of drivers and vehicles. In combination with advertising efforts and newspaper coverage – done by socialisation actors outside of policy-making circles – this resulted in an extensive sense-making with respect to cars as well as the development of driver practices. Infrastructure was built to include petrol stations, car repair shops, car dealers, etc.

The result is that cars became a pervasive feature of modern Norwegian society with a comprehensive infrastructure as well as a diversity of car-related practices of individuals and communities. Policy-making activities related to provide regulations and infrastructure clearly were effective socialisation measures. This resulted in a widespread domestication of cars in Norway. For example, when Hans Hagerup Krag was the head of the Directorate of public roads, he could be seen as developing a policy to deploy cars in Norway. This effort was made above all by being a socialisation actor, which included removing the barrier of unsuitable roads by improving transport infrastructure. On the other hand, politicians were not too keen on a speedy deployment. Norway early began to tax cars and car-use relatively heavy. This was legitimized by labelling the car as luxury, as a relatively expensive and unnecessary artefact. Since Norway was (and is) without its own car industry, cars are imported and from an economic point of view, they are a negative item on the trade balance. Such considerations led to the introduction of import quotas on cars from 1945 to 1960. During this period, those who wanted to buy a private car had to apply for an import license, and such licences were granted on the basis of assumed needs. This favoured people who could argue that they needed a car to facilitate their professional activities, like doctors, shop-owners and craftsmen. Overall, the labelling of cars as luxury items represented a technology policy that at least partly employed an anti-socialisation strategy.

Thus, technology policy with respect to cars could be seen as ambivalent, a mix of deployment and impediment efforts. Such ambivalence may be more common than most of the literature about technology policy suggests. Further, technology policy with respect to cars was not a concerted action. Rather, it was distributed, involving a multitude of actors with a diverse set of interests, objectives and instruments. Deployment was important to some, but most actors were socialisation agents contributing to the adaption of cars and related technologies to Norwegian society – some policy-making insiders, others being outsiders. However, one cannot understand the predominant role of the car in transporting people in Norway without acknowledging car owners' domestication of their vehicle as a combination of a necessary good and as an object of comfort, identity, and freedom. In this sense, deployment and socialisation had strong tail wind, despite import quotas (lifted in 1960) and high taxes.

Cars are definitively technology policy objects, but we have to be aware – as suggested above – that the technology policies that

1 This section is based on Sørensen (1991) and Østby (1995).

are meant to influence the deployment and use of cars may not be confluent. Some measures, like building better roads, may stimulate ownership and use of cars. Other measures, like taxes or road pricing, may work as anti-socialisation strategies. The lack of confluence may also be due to different interpretations of the public, like environmentally concerned citizens versus impatient consumers. Moreover, considering the historical process of appropriation of cars in Norway, it should be clear that the massive deployment was not mainly a policy outcome. It is easier to see socialisation agents – inside and outside of policy-making - that facilitated sense-making, but arguably, Norwegians were easily persuaded to become car owners and drivers. In this sense, the outcome of the Norwegian domestication of cars has shaped technology policy with respect to transportation, most obviously so by motivating anti-socialisation strategies.

Wind power development – in headwind?

Like the car, the deployment of wind power in Norway is basically about imported technology. Technological innovation has been a marginal and backstage issue. Moreover, deployment has been slow, mainly because of a general lack of investments in the production of electricity. Compared to hydro power, wind power has always been considered to be too costly, and technology policy with respect to wind power has mainly been an issue of how and how much to subsidise. In 2012, Norway joined Sweden in establishing a system of so-called green certificates to stimulate investments in renewable electricity through subsidies. While this deployment effort seems particularly beneficial to hydro power, it has also spurred increased willingness to invest in wind power. Per 2012, there were only 315 wind turbines in Norway, with a capacity of 704 MW. The capacity is expected to reach between 3 000 and 3 500 MW in 2020.[2]

With respect to socialisation efforts, the situation is more complex. Existing regulation provide a licencing system that calls for developers of wind power to inform and engage the local public, while the power grid infrastructure has imposed limitations with respect to constructions (Gjerald 2012). Gjerald shows that industrial actors working with wind power see the licencing system as bothersome because it is time consuming, but they also acknowledge the usefulness of the system exactly because it acts as a socialisation machinery. Two public institutions are part of the system as socialisation and deployment actors; the Norwegian Water Resources and Energy Administration (NVE) and the energy transformation directorate Enova. Enova oversees funding support while NVE grants licences.

For a long time, news media together with environmental organisations were the most important socialisation agents with respect to the interpretation of wind power. In the 1980s, wind power was framed positively as an environmentally friendly technology, but this changed during the 1990s. Increasingly, the framing of wind power became critical, with an emphasis on wind turbines being in conflict with conservation of nature, as noisy, ugly and dangerous to birds (Bye and Solli 2007, Solli 2010). Some scientists have tried to counter these views, and according to surveys, the general public is quite positive to wind power (Karlstrøm 2012). Moreover, most of the constructed wind power parks have met with little local resistance. Actually, local communities may want such parks because of benefits in terms of income, employment and improved roads. To some extent, this is the result of local governments acting as socialisation agents (Rygg 2012).

It is interesting and important to note that the Plan and Building Act – a legal instrument that regulates all kinds of major construction work in Norway – actually works as a piece of important socialisation machinery for wind power technology and many other technologies as well. This shows how technology policy to some extent has been automated in a way that has little visibility. The lack of concern for grid capacity, which has been and still is a bottleneck for wind power, is another indication that policy-makers may have thought financial measures, including R&D investments, to be sufficient efforts to achieve deployment of wind power. The existence of standard institutional procedures like the Plan and Building Act may cloud the issue of what technology policy should accomplish.

The situation with offshore wind, a priority area in Norwegian energy research, reflects a similarly narrow technology policy focus. Policy-makers have granted funding for R&D, which is so-to-speak end of story. The involved R&D institutions, together with their industrial partners, have been left with the task of innovating and commercialising offshore wind technology. There are no policy efforts to support any kind of training ground like a home market for offshore wind electricity. While industry is complaining about lack of government support (Hansen and Steen forthcoming), the involved scientists appear to be reluctant to take on any kind of responsibility to socialise the technology besides talking to their industry partners (Heidenreich forthcoming). Presently, there are no visible public deployment efforts and socialisation initiatives are meagre. There are no concrete plans to build offshore wind parks in Norway either.

A hydrogen road to nowhere?[3]

The HyNor project was established in 2003 as an effort to construct a network of filling stations for hydrogen that would provide infrastructure for fuel-cell vehicles to drive the 343 miles between Oslo and Stavanger along the south coast of Norway. The idea underlying the project was to provide a basis for a realistic experiment with the use of hydrogen for transport by building an early stage infrastructure for the provision of hydrogen, which later could become part of something more permanent. The project also included local experiments with the production of hydrogen, trying out several technological options like making hydrogen from gas from waste or reforming natural gas.

2 http://www.vindportalen.no/vind-i-norge.aspx (accessed 9.9 2013).

3 This section is based on Kårstein (2008).

The initiative to make the Hydrogen Road came from Norsk Hydro, a company that had large quantities of hydrogen available. It gained support from other interested parties, like bus companies, and obtained funding from Research Council of Norway and the Ministry of Transport. The project was presented as a user-directed, market close innovation project. The main innovations foreseen were linked to the set-up of a filling station network and related technologies. As a technology policy initiative, the HyNor project has increasingly been presented as a deployment effort with respect to hydrogen vehicles. HyNor is presently applying for funding "for a new permanent fleet of hydrogen cars in Norway, which through the project will identify remaining barriers for a larger introduction of hydrogen cars".[4] Support for such initiatives is sought through Transnova, a public technology policy institution set up to provide grants and advice for pilot and demonstration projects to encourage new sustainable mobility solutions.

From my technology policy perspective, it seems more pertinent to interpret HyNor as a socialisation effort than as an innovation or deployment initiative. The project has not been linked to any short or medium term plan to introduce fuel-cell cars in Norway on a commercial basis. Of course, HyNor could be said to have contributed to innovations regarding supply, storage and filling of hydrogen for vehicles. However, the main issue has been the construction of a sociotechnical imaginary (Marcus 1995) of hydrogen for transport, which includes an image of hydrogen vehicles as clean, safe and with a long range. However, the extent to which this imaginary, this socialisation effort, has been picked up by the public is unclear. Of course, one should not dismiss the technological learning achieved through HyNor. Surely, useful experiences have been reaped. Nevertheless, in the long run, the socialisation gains will certainly prove more important.

CCS – the Norwegian "moon landing" project

The idea that climate change mitigation could be achieved through technologies for capturing, transporting and storing CO2 has played a vital role in Norwegian politics to create broad consensus around energy and climate policy (Tjernshaugen and Langhelle 2009). When Prime Minister Jens Stoltenberg in his televised annual New Year speech in 2007 announced CCS as Norway's "moon landing" project, he launched a large innovation initiative while he performed an important socialisation effort. Still, the technology policy with respect to developing CCS for natural gas power plants has been carried through mainly as innovation policy through large R&D investments with little public visibility. To be fair, the underlying sociotechnical imaginary – gas power plants without CO2 emissions and thus a climate friendly use of an abundant source of fossil energy – has also been communicated, but mainly by ENGOs Bellona and Zero. These ENGOs, together with news media, have been the main socialisation agents.

News media coverage has been a mix mainly of recirculating the sociotechnical imaginary of CCS as a strategy for climate friendly fossil energy and complaints that the innovation and deployment efforts have been half-hearted. There have been nearly no critical voices with respect to whether CCS technology actually can deliver on the promises (Klimek and Sørensen forthcoming). On the other hand, the scientific expertise working with CCS technology is not particularly eager to engage in socialisation efforts, claiming that this is a job for somebody else (Klimek forthcoming). There is little doubt that the international situation with respect to CCS is quite challenging (Scott et al. 2013) and that a supportive technology policy needs to be comprehensive (Markusson et al. 2012). However, Norwegian CCS technology policy is fairly narrowly focused on innovation with little visible reflection among policy actors with respect to the socialisation of CCS, including the challenges of providing infrastructure and regulatory framework. It seems that CCS technology is believed to mitigate climate change in a way that to the public is 'out of sight, out of mind'. Thus, socialisation efforts are left to news media and ENGOs. This suggests that current CCS technology policy is not geared to embed CCS in Norway, but rather to innovate for use in other countries.

Conclusion: Technology policy as an embedding effort

Innovation policy may be described as a broad set of activities (Borrás and Edquist 2013); deployment policies similarly (IEA 2011). Still, as I have argued in this paper, a focus on innovation and deployment is too narrow as a point of departure for analysing as well as making effective technology policies. When innovation and deployment are the main concerns, this facilitates an economic, R&D centred approach that overlooks the challenges emerging from the need to embed new technologies in the relevant social practices. Thus, we need to extend the focus by including the concepts of socialisation and domestication of technology. Innovation, deployment, socialisation and domestication represent overlapping areas of concern, but also distinct issues that need to be considered separately. 'Innovation' is about the development of technology (or other goods) that has economic and/or social

4 Translated from Norwegian; «[...]en ny flåte hydrogenbiler som vil bli i Norge på permanent basis, og vil gjennom prosjektet identifisere gjenværende hindre før en større introduksjon av hydrogenbiler kan igangsettes.» http://hynor.no/art/hynor-prosjektet-i-endring (accessed 10.9 2013).

significance. 'Deployment' concerns putting innovations to use. 'Socialisation' points to the activities needed to embed new technology in society as well as to processes affecting the embedding (Skjølsvold 2012). 'Domestication' focuses on the enactment of technologies in specific contexts, with a view to the development of practices and sense-making.

These interrelated concepts are important to identify and understand the policy actions that are taken to make sociotechnical change happen (or not). Also, they are helpful as a basis from which to criticize missing features of a given technology policy, like the lack of emphasis on socialisation identified in the case of CCS above.

For example, an effective technology policy to reduce the use of petrol-fuelled cars should be based on an understanding of the ways in which such cars have been domesticated in Norway. It may include support of innovations to reduce emissions, develop new fuels or new ways of conducting transport as well as efforts to deploy more environmentally friendly practices. However, in the end, socialisation efforts are needed as an on-going concern to help pave the way for technologies that may mitigate climate change and reduce pollution – in parallel with anti-socialisation measures directed at technologies that should be phased out. This is needed to foster demand for the new technologies but also to actually change the currently well-embedded practices as well as the culture of transportation in the context of everyday life.

Thus, technology policy should address innovation, deployment and socialisation by supporting, mobilising and limiting human as well as non-human actors. Further, technology policy should be informed by concerns as well as knowledge about domestication of the technology or set of technologies that are to be affected. Thus, domestication has a different role than the three other concepts. Understanding domestication, the activities undertaken by customers, citizens and users to finally embed the technology in question, is important to be able to select and shape measures to effectively stimulate innovation, deployment and socialisation towards intended outcomes. In particular, socialisation efforts should be developed from insights into the performance of domestication or at least in dialogue with such performances.

Bijker and d'Andrea (2009) rightly observe that socialisation in most cases is given insufficient attention or even neglected. To some extent, this may be due to the assumption that there are systems already in place that cater to socialisation so that policy-makers may remain unconcerned about such issues (cf. the wind power example). On the other hand, such systems of socialisation also need to be acknowledged when we analyse technology policy practices. Analysing these systems may also remind about their existence as well as allowing assessments of their effectiveness. For example, there is a well-articulated expectation that scientists should engage in explaining their research to the public, but the systems set up to achieve this are not working very well (cf. the CCS example).

The neglect of socialisation challenges is probably also related to policy-makers' way of understanding demand as primarily an economic issue of consumption, downplaying the fact that consumers are also citizens and users. As citizens, the public may want to be involved in innovation and deployment of new technologies, at least to feel informed to the extent that they trust innovation and deployment actors. As users, people want to understand and make sense of the practices they may develop from new technologies. Socialisation efforts should cater to both needs.

The four examples discussed above may be analysed to show – unsurprisingly – that technology policy actions are multi-sited, multi-actor and multi-purpose. This complexity has not been dealt with in this paper, because the main concern has been to argue the need to include more sites and actors – in particular related to the inclusion of socialisation concerns. In order to deal with technology policy-making processes, further development is necessary to provide a better understanding of the role of non-human actors. One avenue to explore, given the emphasis on socialisation and the need to think about domestication, would be a concept of reflexive policy-making regarding technology. This could draw upon suggestions found in Beck (2006) and Latour (2007) to study policy-makers' processes of learning about and interpreting the embedding of new technologies.

Thus, there is considerable need for scholarly work to explore and systematise the analysis of technology policy as theory as well as practice. Hopefully, this might benefit the doing of technology policy. When technology is seen as sublime with respect to the society of the future, it would be nice to be hopeful that the embedding happens in ways that increase the probability that the assumed sublime qualities are realised.

Acknowledgements
The work underlying the paper has been economically supported by Research Council of Norway. I am grateful to the anonymous reviewer for valuable comments and proposals.

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HARRY'S CODE

an interview with Harry Collins

by Robert L. Jomisko

While attending a conference in Budapest in May, I caught up with Harry Collins in Memento Park; an open-air museum filled several dozen statues, busts and plaques from the Communist era. According to the Hungarian architect Ákos Eleőd, who designed it, "the park is about dictatorship. And at the same time, because it can be talked about, described, built, this park is about democracy. After all, only democracy is able to give the opportunity to let us think freely about dictatorship." In retrospect, it seemed a fitting location for the interview. As many of our readers are no doubt already aware, Collins has for at least ten years been engaged in discussions about social aspects of science and democracy. In efforts to impose what some have described as restrictions on public engagement with expertise, there are those who have labeled his proposals 'illiberal' and 'undemocratic'. Others have viewed them as an attempt to ensure expertise is not lost when engaging the public in decision-making. Ever since his early works on knowledge diffusion in the 1970s, Collins has kept reinventing himself. And with an advanced grant from the European Research Council, he shows no signs of slowing down anytime soon.

Keywords: Interview, expertise, democracy, science studies

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Harry Collins, when did you first realize you wanted to become a social scientist?
That's too early in my life, but I started my sociology degree in 1964. So, that's the answer, started in 1964. That's when I realized.

And how did you realize?
It's a series of accidents. It doesn't matter; it's too far back, let's go a little further forward.

How has your perspective on STS evolved throughout your career?
You can say my STS career began in 1970 or '71, when I was doing my master's degree at the University of Essex. My degree was based on the London University syllabus, which had a lot of philosophy in it. I had read a lot of philosophy of social science, in particular Peter Winch's *The Idea of a Social Science*, Thomas Kuhn's *Structure of Scientific Revolutions*, and Karl Popper, and I was really interested in the philosophy of science and philosophy of social science. Then when I was at the University of Essex doing my master's degree I had to do a dissertation, and I thought it would be interesting to go into science laboratories. So, I asked around and I found that in the physics laboratory in Essex University, where I was, were building a new kind of laser called the TEA laser – a Transversely Excited Atmospheric pressure laser. For my dissertation I decided I would do an information diffusion study of this laser. However, I knew I was going to do the study differently to the way it had been done before. It was going to be a knowledge diffusion study rather than an information diffusion study. With ideas from Kuhn and Wittgenstein, I thought I would study the ability to learn to build a laser. It was going to be like learning a language rather than gathering a set of discrete pieces of information that could be read from a book, because I had the idea of paradigm and I had the idea of form of life from Wittgenstein. I wanted to see this as a spreading, diffusing form of life, rather than as people gathering discrete pieces of information from one another.

I went around the country interviewing people who had managed to make one of these lasers work and I was particularly interested in who and how they spoke to other people, whom they contacted and so forth. What I discovered was that the only people who succeeded in building the laser and making it work were people who had social contact with other people who had a machine that worked. Then there were other people who tried to build the laser, but who weren't able to make it work. You could tell whether they worked or not because the laser was a very, very powerful instrument. It would produce a beam of infrared radiation when it was working and you could point it on a lump of concrete, and it would make the concrete smoke. So, you knew if you had your laser working or not.

Only the people able to make it work were those who had prolonged social contact with people who had a working laser. The contact could be by spending some time in their lab, or having some member of their team go up there and spend some time in their lab. That was my discovery and I wrote this up and said, "this shows that learning to build a TEA laser is like learning a language, not like learning discrete pieces of information, because you can only learn it through social contact in the way you learn a language." The title of the thesis was *The Sociology of the CO_2 Laser*. I remember people laughing or pointing to this title and grinning, because no one had ever seen a title like that before. How could you have a sociology of a CO_2 laser?

That's how I began in STS. I then became a Ph.D. student at the University of Bath, and what I decided to do was some comparative studies. I would finish the TEA laser study by continuing to trace the pattern of diffusion of the ability to build TEA lasers. tracing it from where it had all started in Canada, where Jacques Beaulieu had built the first one, and chasing it through all the American laboratories who had built one, and all the way back to the UK, so that I would complete the study and see if the findings held up. Whilst I was doing this I also thought I should compare it with some other areas of science and do similar diffusion studies, but in more competitive areas to see if the knowledge diffusion worked differently. The two areas I picked were parapsychology and the detection of gravitational waves, because I had read articles in the *New Scientist* that showed that both of these were quite controversial areas. Then I went to America, bought an old car and drove 7000 miles around America interviewing scientists in these fields and also in another field called the theory of amorphous semiconductors - which my supervisor suggested I should do, so I could have a theoretical field as well. That never came to anything, because I could never understand the physics.

At the end of my journey, while I was driving across Nevada in my old car – a big white Ford Galaxy, which cost me $200 – I suddenly realized that my study was completely flawed, and was a complete failure. The trick with the TEA laser study was to know whether the laser was working or not, and you could tell whether it was working because the infrared beam would make concrete smoke, whereas with something like the gravitational wave study, we didn't know what the gravitational wave detector was supposed to do when it was working. Should it be detecting gravity waves or shouldn't it be detecting gravity waves? And I thought to myself; how could I have made such a terrible mistake? I've completely wasted all the money from my Ph.D., and I'm a failure. After about half an hour it occurred to me that this was much more interesting than the original study, because if I don't know whether the gravitational radiation detector is working or not, neither do the scientists. How do they decide if it is working or not? From this grew the idea of the experimenter's regress and the relativistic approach to the Sociology of Scientific Knowledge, as my work in the 'Bath School', with its 'the empirical program of relativism', became known. I later discovered there was similar work already going on in Edinburgh – but more philosophical rather than empirical.

That was the start of what I did, and it eventually became a book called *Changing Order*, which wasn't published until 1985. In the meantime I had done a completely different study on Uri Geller and spoon bending children, which was published in a book in 1982. If I had any brains I would have published the second book first. That's how I started to become a sociologist of science; a very relativistic sociology of science, and it was as one of the founders, I think, of the Sociology of Scientific Knowledge, SSK, and was the first person to do any empirical studies in the Sociology of Scientific Knowledge. So, that's how I started, and you asked me has my perspective changed, and the answer is no, not at all.

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?
Well, you tell me what you mean by the 'political turn'?

There seems to have been a shift from focusing on laboratories to a focus on the political arena.
I've always been against it. In fact, I think it was when Wiebe Bijker was President of the Society for Social of Studies. During one of the meetings I remember him saying "we must now move down the boulevard of politics." - that is what is happening to Science Studies now. I stood up and said, "no we don't want to move down the boulevard of politics. There are many, many institutions, which are designed to do politics. Politics are everywhere, but social studies of science have a unique role, and we should stick with the unique role, and we should do it as scientists." Because scientists have a special warrant for getting people to believe what they say is true, and politics is too easy. All you have to do to be a successful politician is to say the same as everyone else wants to believe. But to be a successful scientist you have to say things that other people don't want to believe, and convince them that it is true. I said I disapprove totally of the Society (for Science and Technology) becoming more political. I would like to see this not happen, and so I was always against it. You're going to point out to me in a minute, that in recent years I've become a little more political myself, and it is true. But that is my main view about the politicized tradition of STS. I think it has spoiled STS.

For those not familiar with the Third Wave of Science Studies could you give us a little background?
Earlier on, I said my view hasn't changed at all, and I was slightly surprised that you had no reaction to this. Because I would have expected you to say "what do you mean your view hasn't changed at all? You're notorious for changing your view and turning from a radical relativist in the analysis of science to somebody who says you must have a realist theory of expertise, and we must favor science," aren't you going to say that?

Robert Evans: No, he can ask his own questions.

OK. But since you brought it up, Harry, I'll let you answer it.
The answer is that it isn't a change. It's not a change in the view about the nature of the world. It's just a change in where one turns one attention. I said in something published recently that if I look that way, I can see one sort of thing. If I look that way, by turning around, to look behind me, I can see another thing. But those things aren't in tension. It's just a way of turning your attention from front to behind. What I've done more recently started with a notorious paper called "The Third Wave of Science Studies", which was published in 2002. That turned my colleague Robert Evans and my attention to a different kind of question. The different kinds of question are: How can you still value science? Do you still value science in spite of all the analysis we've learned under what we call Wave Two of Science Studies, all the relativist analysis, all the experiments regress, and all these other things? How can we still value science before it disappears completely under our onslaught?

For those not familiar with the *Third Wave of Science Studies* article could you give us a little background? Why did you and Rob decide to write the 2002 paper?
The paper in 2002 represented a turn of attention to a different thing, which is how can one still value science or expertise in spite of all the things we have accomplished in Wave Two; all the relativism, the experiments, the regress, so on and so forth? The reason why I was led to turn my attention to this was because I worried people were arguing science into a worthless position. On the other hand there were some people asking sensible questions like what relativism has to offer someone who wants to argue with a man from the South African townships, who says he will cure his AIDS by having sex with a virgin. Presumably we can assume that having sex with a virgin is not a good idea to cure AIDS, but how are we going to argue this if we have no scientific proof? Especially if the grounds for expertise have been dissolved. The Third Wave of Science Studies paper was an attempt to find a way of showing that it is still possible to value science despite all the things that we had discovered in Wave Two. A lot of people thought it was a way of turning one's back on Wave Two, but it was not. The main trick was to shift attention from science as a truth maker to expertise, and ask the question: Who do we think has expertise? When we ask a question and we want some advice on something, one cannot imagine a world in which one doesn't value the advice of someone who has more expertise rather than someone who has less. If you try to imagine a world in which you don't value people who have more expertise, it's crazy. You wouldn't be interviewing me to ask what I think. You would be interviewing anyone who is walking in this park, because I would have no special expertise in what I am saying, anybody would do. The Third Wave was simply a turning of attention to expertise and experience.

Did Sheila Jasanoff and Brian Wynne's response to the paper surprise you? Do you think any of their criticism is justified?
Their response to the paper totally surprised us. Robert Evans and I thought of the paper as something fairly small. We thought people would appreciate the point and carry on roughly as before. We were completely astonished by the responses of Wynne and

Jasanoff, both by the fact of the responses and by the content of the responses, which are essentially political. Sheila Jasanoff says things like "oh, why should we take any notice of Collins when the whole world is moving in the other direction; towards democratization of science and expertise?" That's not an academic argument; it's a simple, straightforward piece of politics. Arie Rip also wrote a quite sensible critique which has largely been forgotten. It was a normal, academic critique. His was the one we struggled the most with to answer. The others hardly seemed worth answering. They were just political bellowings. We were very surprised about it. But we were very grateful to them as well, because I think if it hadn't been for those responses we wouldn't have done much with the Third Wave. We'd just have gone back doing what we were doing, maybe written some other little papers with the same lines. But suddenly we realized we must be doing something much more important than we thought we were doing. So, that's how the Third Wave started to grow, and it has grown into a very large thing indeed. And here we are in Hungary with a conference based on a grant which emerges from the Third Wave, and it's marvelous.

Contrary to Jasanoff and Wynne's response, Darrin Durant has argued that Rob and yourself, indeed, are democratic, but that you work within a different tradition, that of John Rawls and liberal egalitarianism. Do you agree with his interpretation?

We certainly agree that there's been nothing undemocratic about what we are doing. It seems bizarre to say so. In fact, I think up till now I've believed that I've lived in a democracy in the UK, and what happens in a democracy you live in is that you vote for politicians every four or five years, a new party comes in, and then you give them a mandate. You delegate them to employ all kinds of experts to make decisions about this and that and the other. Some of the experts are good, some of the experts are bad, but you don't try to make the expert decisions all the time. So nothing seems incompatible about expertise in democracy at all. I very much like Darrin Durant's paper, and I like a lot of Rawls' ideas, but I'm not sufficiently expert in the details of different models of democracy to comment on this in detail. Rob Evans is better at this.

At a conference I recently attended, Brian Wynne said, "Ordinary people should be involved in policy processes". What are your thoughts on this?

He said ordinary people should be involved in policy processes? Well, of course they are. Whenever they vote they decide whether the policy processes that have taken place are good ones or not. They express themselves in that way. I would have no problem with much of what Brian Wynne says. The only thing I object to is the ideas that flow out of the notion of so called 'lay expertise': the notion of ordinary people having as much expertise as so-called experts. There certainly was a time when Brian Wynne was expressing this view, or his followers were expressing this view, and it looked as if the very notion of expertise was going to be dissolved. This I found quite frightening actually. I'm not sure what he believes now, because it is very hard to get Brian to declare in a straightforward way what he believes, what he used to believe, and if he's changed his mind or not. It's not quite clear what his trajectory has been.

What can be done to improve the interaction between scientists and policy-makers?

This is where we get into the actual details. Rob Evans and I have already written a paper in *Critical Policy Studies*, which gives some kind of vague ideas about how science and policy relate, and now we're working on the details of this. My own view is that it's time that STS Studies stood up and were counted. I think that we in STS – at least some of us – are experts in the nature of science. The greatest experts in the nature of science there have ever been. I'm interested in notion of scientific consensus. It doesn't seem to me you can make policy without this notion. You can't make policy science relate to policy without it. That doesn't mean to say that consensus must drive policy. I think policymakers have to refer to scientific consensus and say "here I'm going with scientific consensus or here I'm going against it." As we express over and over again: The bottom line in all policy decisions are politics. It is always politics, which trumps everything else. But the public has a right to know whether politicians are going with scientific consensus or against it. However, we don't really know what scientific consensus is. The next step that my colleagues and myself want to take in Science Studies is to work out what scientific consensus is. What is a strong scientific consensus and what is a weak one? If we can figure out that, then policymakers will be able to refer to these studies and say: 'this is only a weak scientific consensus, so I'm going to go against it' or 'this is a very strong scientific consensus, I'm going to go against it and here are my reasons for it.' We continually refer back to Thabo Mbeki's decision not to distribute antiretroviral drugs in South Africa to point out that Thabo Mbeki was completely wrong in trying to justify his actions by saying there was a scientific controversy over the safety of antiretroviral drugs. There was actually a very strong scientific consensus over the safety of antiretroviral drugs. Thabo Mbeki would have been quite entitled to say "there's a strong scientific consensus over the safety of antiretroviral drugs, but we are not going to use them in South Africa, because we don't want South Africa to come under the thrall of Western pharmaceutical companies, because we don't want to project an image of South Africa as a disease-ridden, promiscuous society, and because we can't afford it." But he didn't say any of those things, which would have empowered his population to agree with him or disagree with him according to how they voted in the next election. Instead he said, "there's a scientific argument over this." It is not true; there wasn't a scientific argument over this, and social scientists – people who understand the nature of science, could say "at this point in time, Thabo Mbeki, there is no scientific dissensus, there is a very strong scientific consensus that antiretroviral drugs are safe. Even though you can find a big argument over it on the Internet." The skilled social scientist would say "those arguments on the Internet do not represent what the scientific community thinks."

I read in an article in *Nature* the other day that you were able to fool physicists into thinking you were one of them. How were you able to pull that off?
This is the notion of interactional expertise. Having spent many years intensively interacting with gravitational wave physicists I learned to speak their language. Out of this came the notion of interactional expertise. What I was doing in that test was demonstrating my interactional expertise in gravitational wave physics – which has probably faded quite a lot by now. I doubt I could pull of the same trick now. But at that time I understood gravitational wave physics pretty well, as a result of being immersed in the discourse of gravitational wave physicists for so long.

I also heard something about physicists evaluating your answers and comparing them to other physicists' answers, in a similar way you're doing with the Imitation Games.
That's right. We played an Imitation Game, which I pretended to be a gravitational wave physicist and other gravitational wave physicists gave answers. Nine people evaluated the dialogues and tried to guess who is who. Seven said they couldn't tell who was who and two said I was the genuine physicist.

Tell me about your new project, the Imitation Game. How does it relate to your earlier work?
It was just that I was interested in this notion of interactional expertise. It is a new concept and it is what you get by being immersed in the discourse of a specialist community. Even though you don't take part in their activities, we argued that you could acquire expertise and make judgments which were as good as an expert. To see whether this was true, we used a modification of the Turing test, called the Imitation Game. We asked people who were interactional experts to compete against genuine contributory experts, and then we had other contributory experts trying to decide whether they could tell the difference. What we showed is that you can't tell the difference. Now we have this wonderful European Research Council advanced grant where we're using the Imitation Game idea to test for the extent to which regular populations in a society understand minority populations, such as gays, or in very religious societies whether secular people can understand religious people. We play about 200 Imitation Games in each location and we use the numerical results as a gage of the degree of integration of one community with another, or degree of understanding of one community by another. We are doing this all over Europe and also in South Africa. It's one of the spinoffs of the Third Wave - something which we could never have foreseen.

What is next for Harry Collins? What are you working on?
I've just completed a book manuscript called *Are We All Experts Now?* It is a semi popular book about the relationship between ordinary people and expertise. It argues, of course, that we're not all scientific experts – except in some narrow respects. I've been completing responses to critics on a number of fronts, especially my book *Tacit and Explicit Knowledge*; that seems to have been discussed in at least two or three specialist journals. I'm still conducting my Wave Two gravitational wave physics study, and another book on gravitational wave physics is coming out at the end of 2013. I'm also completing a manuscript called 'Elective Modernism', which is another way, or another move, you might say, in the Third Wave of Science Studies. It attempts to argue that we should value science, not for its results but for its moral values. It has a slight Mertonian flavor, but it's more radical than Merton. Merton said we should value the values of science because they were so efficacious. We are saying that you should value the values of science because they are just good in themselves – and that's it.

The must-ask question: If money, time, space, and institutional requirements weren't an object, what would be your dream research project?
I don't know. I don't sort of work that way. I think I'm doing everything I want to do as it is, and I don't really have a dream research project.

Photo: Bård Ivar Basmo

NORDIC JOURNAL
of Science and Technology Studies

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.

Keywords: Interview, expertise, democracy, science studies

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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.

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

2 See for example, their conversation in Spivak and Butler (2007) and Graeber (2008).
3 For example Durant (2011).

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.

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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NORDIC JOURNAL
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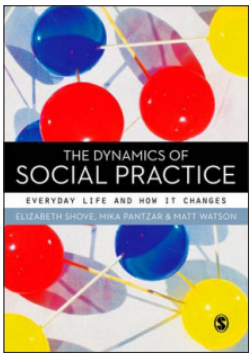
BOOK REVIEWS

The dynamics of social practice: everyday life and how it changes

Elizabeth Shove, Mika Pantzar and Matt Watson. Sage, 2012

by Sampsa Hyysalo



Theories of practice have had a renaissance in the new millennium. As some authors claim, this 'practice turn' has occurred as a response to the need for addressing more duly the actual doing and materiality of our everyday lives, rather than just their meanings, interpretations and intentions (Schatzki, 2002). While most of the debates in practice theory and most research drawing on it have occurred within social sciences, the turn to practice has reinstated the relevance of social analysis for other fields such as design, systems analysis and policies for sustainable consumption. This is in all likelihood due to practice theories' ability to address the socio-material richness of our conduct and communities as well as their capacity to address how complex change processes take place.

The dynamics of social practice: everyday life and how it changes (DSP) by Shove, Pantzar and Watson (2012) is positioned right at the heart of this stronghold of practice theory. It seeks to provide us with fundaments for understanding how social practices exist, prevail and change. In doing so it lays out at least an interim synthesis of the authors' decades-long development of practice theory. It discusses its bewilderingly wide topic (how everyday life changes) in just 160 pages with admirable clarity, positioning the volume as one of the potential text books for understanding practice, and indeed, how all social phenomena can be understood as practices.

DSP departs from a commonly agreed position. Practices consist of elements that are integrated when practices are enacted. These elements can have relatively independent lives, being enacted and held alive in other practices or, in occasion, lying dormant waiting to become enacted. Practices then, are relatively sustained and routinized ways of enacting a set of elements. This, in turn, entails that they are on the one hand performances that are more or less faithfully carried out in everyday conduct, and on the other hand, that they are entities which make up the life of their carriers, be these human or non-human. In this dual capacity practices emerge, persist, and disappear as links between their defining elements are made and broken.

It is safe to say that most if not all practice theories are comfortable with this entry point, but DSP's account of practices has several distinctive features. The defining elements of practice are reduced to three: materials (things), competences (skills) and meanings (social and symbolic significance). Taking materiality as a constitutive element of social practice is a step away from the 1970s upsurge of practice theories, most notably those by Giddens and Bourdieau, and one that aligns the authors with 'material constructivist' and 'post-humanist' approaches such as actor network theory, activity theory, and agential realism. Collapsing understanding, practical knowing and know-how into competence shifts the cognitive and experience element of practice into body, rehearsal and routine. The final element of 'meaning' includes cognitive and emotional aspects of practice alongside significance and symbols, as things that can be socially witnessed. There is thus a clear aversion of mental constructs in defining practice, which sets it apart from e.g. pragmatist and neo-Marxists' accounts of practice.

The second distinctive feature of the book is the extension of its theorizing. DSP is clear in seeking to provide an account of all social life understood fundamentally as consisting of practices, not of practices as a minor (or even major) subset of sociality. Moreover, it stresses that it does not see society as consisting of practices as stable entities or fields, but rather in continuous renewal, emergence and braking apart. The volume locates this relentless change in the intra- and interrelations of practices through three circuits of reproduction; first through changes within defining elements and their relations, then through ways in how practices are bundled together and finally through being part of transformations in large intertwined complexes of practices.

This frame of how the dynamism of practices emerges from the changing interrelations between elements and connections finds its empirical correlate in short illustrative change histories of various practices such as car driving, snowboarding and maintaining thermal comfort. Hence, in contrast to most practice research, DSP does not discuss in intricate detail any one practice, and it does not seek to provide original or convincing empirical correlates to its claims. This is perhaps necessary for the basic scheme to retain its clarity and power of theorizing, and many of the practice change examples build on authors' previous empirical investigations, such as Shove's insightful analyses of air conditioning and hygiene in her earlier *Comfort, cleanliness and convenience* (Shove, 2003). After all, when the aim is to convince people of the usefulness of practice theorizing in understanding the totality of social life – meaning all other forms and moments of sociality and indeed materiality and human agency – stem from social practices, it appears necessary

to draw from a broad range of examples rather than a few potentially idiosyncratic case analyses.

How does DSP then succeed in its grand mission in its short and accessible form? One must hand it to the authors that it does so admirably well. Particularly rewarding are the extensive sets of questions and anchoring to readers' common sense that are provided in each discussed change dynamic along the way. Indeed, DSP is a horn of plenty for great research questions to be further investigated for those who remain unsure of what they should do with their intellectual life.

The dramatic choices made by the authors, however, also leave much to further work on other ways of addressing practice. After the founding elements are settled in, the brevity of expression has perhaps led to keeping things rather 'item-like'. Chapter three discusses in broad strokes how each of the three defining elements has different types of transmission mechanisms, but the account veers towards almost systems-theoretical abstractions in how the change in the three circuits takes place. Empirically, the diverse and purposefully adapted illustrations lead the reader to wonder if the case histories are too purpose-built. Take snowboarding as an example. In several places it is said to be "a blend of surfing, skateboarding and skiing" that needs to continuously enroll new recruits to make up for those who leave for various reasons, something which has also lead to a clearly observable branching between the rigors of Olympic competition and free trick making. While all this is correct, it is highly skeletal. One has a hard time imagining this as a convincing account of snowboarding practices and their change. Snowboarding has drawn just as heavily from gymnastics, acrobatics and other jumping sports.

Similarly, one wonders where all the years of creative envisioning, intertwining and trials with boards, slopes, pipes, safety equipment, filming, advertising and other agential work and fortuitous findings disappeared. Characterizing snowboarding as having just two major branches and singular career paths erases from view the dozens of different orientations and equipment sets that are present in different versions of snowboarding practice. The point is this: the change dynamics of practices tend to be both internally and interrelationally rather complex – and accounting plausibly for this complexity and multicausality is a clear stronghold of most practice theories. The overly terse practice change descriptions of DSP conceal this complexity, for good and for ill.

The second signature facet of DSP – whether this is an upside or downside is left to the reader — is its rather social flavor. When practices are defined as including 'things', know-how, significations and other teleoaffective structures and seen to arise from the connections and configurations between these, the 'social', in principle, should be in the same plane of relevancy as the elements one tends to see as predominantly material or mental. However, the treatment mental and materials get in the book is thin in comparison to one received by issues traditionally regarded as social.

DSP reduces human individuals to (varyingly) competent carriers of practice that adopt, and locally adapt, social meanings of practice in performing it. This reduction leaves out an important aspect of human practicing, namely personal sense. Actors, as in people who act on stage for others, often refer to 'going through the motions', a phenomenon we are familiar with in our other daily less acted out practicings as well, to distinguish between a proper, passionate performance and one without a heart. Sense does not equal meaning, nor does enacting equal performing. Just as importantly, a practice theory without sense renders creativity, art and deviance as mere acts of recombination of elements by a given carrier. This presents a sobering account against the individualist, innate accounts of creativity that shamelessly bestow the wealth of cultural and social resources and dynamics at play simply inside a creative human. Yet going to extremes here risks producing a senseless theory of practice where the joys, frustrations, revelations, endurances and pulsations of practicing are cut out as irrelevant to dynamics of practice. Certainly, the authors could argue that cultural psychology has done enough in these areas, but simply doing away with these aspects appears somewhat strange.

The social tone of writing is amplified by the way DSP treats things. Many of DSP's practice change histories place technology or standardization as a pivot point of change. Yet, nothing is referred to in any detail, and most its things could be called 'things sociological', generic referrals to common technologies that are assumed to be known to the reader. The early car, snowboards, air conditioner units and showering make the story easy to follow. How their details affect the storylines is, however, left both empirically and theoretically hanging. Certainly the authors have done much in this regard in their earlier Science and Technology Studies work, but this makes the lack of attention to material-making in DSP more, not less striking. Would such descriptions and histories really make too tedious a read?

The lack of analysis of the material in DSP is reflected in terms used. The authors insist on practices being configurations, but resort to e.g. explaining that the 'script' of the early car changed, even as they had just remarked how early cars were complex and often improvised technical configurations that required intricate skills and social arrangements to function, such as the chauffeur-mechanic. Such technology is rather unlikely to have had a singular script or even set of scripts for how they were to be practiced.

In the final chapter, the authors outline what practice theory would entail in the framing of the questions of climate policy. It focuses on the critique of the dominating Attitudes, Beliefs, Choices (ABC) background frame of policy making pointing in the face of everyday life that seldom involves clear-cut choices (but is rather run

by routine practicing), notoriously mismatches people's attitudes (that are pro environmental), actions (that continue what they by and large used to be) and beliefs (which are typically well informed of the mismatch).

The practice framing of policy DSP offers would, instead, rather admit that peoples' everyday life is a moving target. It advances a provocative heuristic that one should examine the practices involved, assess where the greatest problems are, such as most unsustainable elements, moments, sites, practices or practice bundles, and then target actions to transform these. The chapter then sides with transition management ideas of offering protection to more desirable forms of practice (e.g. building cycling infranstructure) and exerting pressure on the regime of practice complexes (e.g. congestion charges) to encourage some of the car drivers to defect to bikers. Another example given is Japanese "coolbiz" initiative that combined new purpose built office apparel, elite citizen's example, fashion shows and advertisements with new office building cooling practices, in effect saving hugely on Japanese peak energy consumption through lesser cooling of offices. While all these ideas are well and good, critiquing reasoned choice models such as ABC is in effect an old sociological critique of economic view of man and offering a more social alternative, hence joining DSV in good social theory company.

Could practice theory deliver more? The examples now given resort to "happy face" practice theoretical intervention policies, and one can ask whether the happy face will be enough to curb greenhouse gas emissions. An interesting parallel can be found from policies related to smoking. After decades of piecemeal and relatively unsuccessful policies, often ABC-informed, many Western countries have begun complicating the practice of smoking in earnest by banning it progressively in public settings, increasing taxes, cutting opportunities to advertise, deploying increasing scientific evidence and court cases as well as making counter-advertisement increasingly gruesome and visible. At least in Scandinavia this is beginning to have a tangible effect. The anti-smoking measures also suggest that tightening the noose around a commonly undesired practice can find high public acceptance and result in a relatively rapid (as in a decade or so) sea change in practicing and practice complexes. What could be the parallel "noose pathways" in curbing private car use? Indoor energy use? Are some aspects of those already in use in some cities? Practice interventions themselves provide very useful inspiration for further interventions. Perhaps this connection, and the material and mental dimensions involved, is the part which the dynamics of social practice should have pursued in less terse fashion. After all, practicing as well as interventions on social practices predate (and outnumber) the theorizing on social practices.

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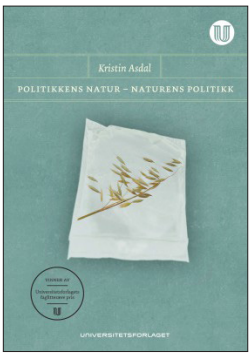
Politikkens natur – naturens politikk

Kristin Asdal. Universitetsforlaget, 2011

by Håkon B. Stokland



The title *Politikkens natur – naturens politikk* can be translated as 'the nature of politics – the politics of nature', and reflects the dual ambition of the book. Kristin Asdal investigates what politics is and how it gets done by simultaneously analyzing the origin and later development of environmental politics in Norway. The book is mainly based on her doctoral thesis from 2004, which has been remolded to match a broader but still mainly academic audience. It consists of six empirical chapters, which traces the development of Norwegian environmental politics by analyzing six defining cases in its history after world war two. Simultaneously, each chapter investigates the nature of politics by examining one political technology at the core of its analysis.

Asdal elaborates on her approach for studying politics and more specifically the politics of nature in the seventh and last chapter of the book. For this, she draws on Max Weber's studies of bureaucracy, Foucault's lectures on *gouvernementalité*, and actor-network theory. Weber treated bureaucracy as a tool for politics, and Asdal has found inspiration in his emphasis on the importance of technical devices and material arrangements in making the conduct of both bureaucracy and politics possible. Further, she has drawn on Foucault's insistence on studying government as practice, and his focus on governmental technologies and programs of government. In its treatment of the origin and development of a politics of nature in Norway, the book has gained much from Foucault's argument that governmental practices creates new realities that in turn shapes society. Finally, Asdal mentions actor-network theory as an important inspiration, mainly because of its role in making material technologies and the natural sciences relevant and accepted as fields of inquiry for the humanities. By drawing on these inspirations, Asdal examines how the politics of nature has been *done* by making what she has coined 'the technologies of politics' the center of her analysis. Asdal defines this term as the different ways in which scientific knowledge partakes in politics, as well as the technical arrangements and procedures that enables and shapes politics.

In tracing the history of environmental politics, Asdal's main focus is to examine how nature has been made politically relevant by different political technologies. The origin of environmental politics in Norway is often assumed to lie in the so-called green revolution of the 1960s and 1970s, and the establishment of the Ministry of the environment in 1972. However, Asdal shows that controversy concerning pollution goes back to the early postwar years and the establishment of an extensive aluminum industry in a number of rural communities. At this point of time, however, the pollution controversy was not a matter of vulnerable nature or the environment – it was a conflict between the business interests of industry and the local farmers whose domestic animals got fluoride poisoning from smoke emissions. A national board for smoke damage (*røykskaderådet*) was established in the aftermath of this controversy, making pollution an object of national management and regulation. However, the board was organized in such a way that it had close ties to the industry it was supposed to regulate. It quickly turned the pollution issue into an industrial issue, and distanced the issue from the damages of pollution on livestock, forests and agricultural land. The measurement and control of smoke emissions, not smoke damage, became the main regulatory strategy of the board. Emission numbers were easier to measure and control, but the disengagement of the issue from the damages made the emission level negotiable and hence the regulation weak.

Asdal argues that nature and the environment as relevant objects of government were created in the second half of the 20th century, and that they were formed in relation to industry and economic reasoning. The environment as a political issue, as well as an influential public opinion speaking on its behalf, originated in a controversy concerning an application to establish an oil-fueled power plant around 1970. This was not a controversial matter at first, but intense work by a few antagonists established relations between the potential power plant and the ongoing international negotiations concerning acid rain. This relation made the damages that the plant could cause in Norwegian landscapes evident, and the reinforced relation between pollution and damage engaged a larger public in the issue. Hence, the pollution issue as an industrial issue was challenged by an effort to make it an environmental issue. The effort paid off, as the plant was never built. The issue of acid rain was, however, not put to rest as the recently established Norwegian environment continued to take damage from other countries' emissions of sulfur dioxides. Asdal shows how the Ministry of the environment and scientists created a vulnerable Norwegian nature in the 1980s and 1990s, in an effort to ensure the prominence of ecology over economy and to make progress in the acid rain issue. As in the case of smoke emissions, the political technology they created in order to attain this goal consisted in the measurement and control of numbers and levels. However, this time they decided to measure the damage and establish levels prescribing how much pollution nature could withstand. The critical levels of nature turned out quite successful in generating a vulnerable nature in opposition to economic growth, and in persuading other countries to commit to reducing their emissions. The compatibility of this political technology of numbers with economic reasoning and cost-efficiency nevertheless turned out

to have a flipside, and soon economists argued that pollution levels should be raised enough to match the critical levels of nature as long as they did not exceed them. Further, Asdal argues that the environmental issue became a full economic issue as the controversy of climate change replaced that of acid rain towards the end of the 1980s. The vulnerable nature at the heart of the issue transformed from national to global, and Norway advocated the political technology of a system of climate quotas based on marked economy in the international negotiations.

In examining the history of Norwegian environmental politics by employing insights from science and technology studies and the field of governmentality studies, the book represents a fresh way of construing 20th century political history. However, Asdal's approach does not only involve the transportation of ideas from these academic fields into the field of political history – it brings something back as well. Most importantly, and this is one of the definite strengths of the book, Asdal approaches the origin and development of environmental politics by studying its history in empirical detail. By doing this, she nuances and criticizes some of the more theorizing and philosophical work on politics and its relations to nature and science within both science and technology studies and the field of governmentality studies. By reference to Bruno Latour's argument that Nature by way of scientists short-circuits the political process, Asdal argues instead that it takes a great deal of effort to make nature a relevant object of government. Further, she argues that nature, once established as a political object, is rather unstable and that it might very well get ignored in favor of for example economic considerations. Additionally, she shows empirically how nature and science can open a political process to new actors and even democratize a formerly closed process, rather than short-circuit it. Considering political technologies of numbers, Asdal nuances the weight put by Peter Miller and much of the governmentality literature on numbers as powerful tools of government. She shows empirically how it might take a great deal of effort to establish a political technology of numbers, and that it might not work as planned or work at all. The theoretical and methodological insights of this book, arrived at through a thorough and yet lively account of 20th Century Norwegian environmental politics, should be of relevance to anyone interested in the history and practice of politics.

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NORDIC JOURNAL
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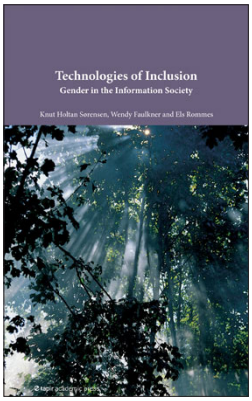
Technologies of Inclusion. Gender in the Information Society

Knut Holtan Sørensen, Wendy Faulkner & Els Rommes. Tapir Academic Press, 2011.

by Siri Øyslebø Sørensen



Does gender still matter? This is the initial question raised in the book Technologies of Inclusion: Gender in the Information Society. By the end of the book, there is little doubt that gender matters in Information and communication technologies (ICT) practices. Along the way new insights into how gender matters are presented. In this book, the authors Knut Holtan Sørensen, Wendy Faulkner and Els Rommes explore practices surrounding ICT. The authors pursue both a policy interest in reducing the digital gender gap and a scholarly interest in understanding how gender and ICT is intertwined and changes over time. In doing so they manage to develop a novel theoretical approach to inclusion as well as gaining new insights into how gender and ICT is co-produced.

The empirical foundation of the book builds on a large European study entitled "Strategies of Inclusion: Gender in the Information Society" (SIGIS), involving a large group of researchers. Like many studies of gender and technologies, the analysis presented in this book takes a co-production approach. However, the effort to balance the mutual influence of gender and technology leads to new ideas about how inclusion can be understood and achieved. Unlike many other studies, this book does not focus on the gender divide as such. The book departs from the standard approach of investigating digital inclusion by taking a more explorative approach to the question of what digital inclusion is and how it takes shape. The emphasis is on exploring practices with ICT bridging the gap (or even transformations of the very idea of there being a gap).

Based on a review of the existing literature on gender and ICT, the authors argue that research on gender and ICT tends to focus on how hegemonic masculinity is symbolically reinforced through technology. In examining the stream of co-construction studies of gender and technology, Sørensen, Faulkner and Rommes argue that this research tends to focus more on changes in technology than changes in gender; thus gender is stabilized while technology is understood as continually changing. In order to capture potential changes in gender through the use of ICT or in meaning ascribed to ICT, the focus is on positive experiences, i.e., inclusion strategies with success in terms of reaching the digitally excluded. However, we are still far from a 'success story'. In the words of the authors (p 242): "...the picture we want to paint on the basis of our studies is not an epic image of progress but rather a struggle – struggle with the meaning of gender, of ICT and of the resulting socio-technical assemblages."

Gender and ICT is in the book understood as a socio-technical assemblage. In gender studies there are many efforts to overcome the reproduction of the gender binary in research and writing. Thinking of gender as an assemblage, or gender and ICT as hybrid assemblages, provides an analytical strategy which avoids the reduction to the gender binary. It becomes possible to talk about gender and technology without getting trapped in classifications of technology being either feminine or masculine. Hence, the book provides insights and ideas relevant to a broader field of gender studies.

Following the idea of a symmetrical, co-produced assemblage of gender and ICT, the gender divide is not portrayed as a binary of included/excluded, but rather as a continuum. Furthermore, inclusion is not understood as simply adaptation to existing practices. Instead, the analytical approach is sensitive to the ongoing changes of both gender and ICT taking place. This analytical sensitivity, across several empirical studies of different sites, is one of the main strengths of the book.

In the book, digital inclusion is not understood as a goal in itself, but rather as an emergence of new socio-technical, ICT related practices and meanings attracting diverse groups of citizens. Hence, it challenges both the dominant narrative of inclusion based on the instrumental importance of ICT, and the mainstream idea of digital inclusion being something that can be measured by accounts of access, use, skills, formalized knowledge or work. Computers and the Internet constitute the main sites of study, covering a broad range of inclusion strategies. Both strategies aimed at women only, such as the use of gender quotas to educational programmes or initiatives to empower women in ICT use, and strategies aimed at including "everybody", for instance in practices of ICT design, are studied.

Gender stereotypes, and the gender binary as such, are challenged throughout the book. One study of 'self inclusion' through online interactivity and socially embedded learning shows how a symbolic reconstruction of women as competent users of ICT is produced. Another example is the analysis of efforts to motivate children and youngsters to use ICT through strategies of entertainment and glamour. This strategy transcends the gender stereotyped toy-tool binary, catering an idea of girls using computers for necessities, whereas boys use them for fun.

The findings of the empirical studies lead the authors to claim that an analytical emphasis on gender in the traditional ways (e.g.

technology as symbolically masculine) tends to portrait gender as a more stable and dualistic system than it might actually be if emphasis was instead put on practices. In the final chapters of the book, the initial question of whether gender matters is developed into a question about whether gender changes with ICT, i.e., does digital inclusion change gender?

According to the empirical studies presented throughout the book, the gendered stereotypes seem sturdy and hard to change, whereas practices change more rapidly. Strong numerical dominance of one gender enforces the symbolic link between the gender and ICT, while inclusion of the previously excluded might in turn change the symbolic meaning of ICT. The authors conclude that ICT did work as a technology of inclusion, but mainly with respect to ICT use. In uses of ICT, both practices catered to by stereotypical images of men and women and in inclusion aiming at a general audience, new ways of doing gender became possible. Thus, domestication became an act of inclusion.

An important insight extracted from the broad range of empirical studies is the fact that normalization seems to be at the core of any form of digital inclusion; hence technologies of inclusion include generic and potentially standardized tools. The book also begins developing a theory of inclusion processes: The authors describe what they coin as 'the anatomy of inclusion' through three interconnected elements: initiation (including motivation), underlying understanding of gender and inclusion problems, and choice of instruments and actions. Hence, the book provides a theoretical basis for further studies of inclusion practices, not only limited to the field of ICT.

Sørensen, Faulkner and Rommes synthesize the empirical findings, suggesting that a strategy of representation and an emphasis on diversity and complexity when trying to understand gender and ICT will produce less gendered ICT-centered assemblages. Hence, the policy implications of the main findings encourage a focus on both quantity and quality, e.g., changing numeric balance in order to change gender symbolism. Furthermore, the authors suggest that strategies transcending gender is the most effective inclusion measure in the long run, as it is destabilizing the gender symbolism of ICT.

In other words: this book is particularly relevant to scholars and students interested in gender and ICT, but in fact any scholar working with gender and technology in general, and in a co-production perspective in particular, should read the book. Technologies of inclusion. Gender in the Information Society represents an unavoidable contribution to the academic debate. The book is well edited and written in a clear and accessible language, thus making it suitable for a broader audience with an interest in ICT practices as well as to policymakers interested in using ICT as an instrument for equality. Furthermore, the clear line of argument and the well-reasoned analysis makes the book a useful text for teaching.

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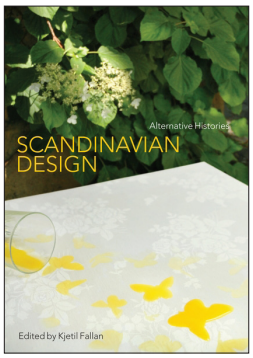
Scandinavian Design: Alternative Histories

Edited by Kjetil Fallan. Berg 2012.

by Maija Mäkikalli



Kjetil Fallan has edited a refreshing book called *Scandinavian Design: Alternative histories* which can be read at least from two perspectives. The book introduces new histories of design in Scandinavia and – as the subtitle points out – offers alternatives to the well-known histories we have read earlier. This history of Scandinavian design is described as a "cleverly crafted concept [which] has led to a disturbingly narrow understanding of Nordic design culture"[p. 1.], meaning characterizations such as "'humane', 'democratic', 'organic' and 'blond'." [p. 4.]. Indeed, the case studies presented in this new book of diverse topics such as the changes of copyright legislation in Denmark, the design process of reverse wending machines in Norway, the Cooperative Union's consumer policy and its "nonbranded" products in Sweden, or design students' political activity in the 1970s Finland have not been part of the earlier narratives on the history of Scandinavian design available in English. According to Fallan, such themes have been 'marginalised' in the previous accounts. [p. 1.] Thus the book opens a wider field of 'Nordic design culture' beyond the narrow conceptions of Scandinavian design and its history to its international readers.

This collection can also be read as introducing topical ways of doing design history – in any regional location. The book is divided into three parts corresponding to three concepts: networks, appropriations and mediations, each of which provides a focus or a tool for analyzing design, how it is, how it has been, or how it works. Indeed, in his introduction Fallan not only defines and contextualizes the concept of Scandinavian design and reasons why alternative histories are needed, he also deals with current design history writing in general, and the structuring concepts of the book help to highlight its contributions to "contemporary developments in design history's theory and method." [p. 7.] As much as the structuring concepts are linked to the case studies themselves, they also refer to the intersection of disciplines this book derives from, such as history of technology, design history or cultural history.

At times the reader may wonder whether a case study in question really benefits from the shared concept of the part or not. On the other hand, Fallan's way of finding shared concepts among these case studies, and structuring the collection accordingly, gives the reader useful material to consider while reading the individual studies with their own methodological choices. Yet another question is whether these alternative histories could have been structured with other concepts, such as 'otherness', or 'alternative moderns', and what results those would have produced with regard to the narrow conceptions of Scandinavian design that the book wants to criticize. In the last chapter of the book, "Epilogue", Fallan suggests tasks for future studies. He points out that gender, class and race (or ethnicity) are categories which still are under-explored in the field of design history in Scandinavia. To some extent these concepts are already in use in the case studies of this book – even if not explicitly so. Class, for instance, makes a good conceptual pair for democracy in deconstructing the narratives of Swedish design history, as we can see in Christina Zetterberg's analysis of Wilhelm Kåge's Liljeblå dinnerware and its subjects.

A crucial reason for the limited understanding of the history of design in Scandinavia among the international readership is the availability and access to the research literature of the topic. A 'Historiography' on design history literature in Scandinavia ('Pan-Scandinavian literature'), Sweden, Norway, Denmark and Finland is a valuable chapter for English-reading students and scholars. This chapter also serves one of the aims of this book: it wants to, and without a question also does, refresh and generate new studies of Scandinavian design history. To Scandinavian scholars this chapter is a reminder of the importance of publishing one's work in English. Fallan also reminds the reader that Norway and Denmark still don't have national survey histories of design. The lack of research literature, written in English, by Scandinavian or Nordic scholars is something that this book corrects. Thus, together with Malene Breunig, Hans-Christian Jensen, Anders V. Munch and Stina Teilmann-Lock from Denmark, Pekka Korvenmaa, Minna Sarantola-Weiss and Leena Svinhufvud from Finland, Espen Johnsen, Stig Kvaal and Per Østby from Norway, Finn Arne Jørgensen from Norway and Sweden, Sara Kristoffersson, Helena Mattsson, Jeff Werner and Christina Zetterlund from Sweden, Fallan succeeds in producing a body of work that brings Scandinavian research to the forefront.

If the structure of the book is rewarding, so are the individual chapters. They offer highly interesting readings of design historical studies in four Scandinavian countries. The earliest cases are from the beginning of the twentieth-century, and the latest from our own time; however, most of the contributors study the period from the 1920s to the 1970s. The cases deal with a wide variety of designed products, processes or related matters: dinnerware, photographs, brands, chocolate bars, trams, textile design production, sofas, legislation or political activity. They all widen our understanding of both design and design related contexts in Scandinavia, and the ways those can be explored. Kjetil Fallan has produced a highly recommended work in editing these contributions for readers both in academia and beyond, both in Scandinavia and outside.

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