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Advancing Understanding about the Relationship between Science, University and Society – An Introduction

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The relationship between science, university and society has been a topic of research in the broad areas of science, technology and innovation studies as well as higher education research for a long time; at least since the seminal dissertation of Robert K. Merton was published in Osiris in 1938 (Merton, 1938). In this groundbreaking work, Merton set out to understand, first, the social and cultural dynamics of science in the late 17th century England and, second, to understand the shifts in patterns of interest in various fields of science and technology. Although Merton's discussion about the dynamics of scientific activity during this period of time was cautious and he tried to dissociate himself from "any enterprise that sought to account for the scientific method or knowledge by reference to social or economic considerations", his work can be regarded as an intellectual kindling for later debates about "internal" and "external" factors affecting the development of science in society (Shapin, 1988: 594).

From today's perspective, Merton's study and the research on the dynamics of science and university in society that has evolved ever since, also marks the starting point of this special issue, an endeavour which has evolved during a series of sessions organised by Research Committees 04 (Sociology of Education) and 23 (Sociology of Science and Technology) of the International Sociological Association (ISA).

As is evident, the broad topic of this special issue is both theoretically extensive and empirically heterogeneous. The evolution of the field not only covers a long time frame, starting at the inception of science studies and ending at present times, but also includes a whole variety of theories developed at different levels of abstraction. These are: 1) research theories that are related to specific domains of empirical research, 2) diagnoses of our times, or 'performative histories' (Godin, 1998), that are messages sent out from scholarly conversation to a wider learned public and 3) general theory of sociology that addresses social theory, the constitution of the social and theory of society (e.g. Joas and Knöbl, 2009; Hammershøj, 2015).

A majority of the literature in science, university and society relations obviously represent research theories, i.e. theories that empirically analyse some specific phenomenon in a concrete social context. These types of theories have been developed, for instance in relation to the ways in which citizen groups understand and become engaged in

science (see Irwin, 2015), the use of scientific results, concepts or technologies in society (e.g. Parthasarathy, 2012), or the ways in which scientists manage boundaries between science and other knowledge forms (e.g. Leith et al., 2016). There is also much literature in science-society interaction which belongs to the diagnoses of the era type of thinking. These theories typically seek to answer questions about what the nature of our epoch is, they are often normative in nature and they endeavour to yield new topical insight. Examples of such theories include the evolving literature about the triple helix of university-industry-government relations (Etzkowitz, 2008) and Mode 2 of knowledge production (Nowotny et al., 2001).

Instead of providing an exhaustive account of intellectual development and heterogeneous debates associated with all of such literature, we will set the stage for the current special issue by way of discussing the relationship between science, university and society with the help of those theories that have been taken up by the articles included in this volume. Once we have briefly considered these theories in their original form, and after we have summarised the articles included in this special issue, we will search for the possibilities for increasing connectivity between different studies at the level of general sociological theory. For this purpose, we will draw ideas from an approach which we regard as a fruitful source of insight to understand the topic in question, namely Luhmann's systems theory (Luhmann, 1995), his theory of society (Luhmann, 2012, 2013) and his application of these general theories in the sociological analysis of the societal system of science (Luhmann, 1990).

Let us begin, however, with some of the most widely used theories of science-society interaction as referred to by the authors of this volume. Although these theories, namely the triple helix of university-industry-government relations (Etzkowitz, 2008), Mode 2 knowledge production (Nowotny et al., 2001) and academic capitalism (Rhoades and Slaughter, 2004), have been around for two decades now, they still figure as major models of science's transformation and are regularly used in the current research as reference points to the topic (see, e.g. Bychova, 2016; Fochler, 2016; Boggio et al., 2016; Hoffman, 2015; McLevey, 2015; Hicks and Wang, 2013; Parker and Crona, 2012; Randalls, 2010; Lam, 2010).

Theories about science, university and society

In parallel with the growing emphasis put on the post-war science, technology and innovation policies, research in science studies has asserted that a significant alteration has taken place in science and university organisation. Roughly speaking, two major positions can be discerned.

First, some authors state that a radical transformation of science and the university organisation has taken place. By using the metaphor of "the triple helix of university-industry-government relations" Etzkowitz (2008; also Gibbons, 2000) has claimed that the closer interaction between universities, industries and governments has given rise to a new kind of research which no longer primarily seeks to advance scientific knowledge but rather focuses on the development of commercially viable products. The triple helix is thus a metaphor that seeks to represent intensifying interaction and, indeed, a complex overlap between the institutional spheres of the university, industry and government. As a result, the boundaries between these spheres have become blurred so that "the extension of knowledge" becomes integrated into a compatible relationship with the "capitalization of knowledge" (Etzkowitz, 1998: 824–829). The increasing co-operation between the three spheres with particular interests in knowledge production also implies the emergence of the entrepreneurial university, a hybrid organisation which incorporates economic development alongside scientific research and higher education, for instance, through technology transfer offices, spin-off firms and science parks (Etzkowitz, 2008).

Another example of such a radical theoretical stance is the Mode 2 knowledge production thesis (Nowotny et al., 2001; Gibbons et al., 1994), which claims that science increasingly has become fused with other societal forms of practice. It states that research problems are no longer set and solved within the academic community but, instead, in relation to co-operations with their societal

contexts. Compared with the previous Mode 1 science, which subscribes to knowledge produced within autonomous disciplinary communities, Mode 2 science operates within more permeable organisational boundaries, and is managed for the achievement of particular useful purposes, such as a technological application or commercial innovation (Gibbons et al., 1994: 3–6). Various locales and practitioners are thus involved in Mode 2 knowledge production, spanning from scientists of different disciplines to industrial researchers and other societal stakeholders (Gibbons et al., 1994: 32–33). According to the Mode 2 thesis, the emergence of a societally more integrative mode of knowledge production also parallels the transformation of the university: the organisational structure of the university has thus become "stretched" so as to respond to the needs of the economy as well as other societal institutions, such as that of mass education (Gibbons et al., 1994: 70–89; Nowotny et al., 2001: 79–94).

Whereas the above-mentioned models speak about a very profound transformation of science and the university, there are also moderate views on the change. According to these perspectives, the political use of market and market-like mechanisms has increased in the fields of science, university and higher education. In their study of public universities operating in English-speaking countries, Slaughter, Leslie and Rhoades (Slaughter and Leslie, 1997; Rhoades and Slaughter, 2004), for instance, found that during the past two decades universities have become increasingly oriented towards "the profit motive" and "market-like" behaviour due to the neoliberal policies aimed at securing nations' competitiveness in the global economy. The resulting 'academic capitalism' refers to efforts by universities and individual scientists to secure research grants and other forms of external funding on the basis of which they can work in a situation where the basic funding of universities has diminished (Slaughter and Leslie, 1997: 8–9; Rhoades and Slaughter, 2004). This development has created a lot of tension, including the encouragement of professors to become commercially active simultaneously with their teaching duties being increased (Slaughter and Leslie, 1997: 8–9; Rhoades and Slaughter, 2004; cf. Münch 2015). Despite these difficulties that have been extensively addressed (e.g. Münch, 2015; Wadmann 2014; Brown, 2010; Tuunainen and Knuuttila 2009; Tuunainen, 2005; Krimsky, 2003), Slaughter and Leslie maintain that academic, commercial and bureaucratic cultures are integrating and that the distance between universities, industries and governments is decreasing. Instead of being organisations oriented towards producing scientific knowledge under strong institutional autonomy, universities in this perspective are becoming engaged in what other scholars have called entrepreneurial, commercialised, privatised and post-academic science (e.g. Etzkowitz, 1998; Thackray, 1998; Radder, 2010; Mirowski, 2011; Ziman, 2000).

Contributions in this volume

While the above-mentioned theories discuss the transformation of science and the university in rather general diagnostic terms, our task in this special issue is to open up the subject to qualitative empirical research and explore, on that basis, the possible theoretical directions with the help of which an advanced understanding of the relations between science, university and society could be achieved. The first article by Norma Möllers draws from an ethnographical study of a government-funded, transdisciplinary research group which was engaged in the development of a "smart" video surveillance system for screening "dangerous" behaviour in public places. Anchoring her study in the discussion about the neoliberal technoscience (Lave et al., 2010), theories of transformation in science and the university (e.g. Gibbons et al., 1994; Etzkowitz, 2008) and symbolic interactionist perspective on scientific practice (e.g. Clarke and Star 2008), she directs attention to the ways in which scientists manage the conflicting demands present in "hybrid spaces" composed of academic and industrial actors and their divergent interests. More specifically, she uses the concepts of "forward tailoring" and "reverse tailoring" to understand how scientists translate either practical problems of research funders into research problems that are sensible in "scientific worlds" or, vice versa, scientific problems into ones that are close enough to those issues that

funders want to have solved. Thus, the contested or negotiated conditioning of the forms of scientific problems becomes the centre of her empirical concern. The rich analysis of this kind of 'boundary work' (Gieryn, 1999) presented by the article increases our understanding of the various kinds of articulations and translations scientific practice rests upon at the grass-root levels of universities, simultaneously as it further elaborates the ways in which stability at the interface between science, government and the wider public can be achieved.

The second article by Pia Vuolanto also addresses the topic of boundary work between science and society, now in the context of a newly institutionalised field of nursing science. Making use of symbolic interactionist 'arena analysis' (Clarke and Star, 2008), she investigates the process whereby the representatives of different social worlds, such as those of medical specialists, sceptics, nurses and patients, pulled nursing science in different directions at an early stage of its academisation (Neave, 1979). Instead of being a clear example of neither Mode 1 or Mode 2 science nor an instance of transformation between these, nursing science in Vuolanto's account is a discipline which straddles two ideal-typical activities, which are the production of knowledge for the academic community and production of knowledge for societal stakeholders (cf. Albert and McGuire, 2014). Another way to understand and discuss the tension-laden relationship between nursing science and society, according to her, would be to see it serve distinct academic, corporate, professional, policy and public markets (Ylijoki et al., 2011), all of which require different kinds of contributions on the part of practising scientists. In addition to being interesting in shedding empirical light on the complex relationships between professionally oriented disciplines and different extra-scientific interests, Vuolanto's study also underlines the need to further theorize the ways in which different interests are being combined with or translated to one another for the purpose of providing a stable context for a new discipline to institutionalise. The article thus advances a small but evidently growing literature which combines research on boundary work with that of discipline formation (Kurath, 2015; Beddoes, 2014).

The third article by Manuela Fernández Pinto describes strategies used in the social construction of ignorance, especially when pursuing clearly commercially driven research. Her concern arises from the mutual changes in corporate research and university organisation. The former has turned the strategic view from the "in-house research labs" to the outsourcing of research and development (R&D), while universities have increasingly adopted new forms of liaison with external funding sources. The Cold War era's R&D regimes supported strategic deals within the military-industrial-academic complex (MIAC) in the political environment of universities that motivated the creation of costly research units inside MIAC relevant industrial corporations. Such arrangements have been partly restructured by the current research, development and innovation (R&D&I) regime, which stresses commercial research through wider societal engagements so as to produce innovations for differentiated markets, and thus find new potential for economic growth (see Kantasalmi, 2015). Such a switch of emphasis in the science policy regime and the related permeability of the university organisation present new issues for the organisation of scientific research. This happens, for instance, when organised secrecy of the Cold War classified knowledge becomes amended in grey zones of commercialised science in terms of 'limited secrecy' (Etzkowitz, 2011). To better understand the complicated changes in the organisational couplings of science and the university, we would need advanced conceptual guidance to regulate the consistency between the diagnoses of the times that speak about transformations in knowledge production and the university organisation, and the variety of empirically-based research theories that enrich them.

In search of a more general theoretical view on science-university-society interaction

As illustrated by the articles published in this special issue, transformations in the inner organisation of the 21st century university system are

becoming all the more obvious. Both STS and higher education research point to the processes that bring some sorts of "hybrids" to substitute for preceding pure forms, that is, differentiated formats of communication or logics of practices. These hybrids have a twofold meaning that relates to the sociological studies of scientific practices and the literature that speaks about the transformation of the university organisation.

First**,** hybrids address the fact that research groups are often simultaneously engaged in the production of societally useful end-products and the creation of related scientific knowledge. In this view, science is seen as a deeply societal endeavour where practical utility operates as the paramount justification for scientific research. As empirical research shows, combining theoretical understanding and societal use is not an extraordinary feature of scientific practice but, rather, a quite common attribute of much of the current science (e.g. Hessels, 2010; Miettinen et al. 2015; Powell et al., 2005; Tuunainen, 2001).

Second, the term hybrid refers to a corpus of research according to which the entire university organisation has been in a state of fundamental transformation. These studies, proliferating in the fields of higher education research and research policy, argue that financial considerations related to global economic competition have permeated academia. Science and technology policies have begun to emphasise potentially lucrative areas of research, while simultaneously universities have encountered hard times due to considerable cuts in governmental allowances. In consequence, universities' dependence on external funding has increased in tandem with the privatisation of research results. In the wake of these developments, universities are in a state of profound change (e.g. Etzkowitz, 2008; Owen-Smith, 2003, 2006; Marginson and Considine, 2000).

In order to advance our understanding of the nature of this conflation and the related forms of social order coming up at the interface of science, university and society, we want to point to the need for varying perspectives in the theoretical regulation of empirical observation at the level of general sociological theory, that is, theory of society and social theory concerned with the most general presumptions of the social sciences. In this regard, the recent developments in systems theory (Luhmann, 1995) and its application to societal theory (Luhmann, 2012; 2013) offer potentially useful concepts and ideas. The increasing precision in the empirical analysis of the changing forms of knowledge production in universities and the multiplicity of the diagnosis-of-the-times type of theorising both underline the need for amplifying conceptual coherence at the level of sociological theory. Because all of the articles included in this special issue point to some sort of hybridity and the related demarcation problems between university and society, we believe that attention directed in different ways in which societal systems (e.g. science, education and economy) are coupled via organisations (e.g. universities) might prove to be a fruitful avenue to increase the connectivity between the individual empirically-based research theories.

In Luhmann's view, modern society evolves primarily according to the principal of functional differentiation. His theory offers an explanation for the emergence of autonomous communication within society. Such systems continue self-reproducing their unique communication formats as operationally closed to their respective environments, while observing problems pertinent to their particular functions to the society. Society is conceived as a particular social system, which contains all possible communications and nothing more; consequently, that is a world society with nation-states as its subunits (see Pfeffer and Stichweh, 2015). This offers a possibility for distancing analysis from methodological nationalism, which has often framed studies on higher education and innovation policy. Furthermore, societal functions of systems, or their codes of communication, do not conflate. Differentiated systems (e.g. science, education, economy and politics) do not communicate directly with each other, but they can communicate about observing each other.

All articles of this special issue refer to the need of sharpening theoretically meaningful distinctions that could enhance describing the nature of increasingly complex boundary controversies, that is, the hybridity in organising relations of universities in societal environments. Luhmann's conceptuality offers various tools for grasping

the interdependency of societal systems as they become organised in the university and system of higher education. The concept of 'structural coupling', which replaces the input-output distinction of older system theory, appears to be particularly fertile for describing a system's selective openness to its self-constructed environment. The empirical analysis of such couplings might be one promising way of describing internal changes in the university in regard to its traditional ways of organising the interdependence between the function systems of education (teaching for the purpose of effectuating psychic systems of persons), science (controlling the uncertainty by means of defining researchable problems) and administration or research management which draws upon a system of politics (producing binding decisions). Thus, instead of conflating their codes, these functional systems can resonate with each other, meaning that a given system's societal environment (i.e. another system) irritates or stimulates its internal state thus sensitising it to changes pertinent to its own structural state (Luhmann, 1986, 40). The more extensive the system's internal structure, the more likely it is that it resonates with its environment. For example, the more there exists variety in the disciplinary structure of science, the more likely it is that science is stimulated by its environment, such as education, economy or politics. In Luhmann's perspective, these sorts of influences are conceptualised as structural couplings between autonomous social systems and their self-constructed environments. The analysis suggests one to observe how the social system of science transcodes external issues and influences into topics of its own, or, vice versa, how scientific results become translated into forms that are useful, say, in the economy by way of patenting and licensing.

It is precisely these sorts of relations and translations between science, economy and other social systems that are in focus in the articles included in this special issue. For instance, notions of forward and reverse tailoring by Möllers point to the processes where scientists voluntary orientate their research to meet external expectations set by the research funding agencies. Although the concepts of tailoring introduced by her are useful in understanding the processes that are taking place here, further theorisation with the help of ideas provided by Luhmann would provide additional benefits, simultaneously as the intellectual value provided by the original conceptualisation would not be diminished. In Luhmann's conceptuality, forward and backward tailoring could be regarded as a process reflexivity where special semantics are produced within the system of science so as to constructively sensitise the operations of science to external policy conditions mediated by governmental research funding. The question here is not about a sheer terminological shift from one analytic language, that of Möllers, to another, i.e. Luhmann. Rather, the advantage of trying to work towards a more abstract conceptuality is to open up an avenue for thinking and communicating across individual case studies and research theories they have developed. With the help of the conceptuality developed at the level of general sociological theory, such as that of Luhmann, intellectual integration of otherwise relatively heterogeneous field of research might be achieved.

Thus, the back and forth tailoring introduced by Möllers and considerations of boundary work described by Vuolanto could together be interpreted as specific reflection performances that produce relevant schemes for organisational (whether a research group or discipline in the university) decisions. In situations like these, the inside/outside distinction of science is maintained as the organisation aims to reduce the contingency in its environment, whether in gaining academic recognition for a new specialisation or adjusting research problems to meet the requirements of external funding. In such internal orientations, specific decision schemes are deployed, and these are conditioned by organisation-specific arrangements that have been previously decided upon more established structural couplings with the societal environment, say stable partner structures or other contractual forms. The latter, however, are not brought about arbitrarily but, instead, along the premises that different function systems develop to meet historically changing boundary conditions.

The notion of technoscience, as mentioned by Möllers, can very well be a semantic level indica-

tion of a mutual learning process between the two societal forms of knowledge production, science and economics. Under such structural conditions, scientific knowledge production becomes more responsive to research, development and innovation-oriented (R&D&I) policy programmes. In addition to the possible internal correctives implied in the mutual long-term learning between the systems of science and economics, the policy programme formulation of R&D&I could be viewed as a kind of specific contingency formula developed within politics so as to modify the premises of internal steering within universities to better adapt the organisation to the external political and economic environments (see Kantasalmi, 2015).

Based on Proctor's (2008) views on ignorance, Fernández Pinto discusses the genre of studies with a constructivist view on ignorance, called agnotology, and then synthesises the main practices of constructing ignorance in research driven by the tobacco industry. She demonstrates the fertility of such taxonomy in understanding the strategies of ignorance production in current controversies over climate change, pharmaceuticals and financial crisis (see also Kleinman and Suryanarayanan, 2013). This is an important viewpoint as long as the R&D&I policy regime seeks to increase the variety of knowledge production, either via the detailed allocation of public research funds or by austerity measures reducing university science's relative portion in the R&D&I expenses. Both of these mechanisms offer space for recruiting university trained doctoral students and PhDs into development and innovation driven research programmes in private companies. Along such development the variety of doctoral training programmes increases within organisations of higher education (Kantasalmi, 2015; Kehm and Teichler, 2016) and, in consequence, the selective openness of science and tertiary schooling to their respective societal environment alters, e.g. in regard to researchers' labour markets. Such an increase in the organisational proximity between the university and industry points to the core of the global system of scientific communication, which is basically processing meaning as guided by concerns over true and untrue statements. Thus, the possible biasing effects produced, for instance, by the research funding sources are supposed to be neutralised in the course of global science communication, and by means of its institutionalised ways of organising the production of new knowledge.

The classified knowledge of industrially-oriented science with its noxious societal consequences has already awoken worrying dystopias (e.g. Krimsky, 2003). More recently, the urge for speedy dissemination of novel information via the technologically empowered mass media is effectively intervening the loci of different knowledge forms. Such situational complexity is underlined by all of the authors of this special issue, as well as much of the STS literature discussed in this introductory article. Drawing on Bourdieusian presumptions of the primacy of power, Münch (2015), for instance, views harmful effects in hybrids, like the ones discussed by Möllers, Vuolanto and Pinto. In reference to systems theoretical concept of structural coupling – also a hybrid for him – he envisions the economisation of science, based on political steering, to result in conflation of institutional logics of science and economy as a consequence of which science would lose its autonomy, and claims that systems theory has limits to enfold such hybrid forms in politically meaningful ways (for contrary claims, see e.g. Karafillidis, 2015).[1] This should not be taken as an unavoidable outcome, however, as each of the functionally differentiated systems, such as science, can generate several programmes according to which it operates. Organisations, like universities, are carriers of such programmes and allow different forms of couplings to be made, say, between innovations expectations of the economy and university research and teaching programmes. Such situational complexity frames the daily interaction between individual minds, however, and it is currently further confused due to the deliberate production of ignorance, i.e. agnotology, which can be motivated not only by lucrative commercial aspirations, but even on the basis of our differing moral valuations (see Proctor, 2008: 9).

Conclusion

The articles published in this special issue all point to the relationships between science, university

and society as well as the related transformations in the organisation of the university. The co-evolution of education and science has brought about adaptations in the university ever since the socially more inclusive systems of higher education started to emerge in the 19th century. The post-Second World War expansion of the system of higher education, both in terms of enrolments and in the number of organisations of tertiary schooling (Meyer et al., 2007), suggests asking whether the institutionalised coupling of education and science has loosened. In other words, universities, as part of a complex system of higher education, are starting to emphasise the social form of school, simultaneously as science is starting to emphasise its new social forms of collective, industrially oriented knowledge production. The consistency of the scientific labour force is still largely regulated by what Kant viewed as the core of the university, i.e. the factory-like (*fabrikenmässig*)[2] production of doctors, but along with the expansion of doctorates, the inner logic of schooling has removed the significant level of scientific education from post-graduate schools into the post-doctoral training programmes, which may very well have their organisational loci outside of the university system. Thus, the legitimacy of the R&D&I contingency formula offers a space for the emergence of entirely new organisational arrangements as for the continuation of publicly funded industrial doctorates (see Kantasalmi, 2015).

All of the authors in this volume capture the relevant trends with respect to the academy's current dynamics, and they do so both interestingly and with a high level of quality. Because of their focus on the empirical phenomena in specific contexts, there is, however, an opportunity for increasing theoretical connectivity between the studies at a more abstract level, an effort which would in our view also serve future empirical research on the current transformations. A discussion of the analyses provided by the authors at a more general theoretical level might thus allow for beneficial insights into the complex relationships that the university organisation has encountered in its current societal environment. The lead metaphors, such as Mode 2 knowledge production or the triple helix of university-industry-government relations, only point to the relevant and topical phenomena, simultaneously as the real challenge is how to adequately focus and conceptualise the descriptions so as to provide fruitful linkages between different interpretations. The papers published in this volume do a valuable job in continuing this line of thought. A characteristic of high-quality empirical research is in our view that it opens up more interpretative avenues than it closes. The three papers published here are of this sort and, therefore, they spurred our thinking towards an interpretative experiment, which could further advance our understanding of the phenomena taking place at the borders of science, university and society. This sort of theoretical work, which would make use of the general theoretical understanding of learning and knowing is, however, a collective mission designed for the interdisciplinary community of researchers. In achieving this goal, the recent developments in systems theory might provide a sufficiently general conceptual ground for enhancing connections between different research theories that regulate empirical studies.

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NOTES

1 For the preference of the Luhmannian notion of 'structural coupling' even in the Bourdieusian frame, see Arnoldi (2007: fn. 51). For an empirically productive reading of the 'structural couplings' in Luhmann's theory, see Knudsen (2007).

2 By this, Kant in "Der Streit der Fakultäten" refers to an organisation principle resembling the division of labor where universities create doctors for the learnedness and scholarship in society. Today, we would first think the massive scale in tertiary schooling of doctors, and surprising variations in contents of doctoral programmes, such as the "industrial doctorates" in the European Higher Education Area.

The Mundane Politics of 'Security Research:' Tailoring Research Problems

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Abstract

Since the late 20th century, Germany's federal science policy has shifted towards an emphasis on commercialization and/or applicability of academic research. University researchers working within such strategic funding schemes then have to balance commitments to their government commission, their research, and their academic careers, which can often be at odds with each other. Drawing on an ethnographic study of the development of a 'smart' video surveillance system, I analyze some of the strategies which have helped a government-funded, transdisciplinary group of researchers to navigate conflicting expectations from their government, academia, and the wider public in their everyday work. To varying degrees, they managed to align conflicting expectations from the government and their departments by tailoring research problems which were able to travel across different social worlds. By drawing attention to work practices on the ground', this article contributes ethnographic detail to the question of how researchers construct scientific problems under pressures to make their work relevant for societal and commercial purposes.

Keywords: directed funding, commercialization, tailoring, boundary work, algorithms, surveillance technology

'Neoliberal technoscience' and directed research funding

Since 2007, the German Ministry for Education and Research has funded projects which are supposed to develop security technologies and procedures with a funding scheme called the "Security Research Program." The program has heavily emphasized the development of new surveillance technologies, such as those used to monitor urban spaces. Funding requirements for university researchers include the commitment to finding solutions to security problems, collaboration with small and medium enterprises, and the inclusion of social scientists or legal scholars. The research program's goal is to increase citizens' security through transdisciplinary research, and to strengthen the position of German companies on national and international markets by transferring the research to security products and services.

Directed funding schemes like the Security Research Program can be situated in an ongoing debate on 'neoliberal technoscience' and the increasing commercialization and applicability of scientific research. As Lave, Mirowski, and Randalls (2010: 667) point out, cross-cutting features of 'neoliberal technoscience' include, among other

things, the "rollback of public funding for universities" and "the separation of research and teaching missions, leading to rising numbers of temporary faculty." Particularly the rollback of long-term funding makes scientists more dependent on short-term directed funding schemes sponsored by industry or governments, and thus more amenable to the latter's demands to make their research relevant for societal or commercial purposes.

However, it remains a subject of ongoing debate how and to what extent knowledge production is changing under conditions of 'neoliberal technoscience.' Although scientists working in directed research projects have to anticipate demands for commercialization and social relevance if they want to obtain funding, it seems unlikely that they will give up their commitment to their academic disciplines. Academic institutions and organizations, in turn, may not always reward the kinds of research that governments or industry fund scientists to carry out. Thus, scientists working in directed funding schemes may have to navigate multiple and conflicting disciplinary, political and economic demands.

This paper explores the ways in which scientists deal with such conflicting demands in their everyday work. Although we have a fairly good idea of how organizations manage tensions resulting from the changing institutional landscape on an administrative level (Guston, 1999; 2001; Miller, 2001; Parker and Crona, 2012; Tuunainen, 2005a, 2005b; Tuunainen and Knuuttila, 2009; Wehrens et al., 2013), knowledge production 'on the ground' is still relatively unexplored. The aim of this paper is thus to contribute empirical detail regarding knowledge production under conditions of directed research funding, and to further the understanding of how scientists construct scientific problems under pressures to make their work relevant for societal and commercial purposes.

Drawing on an ethnographic study involving a transdisciplinary research group commissioned by the Security Research Program to develop an automated closed-circuit television system (CCTV), I show how scientists navigated conflicting expectations in their work by tailoring research problems that were able to travel across different social worlds. By tailoring research problems that fell into their departments' previous lines of research, but could also be interpreted as practical problems pertinent to surveillance systems, the scientists in my study managed to "keep politics near enough" to secure their funding, but "not too close" to interfere with their research interests (Gieryn, 1995: 434–439). However, tailoring their work also meant continuous 'articulation work' (Fujimura, 1987, 1996; Star and Strauss, 1999). The varying extent of the articulation work necessary to cope with conflicting expectations was tied to the ways in which they positioned themselves with respect to the government's demands: The more work they had to put into adjusting their scientific problems to conflicting demands over the course of their project, the more problematic was their experience of the government's demands.

Tensions, misalignment, and articulation in scientific work

A number of scholars have raised the question whether political efforts to commercialize university research have led to significant changes in academic practices and institutions. Drawing attention to modes of knowledge production, terms such as 'mode 2' (Gibbons et al., 1994; Nowotny et al., 2001), 'post-normal science' (Ziman, 2000) and 'academic capitalism' (Slaughter & Rhoades, 2004) attempt to capture the increasing importance of political and economic considerations in academic research. These models claim that such considerations shift the purposes of scientific work from understanding the basic principles of the natural world to the development of applicable and marketable technologies. Others have framed the question in more institutional and organizational terms, claiming that changing notions regarding the purpose of science are reflected in increased interdependencies between universities, industry and governments, eventually resulting in 'entrepreneurial universities' (Etzkowitz, 2003; also see Kleinman and Vallas, 2001 on converging academic and corporate cultures).[1]

More recent work has provided plenty of evidence that changes are, by far, not as sweeping as earlier attempts to capture 'neoliberal technoscience' have suggested. This work has examined

in more empirical detail how university-based scientists and organizations perceive and deal with the complexities of their changing environments. For example, scientists display varying attitudes concerning engagement with corporate or policy actors, ranging from advocating engagement to outright resistance (Goldstein, 2010; Holloway, 2015; Lam, 2010; Owen-Smith and Powell, 2002). What seems to account for the variety of attitudes among scientists is the fact that the current ecology of academic knowledge production is one of multiplying contradictory regimes, logics, or social worlds (for different takes on the theme of multiplicity, see Miller, 2001; Owen-Smith and Powell, 2002; Tuunainen, 2005b; Vallas and Kleinman, 2008).[2] On the individual level, tensions resulting from conflicting social worlds may be experienced by scientists as considerable 'role-strain' (Boardman & Bozeman, 2007).

The bulk of the literature has emphasized how organizations manage such tensions on an administrative level, emphasizing a struggle over resources. In the case of private companies using university resources ('hybrid firms'), tensions may be managed through geographical or physical separation and formal redistribution of academic and corporate roles and resources in an attempt to maintain what are perceived as traditional cultural boundaries (Tuunainen, 2005a, 2005b; Tuunainen and Knuuttila, 2009). In the case of specialized 'boundary organizations' dedicated to coordinating and facilitating research spanning multiple domains (i.e. academia, corporations, and policy), struggles may be managed through the provision of resources and legitimacy for 'hybrid research' and by negotiating multiple stakeholder demands (i.e. Guston, 1999, 2001; Miller, 2001; Parker and Crona, 2012; Wehrens et al., 2013). With its slightly more functionalist slant, the notion of boundary organizations has gained particular popularity, as it asks what conditions enable such 'hybrid spaces' to successfully coordinate and facilitate 'hybrid research.' Interestingly, the literature suggests that boundary organizations, despite their considerable efforts, are rarely successful in resolving occurring tensions in the long run.

We know less about the ways in which scientists deal with conflicting demands on the ground in their everyday work. Accounts of how scientists construct and go about their scientific problems under increasing pressures to make their work relevant for social or commercial purposes are also sometimes difficult to reconcile. For example, while Cooper (2009: 648) argues that "commercially engaged scientists […] are more likely to express the importance of market-oriented solutions," Calvert's (2006) work suggests that scientists might only do so strategically to secure funding, while they continue with their previous lines of work regardless of their funders' demands. On the other hand, Parker and Crona's (2012) study suggests that scientists choose their problems and approaches according to who the most powerful stakeholder is at a given time, perhaps slightly understating scientists' agency and perspectives. The picture painted here is one in which scientists either do what they want regardless of the conflicting demands posed on them, or simply obey the 'most powerful' stakeholder at any given time.[3] What is missing from these accounts is a deeper analysis of how scientists struggle through conflicting demands, how these struggles shape their work and, in turn, what kinds of working processes and objects make navigating conflicting demands more or less feasible. Paying attention to conflicts and processes might also enable us to better understand why scientists position themselves differently under similar conditions, and why this is easier for some more than others.

Social worlds/arenas theory is useful to analyze how scientists navigate what they experience as competing demands, because it focuses on conflict and process, and because it offers a range of sensitizing concepts for the analysis of scientific work (Clarke, 1991; Clarke and Star, 2003, 2008; Gerson, 1983; Strauss, 1991). From an interactionist perspective, academic disciplines and specialties can be viewed as social worlds, as groups which share commitments to common activities, as well as resources and ideologies stipulating how to go about their work (cf. Clarke, 1991: 131; Strauss, 1991). Social worlds lack clear boundaries and can be laced with conflict, but can more or less coincide with formal organizational structures such as university departments. This is a situation where university researchers have to

navigate demands both from their specialty fields and from their respective organizations.

Demands put forth by directed funding schemes, such as the German Security Research Program's demands for applicability and commercialization, can then be viewed as posing another set of constraints on participating university researchers. Since at least the 1990s, long-term funding and numbers of tenured faculty in Germany have declined in relation to student numbers, a development which has in turn increased the importance of third-party funding for faculty to conduct their research and to fund their doctoral candidates and postdoctoral researchers (cf. Kreckel, 2008). If 'soft money' from the government becomes increasingly important to conduct research and fund academic staff, but at the same time is increasingly tied to demands for applicability and commercialization, scientists in Germany are likely to be more amenable to these demands. Because scientific and practical problems are not necessarily congruent, however, current government discourses via directed funding programs turn university researchers' workplaces into an arena rife with potential conflict in which scientists have to balance commitments to their research, their academic careers and political demands for marketable technologies. I therefore understand the commercialization pressures scientists face as a need to simultaneous negotiate multiple commitments in misaligned or competing social worlds.

It is useful to remember that misalignment between scientific work and social worlds is not an unusual feature of scientific work. Scientists routinely have to coordinate their work with their departments, their disciplines, or their funders through a mundane process of continuous reorganization and tinkering (Fujimura, 1987, 1996; also see Knorr Cetina, 1981). This means that, in addition to their intellectual labor, scientists have to "articulate alignment" – "pulling together everything that is needed to carry out production tasks: planning, organizing, monitoring, evaluating, adjusting, coordinating and integrating activities" (Fujimura, 1987: 258). Articulation work feeds back into the construction of scientific problems, creating scientific problems which are 'do-able' (Fujimura, 1987) given available skills and resources, connect to concerns in wider fields of research or disciplines, and are interesting for funders.

Articulating alignment in scientific work is more likely to succeed if abundant resources are available. For example, in cases where demands cannot be reconciled and resources are available, scientists may split and package their work, and outsource undesirable tasks to subcontractors (see i.e. Baumeler, 2009; Fujimura, 1987, 1996). Such divisions of labor allow scientists to pursue their scientific interests while at the same time formally satisfying their funders' demands. However, if the resources to do this are lacking, as was the case in my study, scientists may tailor their research problems to fit the needs of what they see as conflicting demands from misaligned social worlds. Calvert (2006: 208–9) defines tailoring as researchers' efforts to "make their work appear more applied to gain funding and resources."

Extending Calvert's concept of 'tailoring,' I understand it as a specific instance of articulating alignment under conditions which pose strong constraints on articulation work. Tailoring can be generally understood as the mutual translation between researchers' scientific interests and practical problems. There are at least two kinds of tailoring, which are likely to transition into one another iteratively during the research process, but which can be distinguished by their purpose and process. *Forward tailoring* serves to obtain funding by translating practical problems articulated by funders into scientific problems. This is the original meaning of Calvert's definition stated above. The typical case for this kind of tailoring occurred in my study in the process of writing grant proposals for directed funding schemes. However, I also observed a second kind of tailoring, which I term *reverse tailoring*. This strategy reacts to existing research problems which were ill-fitted to the needs of the different social worlds involved in the research process. The typical case for this kind of tailoring occurred in my study if research problems fit the needs of the funders, but not what scientists see as the needs of their discipline. In such cases, scientists translate problems which are interesting to them and feasible with the available skills and resources into new problems which are close enough to what they anticipate

to be the practical problems funders want solved. Reverse tailoring serves to keep existing funding which would be risked if they were to diverge too much from funders' demands, while at the same time allowing scientists to pursue their research interests. Both kinds of tailoring serve to protect researchers' relative autonomy against what they perceive as increased pressures to produce commercial and/or applied research, and, in a reading more focused on power relationships, can thus be understood as a specific kind of 'boundary-work' (Gieryn, 1983, 1995, 1999).

The German Security Research Program

This paper is based on ethnographic fieldwork in which I accompanied a transdisciplinary group of researchers based in universities, research institutes, and companies who were commissioned to develop the software for an automated closed-circuit television system (CCTV) within the German Security Research Program.[4] The researchers tried to mechanize surveillance processes in order for the systems to identify 'dangerous' behavior and situations automatically and in real-time, and to alert the human security staff in such cases. The idea was that operators do not have to watch the screens at all times, but are alerted by the systems in an event of interest.

In its first round (2007–2012), the program has mainly funded the development of security and surveillance technologies. By investing in university and corporate research and development, the program's overall goal is to increase citizens' security, and to strengthen the competitiveness of German medium-sized technology companies on international markets. To ensure that the research meets these goals, the government has formalized its demands in the program's funding requirements and review criteria.

In terms of content, research projects have to clearly outline how they plan to contribute to the solution of national security problems. Mobilizing imageries of crime and terrorism, and referring to the limited capacities of human security staff, the government expects the researchers to develop technical fixes to social problems of crime and terrorism, as well as to increase the efficiency of surveillance processes by mechanizing them:

> Do operators always react instantly when seeing something conspicuous on the screens? Unfortunately not, because it would require a lot of people to monitor 1,700 camera screens. […] In order for the system to detect further – and very diverse – conspicuous events on its own, we need to turn to science. […] The software would have to analyze the passengers' movement in the footage and filter all movements of normal speed. What movements are typical for violent crime? It will be necessary to identify this. There is a lot of work ahead for the researchers.[5] (Bundesregierung, 2011; my translation)

Government expectations concerning crime, terrorism, and security work indicate a shifting political understanding of university researcher's professional 'jurisdictions' (Abbott, 1988). Implicit in expectations to contribute to the solution of security problems is the government's understanding that academic researchers can act as experts on crime and terrorism. Similarly, the government's expectation that new technology should render surveillance processes more efficient and effective assumes that engineers can act as experts in security work.

The government expects researchers not only to assume responsibility for solving security problems, but reframes their work explicitly as an economic activity:

> Through research and innovation, [the Security Research Program] offers the possibility of promoting the competitiveness of the companies involved, as well as their security technologies' marketability, to establish security as a national, locational and economic factor, and to open up possibilities on a European level. (Bundesministerium für Bildung und Forschung, 2007: 7; my translation)[6]

Pressures for commercialization are particularly pertinent to the technological projects funded by the Security Research Program. These expectations are formalized in an explicit obligation to transfer the research into products or patents ("*Verwertungspflicht*"), thus encouraging researchers to orient their work towards economic growth and international competitiveness.

In terms of organization, research projects are required to work in a transdisciplinary fashion,

collaborating not only across disciplines, but also with end users and small and medium enterprises. In order to shorten the duration of technology transfer from research to market, the government has formalized the involvement of small and medium enterprises in its funding requirements. By incorporating both end users and industry, the government hopes to ensure the development of useful technologies.

Finally, particularly with controversial technologies – surveillance technologies being a prime example – the government has incorporated additional reflexive mechanisms to account for potential undesirable consequences, perhaps also for reasons of legitimacy. Because the program puts heavy emphasis on applicability and commercialization, the government expects research projects to calculate the possible social consequences of the security technologies' use. In order to monitor the projects for possible undesirable implications, the government has made it mandatory for technological projects to work with social scientists or legal and ethics scholars.

The Security Research Program's criteria are put through an altered review and selection process which differs significantly from traditional peer review. Instead of recruiting reviewers from within academia, and selecting them according to their specialties, it outsources the review and supervision of projects to a spin-off organization of the Association of German Engineers (VDI). Employees of this organization are responsible for both reviewing grant proposals and monitoring projects. Although some of them have a doctoral degree in the natural or engineering sciences, they have left their academic career path to be employed full-time by this organization. Once these employees have made their initial selection of grant proposals, they forward the project proposals to the Federal Ministry of Education and Research for final approval. The way in which the Security Research Program structures its review process and project supervision thus shifts discretion from academic review panels ('traditional' peer review) to bureaucratic entities, and can be read as the German government's expansion of social control in order to protect its investments.

Developing a 'smart' CCTV system

The researchers in my study applied to the program by proposing to develop the software for an automated CCTV system. University researchers included computer scientists, geoscientists, electrical engineers and legal scholars. Furthermore, the project included members of two private research institutes who were mainly computer scientists by training. On the corporate side, the project comprised a consulting agency that carried out cost-benefit analyses and an IT company which was supposed to integrate the system for technology transfer. Finally, the project included two officers from regional police crime units, who were expected to share their expertise in detecting criminal behavior. The project was relatively large, and at different times involved between 25 and 30 members, about half of whom were university researchers. In my analysis, I have focused on the university researchers involved in the project. Thus, when in the remainder of this paper I refer to researchers, I mean the project's senior scientists on the faculty level, as well as their doctoral candidates, all based in different universities across Germany. I have substituted all names, places, and unique technical terms with pseudonyms.

The group's goal outlined in the grant proposal was to mechanize surveillance processes in order for the system to identify 'dangerous' situations automatically and in real-time. Their idea was that operators do not have to watch the screens at all times, but are alerted by the system to an event of interest. They argued that their surveillance system, in contrast to non-automated CCTV systems, would facilitate intervention before the fact, and would also reduce personnel cost through automation.

The Security Research Program, as outlined above, expected the group to develop technical fixes to social problems of crime and terrorism, and to increase the efficiency of surveillance processes. Furthermore, they expected the group to consider privacy regulations in the system's design. These expectations refer to two separate groups of actors: solving problems of crime and terrorism and considering privacy regulation both refer to monitored individuals, while increasing the efficiency of surveillance work refers to human operators and security staff. In what follows,

therefore, I show how the researchers navigated expectations from academia, the government, and the wider public in their work by analyzing how the researchers classified deviance and conformity of monitored groups, and how they mechanized the work of human operators.

The selective memory of 'smart' CCTV

The German government expected the research group to consider possible undesirable consequences of their surveillance system's use. As in most technological projects funded by the program, this meant reducing all possible social implications to data protection issues. Data protection guidelines are relatively well institutionalized in Germany's legal code. Video footage may usually be stored up to 24 hours; longer storage is only permitted in case of a reported criminal incident. To account for privacy rights, the Security Research Program has made it mandatory for developers and legal scholars to collaborate.

Over the course of the project, the researchers never openly questioned whether the expectation of "privacy-friendly security solutions" (Bundesministerium für Bildung und Forschung, 2012: 7) was a legitimate one, but, on the contrary, situated themselves as researchers sensitive to the risks of privacy violations. However, they did struggle intensely with the negative public responses to their work. All researchers were acutely aware that privacy in relation to surveillance technology is a highly controversial issue of public debate in the German media landscape. They actively monitored the criticisms of their work in the wider public sphere, which framed their work as a violation of privacy rights, and public responses to their work were a frequent topic of conversation throughout the project. Furthermore, many, particularly the junior researchers, struggled with the deeply political nature of their project. As Martin, the project's principal investigator explained:

> Personally, my assessment is that in Germany, people are very critical towards new technologies. That isn't only true for video surveillance [...] you can observe very critical attitudes in many areas which, to be sure, in many cases are justified. And I don't want to say that you have to accept everything uncritically, but the range is relatively broad […] I don't want to say it's better in other countries where it's perceived less critically, but it's a broad area – let's not discuss this too politically now. (Interview with principal investigator Martin, January 2011)

We can see that Martin is pulled in different directions by what he perceives as conflicting demands from the government and the wider public: While the government expects the group to contribute to public and private surveillance, he assumes that part of the public condemns the development of new surveillance technology. On the one hand, he recognizes that critical engagement with surveillance technology is necessary while, on the other hand, he cannot delegitimize his own work. Even though the researchers decided to build privacy-by-design measures into their system, the fact remained that ostensibly they were developing surveillance technology and thus contributing to public and private surveillance. His struggles were rooted in his personal political stances, as well as his commitment to his work.

Such tensions between conflicting expectations from the government and the wider public, as well as researchers' own ambivalence about surveillance resulted in ambivalence about whether or not they should include social issues as a legitimate part of their work. This is exemplified in how the researchers tried to explain their consideration of privacy regulation in the project:

> I already mentioned our colleagues in the data protection area. I mean, potentially, [the system] produces a large amount of personally identifiable data. Someone has to explain that to us engineers, because if you're not an expert you won't know if these are personally identifiable data or not […] so we're frequently discussing and thinking about how we can design [the system] technically in a way that data protection problems don't occur in the first place. (Interview with principal investigator Martin, January 2011)

> At this point, I can already reveal [that] we have a special legal division here with us in the project. [...] I mean, they're specifically here to advise us, well, in our scientific ambition, not to do stuff that legislation explicitly prohibits. So we have to see that we somehow don't gather – what do you call that? – personally identifiable data. That means we

> have to, at the point where we collect data that in the end points to one specific person or thing – because certain regularities are saved too exactly – we want to try to make it so that the data base we create can't be used with abusive intentions, I dunno, to somehow discriminate against people. (Interview with doctoral candidate Robin, January 2011)

These quotes show that, on the one hand, the researchers tried to position themselves as sensitive towards possible undesirable consequences of their work by demonstrating that the group built privacy regulations into the surveillance system's design. To some extent, they broke down distinctions between 'technical' and 'social' problems, thus creating overlaps between the worlds of law and engineering. On the other hand, they point out that their work is controlled by 'external,' competent authorities. This is particularly clear in Robin's statement: Although the legal scholars were formal members of the research project, Robin situated them as external to the project, because he did not understand them as part of the "scientific, ambitious" collective identity which developed the system. By underlining external authorities, he also drew a line between the researchers who follow their 'scientific curiosity' in a sheltered university environment, and the legal advisors as experts for the real world 'out there.'

The researchers resolved conflicting expectations from the government and the wider public by assuming partial responsibility for possible undesirable consequences of the surveillance system's use. In collaboration with the legal scholars in the project, they decided to 'inscribe' (Akrich, 1992) privacy regulations into the surveillance system by minimizing the personally identifiable data – the actual video footage. This means that they discarded any actual video footage immediately after analyzing it, which would only take a few seconds. While there would be a live feed from the video cameras, surveillance staff would not be able to go back and sift through the footage to look for specific people and events. The researchers thus excluded information about single individuals from the database, and embedded 'memory practices' (Bowker, 2008) into the surveillance system that prescribed individuals' identities as irrelevant to surveillance processes. This is how the system's memory is "selective": As a consequence of the researchers' negotiation of conflicting expectations from the government and the wider public, only the temporal and spatial qualities of monitored individuals' movement remained. Thus, boundaries between legitimate and illegitimate tasks could only be drawn rhetorically, while in their work on the system there was no other option than to give way to pressures to consider possible undesirable consequences of their work. Following Latour (1993), the way in which they dealt with what they perceived as the critical wider public can be described as rhetorical 'purification,' which could not be maintained in their work on the ground.

Classifying 'dangerous' behavior

Because the government expected the research group to develop a technological fix to problems of crime and terrorism, the group had to classify 'dangerous' behavior in order to code it into the surveillance system (cf. Bowker and Star, 2000). The embedding of privacy regulations was consequential for how the researchers built concepts of deviance and conformity into the surveillance system. Because they only kept computer-generated trajectories of movement, they needed to come up with a theory of how to read dangerous behavior from nothing more than a movement pattern.

For the researchers, defining crime for the purposes of their surveillance system was highly problematic for different reasons. Robin, who was primarily responsible for the behavioral analysis component of the software, told me about the problems that emerged when he tried to obtain knowledge about 'dangerous' behavior from the police officers. He told me that the officers had handed him a list of 43 different dangerous situations that they would have liked detected by the surveillance system. This list included situations as diverse as people running into train tracks, drug trafficking, suitcase bombs, and assault and battery. Robin was not very happy about the officers' insights, and strongly problematized the indexicality (Garfinkel, 1967) of social behavior, which can only be meaningfully understood in context and specific situations:

> So the guy who drops a suitcase bomb, right? He'll be damned if he danced around before planting his bomb somewhere, he'll just walk past and discreetly leave the suitcase […] so I have problems with the very interpretation of behavior, because how can we project this merely visually detectable behavior onto some concrete intention? For instance, this here's a culprit and this is a normal passer-by. Well that's simply not quite possible without problems. […] We can't say every time someone zigzags that's a bomber or something. That means some things we're simply not allowed to do and certain things we're just not capable of doing. (Interview with doctoral candidate Robin, January 2011)

For Robin, crime and terrorism were not so much social problems to which he wanted to contribute a solution. Rather, crime or criminal behavior presented itself as a practical problem for his work. Particularly, and Robin repeated this throughout the following months, he did not see himself as *professionally competent* to define and code dangerous behavior:

> Drug dealing? Well, I have to admit with drug dealing we don't stand a chance except if people act particularly stupid somehow. The only thing that happens with drug dealing, so first [there] is the typical exchange: Two people meet physically, well they're at the same place at the same time. We can detect things like that, the problem is just that [with this procedure] we automatically suspect everyone else in the scene whose paths cross for whatever reason, right? […] we can't just say here, the typical drug deal has the duration of ten seconds [and] all other interactions take much, much longer, right? Then we'd stand a chance but, *who's supposed to decide this?* (Interview with doctoral candidate Robin, January 2011; my emphasis)

Robin did not perceive himself as professionally qualified to decide what might still count as 'normal' and what might already count as 'deviant' behavior; more importantly, he did not want to assume responsibility for such decisions, either. According to Gerson (1983: 367), questioning whether or not specific problems are a part of one's work is a typical indicator for problems of legitimacy: "The emergence of a new segment or intersection […] always raises the question: 'Is this new way really part of our work? Is it really X-ology?' Such questions are the essence of issues of problem legitimacy." Robin decided that defining dangerous behavior was not a legitimate part of his work, and forwent the original proposal's plan to classify different types of dangerous behavior. In contrast to the researchers' negotiations of privacy, there is little ambivalence about whether or not defining deviance and conformity was part of his job: Robin clearly rejected government expectations to act as an expert on crime and terrorism.

A couple of months later, I had the opportunity to learn more about how dangerous behavior fit into the project. I was invited to a meeting where all project partners presented the state of their research to the funding institution's representatives and discussed further steps. After all partners were finished with their presentations, the principal investigator of the legal unit pointed out that the researchers had not explained how they wanted to achieve the detection of dangerous behavior. He noted that this posed a problem to his work, as he needed to know the CCTV system's specific procedures in order to evaluate whether they were legal according to current legislation. Robin and Max, another geosciences doctoral candidate, sat next to me, disgruntled. Robin moved closer and whispered that he was scared of being forced to integrate even more problematic system functions into his already problematic work. As a result of the legal professor's request, and after some perplexity among the rest of the university researchers, the principal investigator decided to split up all participants into groups to discuss different dangerous situations.

Since I was particularly interested in the interaction between the researchers and the police, I followed the group which included Mr. Weber, one of the crime unit officers. The group hesitantly began to discuss the "storyline" of a situation in which the system might be used – note that, at this point, the project had already been running for almost a year. The group did not get much further than deciding the scenario's location (a train station), and the discussion was frequently interrupted by awkward silences. While the principal investigator tried to keep up the discussion, I

noticed that Mr. Weber remained silent. I found this strange because I expected this scenario to be his area of expertise, so I was surprised that he did not provide the researchers with more information about what it is like to survey a crowded train station. I was not the only one to notice this, and as the conversation came to a halt, the project coordinator turned to Mr. Weber and asked: "Mr. Weber, why don't you tell us how you in your work know when someone's up to something? You have the practical experience…"The group looked at Mr. Weber with undivided attention. Mr. Weber shrugged uncomfortably and responded: "Well, yeah, that would be great if you could deduce certain behavior from movement patterns..." This surprised one researcher named Jonas, who moved abruptly toward the officer and cried out: "Oh, so you don't know either!?" The officer said nothing and the group mumbled through the awkward moment (field notes, May 2011).

After one year into the research project, 'dangerous behavior' – the very linchpin of the project – turned out to be an empty signifier. On one hand, the police officer could not turn his implicit police knowledge into knowledge explicit enough to translate into machine-readable code (Collins, 2010: 138). The researchers, on the other hand, did not see themselves as professionally competent to define dangerous behavior. But what struck me was not that they both were not able to create a workable classification system of dangerous behavior, but that they left this issue unresolved, and that the university researchers did not seem to care too much about it. To the university researchers, defining dangerous behavior simply seemed not to be the most important or interesting part of their work. This shows how the government's expectations that they act as experts on crime and terrorism did not align with what the university researchers viewed as interesting research problems.

However, Robin still had to code a concept of deviance and conformity into the surveillance system, because this was what he committed to do when he signed up for the project. How did Robin achieve this? He translated the problems formulated in the grant proposal into problems that he felt actually able to solve by using techniques from his discipline with which he was already familiar. This means that he constructed 'do-able problems' (Fujimura, 1987) by modifying existing algorithms he had already worked with at his department. By using these algorithms, Robin created his own theory of dangerous behavior. More precisely, he borrowed from a project that developed GPS technology in order for biologists to track seagulls and map their flying routes. These seagull data indicated the individual seagulls' coordinates at any given moment – hence their movement trajectories were stripped of everything but their spatial and temporal qualities. Biologists could, for instance, see where the majority of the flock was, and where some seagulls strayed from it. As he explained to me later, the seagull movement became, per analogy, his theory of deviant behavior:

> This isn't about dangerous behavior. I can't say anything about that. I can only make statements about what's significantly different. So what I ask is: What does everyone do in this situation? Everything other than that is significantly different. (Doctoral candidate Robin; field notes, May 2011)

Robin redefined the surveillance system's objectives from detecting "dangerous" behavior to detecting "significantly different" behavior, which might also be dangerous. His modified algorithm detected patterns of aggregated movements across the monitored space, thus analyzing "what most people do." He assumed that when people behave significantly differently than others, then there is an increased chance that these people are exhibiting the kind of behavior the system was supposed to detect. His theory was thus that 'conformity' means 'what most people do' and deviance is everything else, which means that the software detected not dangerous behavior, but risky behavior. Thus, he inscribed a binary classification of deviance and conformity which was based on statistical normalcy. The question of margins – what should still count as normal and what should count as deviant – was displaced by Robin to a hypothetical end user in an unknown future. As he told me later: "We are engineers, we don't want to assume responsibility for definitive decisions over dangerous behavior" (field notes, April 2012).

Robin's problems show how he struggled with conflicting understandings of his work: On the one hand, the government expected him to act as an expert on crime and terrorism, while on the other hand, he viewed defining deviance and conformity neither as a legitimate part of his work, nor as an interesting research problem. But, because he was committed to both the research project and his field of research, he had to find a way to satisfy the requirements of both worlds. He did so by adapting his theory to existing research, which offered him a sufficiently explicit conceptual foundation to solve two separate problems. First, his seagull theory allowed him to continue his work – which was primarily his doctoral dissertation, while secondly being close enough to the original plan to be interpreted by the funding institution as the execution of his commissioned research. Following Star and Griesemer (1989: 393), the seagull theory of deviant behavior can thus be described as a boundary object. Boundary objects have "different meanings in different social worlds, but their structure is common enough to more than one world to make them recognizable, a means of translation" (Star and Griesemer, 1989: 393). His seagull theory allowed Robin to balance government expectations of developing security technology and disciplinary expectations of developing a legitimate topic for his doctoral dissertation. But, following Clarke (1998: 7–8), we could also say that Robin's seagull theory *disciplines* his work in two ways: On the one hand, it aligns his work with the wider research in his department and, on the other hand, this alignment indicates that his discipline tends to bear greater control over his work than the government's demands.

Configuring surveillance operators

The Security Research Program expected the group to increase the efficiency of surveillance processes by partially mechanizing them. This means that the researchers configured how surveillance operators and security personnel would use the CCTV system (cf. Woolgar 1991; Hanseth and Monteiro, 1997), including the ways in which they would observe people and move through the monitored space. The system architecture played a major role in configuring these surveillance practices. It did so by ordering the relationships between the infrastructural components into a hierarchy – cameras, servers, storage, mobile devices, security staff, and communication protocols, among other factors.

One example of this hierarchical ordering is the way in which the researchers conceptualized the cameras as a self-organizing, decentralized, and autonomously communicating network. The idea was that the network would automatically compute the maximum coverage of the monitored space with a given number of cameras. Delegating parts of the observation to the CCTV system was supposed to compensate the limited attention span of surveillance operators: The users had only to act on their own discretion when the CCTV system detected something out of the ordinary and sent an alert to the user's screen. The user's job as defined by the group was to qualify the alert by deciding whether there was a reason to intervene. It was not the system's users who were supposed to control the cameras, but the camera network itself. Thus, the researchers distributed surveillance processes between technology and users by assigning significant parts of the observational work to the surveillance system, leaving the human operators with the task of decision making.

However, as Kai – a computer science doctoral candidate – explained to me, his preference for self-organizing networks over a manually controlled network was the mathematical problem at the core of it. The autonomous network was a modification of a geometrical problem known as the "art gallery problem." What Kai found exciting about this problem, as he explained to me, was that the problem was not unambiguously solvable, but that its solution could only be approximated with algorithms. If the maximum coverage could only be approximated, it meant that Kai also accepted the risks of potential instability. What seemed to be more important to Kai was the question whether the underlying problem was interesting against the backdrop of his department's line of research, while he never really talked about what the self-organizing network would do to render surveillance processes more efficient. Although the government expected the group to make surveillance processes more efficient, we can

see how questions of applicability faded into the background in their day-to-day work.

The preference for 'admirable problems' is even clearer in how the researchers from the department responsible for the system architecture dealt with questions of system stability. They originally designed the CCTV system as a (more or less) decentralized network to secure its stability. The idea was that if one part was damaged for whatever reason, the remaining components of the CCTV system would continue working and avoid a total breakdown. However, Kai explained that this architecture was by no means a guarantee of stability, and acknowledged that there were much more practical and applicable solutions. For instance, they could have built a centralized system and physically secured the center. This would not only have been just as effective, but also much more economical than the solution they had proposed. However,

> Securing the center would have been much cheaper, but not as interesting as a research topic. But, you know, it's not that important to me that people use it anyway. I actually wouldn't like it very much if the system worked, I mean, if the state monitored us. I just have an interest in it as a researcher. If I owned a house I'd set up a [CCTV] system right away, but if the state did it I'd be against it. (Doctoral candidate Kai; field notes, April 2012)

Kai knew that there were more practical and cheaper solutions to problems of stability. However, he was writing his doctoral dissertation for one of the participating computer science departments, which focuses on self-organizing, decentralized system architectures. Designing the CCTV system as a decentralized network aligned with the department's work and was considered a recognized research topic for an academic audience. For Kai and the other project members from his department, the recognition of their work by an academic audience thus seemed to be more relevant than that of the funding institution.

Kai's view of working at a university differed starkly from that of the government: While the government within the Security Research funding scheme framed university research explicitly as an economic activity, Kai drew a sharp distinction between what he viewed as academic and industrial research:

> In science, you can basically do what you want. In the industry, you won't be able to follow your interests; they'd never build the kind of system we're developing. Here, we're able to experiment, which wouldn't be possible in the industry – they'd bite your head off if you'd propose a concept like ours. (Doctoral candidate Kai, field notes April 2012)

While Kai surely plays down the structural constraints of research at universities, his statement shows that he, too, rejected the government's expectation that he act as an expert in surveillance work, a perspective which was shared by all of the doctoral candidates in the project.

The way in which the researchers configured surveillance operators again shows conflicting understandings of their work: While the government expected the researchers to make surveillance processes more efficient, for the scientists responsible for this task, this was not interesting enough as a research problem. But, because they were committed to both the research project and their field of research, they had to satisfy the requirements of both the government and their disciplines. They did so by translating between a practical problem (a functioning and stable CCTV system) and their own research interests (distributed algorithms for decentralized system architectures). However, Kai's case shows a much more pragmatic approach than Robin's: While Robin had to translate the grant proposal into doable problems when he realized that they were ill-fitted to satisfy the requirements of his discipline, Kai's supervisor had already created a problem while writing the grant proposal already which was both recognizable as a relevant practical problem to the funding institution, as well as an as an interesting research topic to them and their department colleagues.

Tailoring is invisible work

The Security Research Program expanded social control into university researchers' work by stipulating the purpose and social organization of their work: They were to contribute to the solution of

security problems and collaborate in a transdisciplinary fashion. The university researchers in my study then had to balance commitments to their government commission, their disciplines, and the wider public, which were often at odds with each other. What allowed them to navigate these conflicting expectations was their ability to create research problems that fell into their departments' previous lines of research, but could also be interpreted as practical problems pertinent to surveillance systems. This practice is nicely captured by Calvert (2006: 208–209) as research tailoring, which she defines as making one's work "appear more applied to gain funding and resources."

Tailoring was crucial to "keep politics near enough" (Gieryn, 1995) to secure the researchers' funding, but "not too close" to interfere with their research interests. Their tailoring practices can thus be described as 'boundary-work' (Gieryn, 1983, 1995, 1999), because it served to protect their relative autonomy against the expansion of government control. However, unlike other research on multiple commitments in academic research, they did not protect their work from government oversight by quarreling with the funding institution about the legitimate boundaries of their work (cf. Gieryn, 1999; Jasanoff, 1990; Wehrens et al., 2013).[7] On the contrary, this type of boundary-work was reliant on the avoidance of conflict. It was thus not open boundary disputes which allowed them to manage their proximity to politics, but their carefully tailored research objects.

Based on my study, we can add a few points to Calvert's definition of tailoring. First, the purpose of tailoring is not only to gain funding, but also to secure existing funding. This is exemplified in the differences between Kai's and Robin's cases. In Kai's case, the tailoring could be termed 'forward tailoring,' because the translation was done in the grant proposal to attract funding, and then carried on throughout the entirety of the research process. This was a common and surprisingly open practice, as indicated in my field notes:

> The group is discussing possible ideas for a successive grant proposal within the Security Research Program. That is, the professors are talking while the doctoral candidates listen or work on their laptops. […] Martin [the principal investigator] jumps up and draws a table on the whiteboard. "We have to distinguish this – one is the paper perspective, the presentation perspective is another thing," and he fills out one column with application scenarios, and the other column with their corresponding research areas. "The story has to start with the user," he explains. On Martin's suggestion, the group decides that the consulting agency use their contacts in public transportation to find out whether they have "shopping lists" in order to develop the grant proposal from there. (Field notes, May 2012)

By contrast, Robin's case could be termed 'reverse tailoring.' He realized during the research process that the problem outlined in the grant proposal and his research interests were ill-fitted. But, because the government monitored the project's progress in intervals of six months and reserved the right to terminate funding if it evaluated the project as failing its goals, he needed to construct a new problem close enough to the original commission to satisfy the funding requirements. He did so in reverse, by defining the new problem in terms of its available solutions. Reverse tailoring was a strategy which drew significantly more resources than forward tailoring, because it necessitated continual adjusting, both rhetorically and in practice.

Second, the varying amount of work which went into tailoring their research also accounted for the varying degree to which the researchers experienced role conflict: Researchers who could work with problems which were well-fitted from the beginning moved with much more ease between social worlds. These researchers experienced their multiple commitments to the project, their departments, and the government's demands as less problematic than did researchers who had to work with ill-fitted research problems. This is again clear in contrasting Robin's and Kai's cases: While Kai could more or less straightforwardly carry out his part of the project, Robin struggled greatly throughout the project. Whether or not scientists' balancing acts become stabilized thus seems to be strongly linked to the ways in which research problems are structured: Although in both Robin's and Kai's cases demands were misaligned, it was certainly easier for Kai to navigate them than for Robin.

Third, in contrast to Calvert's (2006) assessment, tailoring was neither a single event during the research process, nor mere 'window dressing' which just portrayed their work as security research in order to obtain funding. Rather, it was a *continuous* negotiation to align their commitments to both their fields of research and the government program, and in some cases it required a tremendous amount of work.

The work that this tailoring required was 'invisible work' (Star and Strauss, 1999). This means that it was illegitimate work from the perspective of the funding institution and needed to be hidden (Möllers, 2016). If working within the framework of the Security Research Program indeed meant this amount of invisible work, why did they then apply to the program in the first place? The reasons the university researchers gave me in response to this question were strongly related to structural working conditions at German universities, rather than to the content of their work. Again, from my field notes:

> I'm outside with Martin [the principal investigator] and Robin [a doctoral candidate] for a smoke. I ask them why they applied to the Security Research Program, and how they designed this sort of huge, transdisciplinary project. Martin responds: "You need a lot of imagination to apply for a grant. This is a sort of top-down process; while you're working on one problem, new problems occur, which gives you reason to apply for another grant." Robin adds: "Well, and the grant proposals are mainly written to secure funding for the doctoral candidates." (Field notes, May 2011)

The rollback of long-term funding and the decline in tenured positions in relation to student numbers at German universities have opened way to an increasing number of short-term positions and precarious working conditions (Kreckel, 2008). For the senior scientists in my study, continuously producing grant applications was an acceptable and common remedy to the problem of securing funding for their doctoral candidates and post-docs. This arrangement is also evidenced by the high fluctuation of doctoral candidates and post-docs throughout the project duration: The researchers who had worked on the original project proposals usually left the project once they completed their dissertations. New doctoral candidates took their place, using the project to write their own dissertations.

Conclusion

The group never ended up transferring their work into a functioning and marketable surveillance system, despite the German government's significant expansion of control over the group's work. Neither its requirements in terms of content and organization of the group's research, nor the regular monitoring of the project's progress, nor even provisions to terminate funding in the case of negative evaluations at all led to commercialization. While this shows that scientists seem to have some leeway in finding creative workarounds, this does not mean that they do not, occasionally, struggle greatly with the constraints posed on them by directed funding schemes. Rather, the ways in which scientists struggle through conflicting demands shape their scientific work, just as the ways in which scientific problems are constructed shape the extent of their struggles.

To be sure, this was not simply a case of 'bad science.' The senior university researchers involved in the project were all respected scholars in their fields. Their reputation is also indicated by the fact that, during the project, they published several peer-refereed articles in international journals, and regularly presented peer-refereed papers at international conferences. Furthermore, all participating senior scholars, either during or after the project, were able to obtain the prestigious grants from the German Research Foundation (DFG), which have to undergo a rigorous peer-review process. However, *saying* "development" and *doing* "papers" and "grants" was viewed as the better long-term strategy for those who worked within an academic reward system.

My study reaffirms the need to remain attentive to the potentially multiplying lines of conflict researchers face in the midst of changing relationships between universities, governments, and industry. There was more at stake for the researchers than 'just' balancing their research and academic careers with the government's requirements. All of the university researchers were acutely aware of the deeply political nature

of their work, as it related to highly controversial issues such as surveillance, discrimination, and privacy. Furthermore, personal struggles with surveillance technology were a shared issue among some of the doctoral candidates, and were importantly rooted in their personal political stances and commitments to the general public. It is thus important to pay attention to the multiplying demands (cf. Vallas and Kleinman, 2008; Tuunainen, 2005b; Owen-Smith and Powell, 2002) scientists have to deal with in their day-to-day work in order to gain a richer understanding of scientific work under increasing commercialization pressures. However, this should not only include scientists' attitudes towards commercialization pressures, but, importantly, also the practices by which they 'make it work' despite the potential for conflict. We need more analyses of the way in which scientists struggle through conflicting demands, how these struggles shape their work, and, in turn, what kinds of working processes and objects make navigating conflicting demands more or less feasible.

Not accounting for the multiplicity of constraints that university researchers face might also too easily obscure the social and structural conditions of their work. The amount of invisible work which went into their tailoring practices shows just how strongly they were being pushed and pulled in different directions by the government, academia, and the wider public. The researchers' reasons for applying to the Security Research Program despite these problems were, in turn, strongly tied to structural working conditions at German universities. Consequently recognizing that tailoring practices are to a certain extent a product of powerful misaligned or competing social worlds has implications for science policy. There are good reasons for governments to ask universities to contribute their expertise to the solution of societal problems, and good reasons to ask scientists to be accountable to citizens. However, my study indicates that this might be difficult to accomplish in a meaningful way if academic institutions do not reward the solution of practical problems, or if directed funding schemes ask scientists to engage in highly controversial activities.

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Notes

1 Theories of 'radical changes' in knowledge production share conceptually problematic assumptions which render them theoretically inadequate for the analysis of knowledge production. The models' claims of change rest on distinctions between 'old' and 'new' forms of knowledge production, which they tend to equate with 'basic' and 'applied' research. Despite occasional cautionary warnings, these dichotomies also persist in some of the empirical work on boundary organizations (although now couched in a different language which implies that 'hybrid' research and spaces were not hybrid 'before'). Both distinctions, as well as their equation, are quite problematic. The major pitfall is that they frame science in essentialist terms, a conceptualization which has received much scrutiny within previous STS work. For example, Gieryn's (1983, 1995, 1999) seminal work has shown that boundaries between science and non-science, or science and politics, are culturally and historically variable and relative to institutional and organizational contexts. Gieryn concludes that only little can be said about an essential core of science, which leaves us to understand it simply as consisting of scientists' practical accomplishments. In fact, science seems to have always been an 'impure' hybrid (Latour, 1993), which suggests that, historically, there might be more continuities than the models assume (see also Etzkowitz & Leydesdorff, 2000; Fuller, 2000). If no stable core of science exists, then distinctions between 'old' and 'new', 'basic' and 'applied', or 'pure' and 'hybrid' research seem fairly inappropriate in the attempt to understand the consequences of current pressures towards commercialization.

2 For a comment on their varying degrees of structuralism, see Jones (2009).

3 Quite a few of the empirical studies discussed above talk about scientists' "strategies" or "practices," but I feel that we end up not knowing very much about what scientists actually do in their work. This is perhaps also due to the fact that many studies are solely grounded in interview data. Had I merely analyzed the interviews I conducted at the beginning of my fieldwork, I too would be telling a different story. What the literature on both management and scientists' perspectives seems to routinely miss is that people often do things that are different from what they say they do (i.e. saying "system development" but doing "academic papers." See Khan and Jerolmack, 2013; Jerolmack & Khan, 2014 for an insightful discussion of what they call the problem of "attitudinal fallacy").

4 I gained access to the group as an embedded researcher. My task was to analyze automated surveillance systems for 'social implications' which included, for example, controlling for bias, or questions of public perceptions of video surveillance. My role in the field shaped the focus of my observations, in that I followed the work (cf. Marcus, 1995), and the work was mainly carried out by the junior researchers in the project. The group had at different times between 25 and 30 members, about half of them university researchers. My material is ethnographic, which means that it includes (1) observations, (2) in-depth interviews with the scientists, (3) documents which the scientists produced in the research process (i.e. grant proposals, papers, presentation slides), and (4) government documents for the funding scheme. After initial in-depth interviews, I carried out fieldwork with the research group over the course of two years. Interviews were unstructured and lasted between 60 and 120 minutes. I then observed project meetings which were held on average every two months for 2–5 days in different places in Germany. These included (1) meetings where (mostly the junior) researchers assembled the work of the past months ("hackathons"); (2) meetings where all project members presented the state of their research to the funding institution; (3) and two public demonstrations of prototypes. My analysis was guided by strategies developed in grounded theory and situational analysis (Clarke, 2005; Strauss & Corbin, 2008), using concepts developed in interactionist STS as sensitizing heuristics. All material is in German, and was translated by me for the purpose of this paper. I have substituted all names, places, and unique technical terms with a pseudonym.

5 "Reagieren Sicherheitskräfte immer sofort, wenn sie etwas Auffälliges auf dem Monitor sehen? Leider nicht, denn um 1.700 Kamerabilder ständig zu überwachen, brauchte es viele Menschen. […] Damit das System von selbst weitere – und sehr verschiedene – Auffälligkeiten erkennen kann, ist die Wissenschaft gefragt. […]Also wozu dann die Kameras? Zunächst dazu, um den Täter zu identifizieren und zu fassen.

Dem Opfer hilft dies allerdings wenig. Auch hier wäre Bilderkennung erforderlich. Und daran wird tatsächlich gearbeitet. Das Programm müsste die Bewegungen des Videobildes auswerten und alle normal schnellen Bewegungen der Fahrgäste herausfiltern. Welche Bewegungen sind typisch für eine Gewalttat? Diese wären zu identifizieren. Viel Arbeit für die Forscherinnen und Forscher" (Bundesregierung, 2011).

6 "[Das Sicherheitsforschungsprogramm] bietet die Möglichkeit, durch Forschung und Innovation die Wettbewerbsfähigkeit der beteiligten Unternehmen und die Marktfähigkeit der von ihnen erarbeiteten sicherheitstechnischen Lösungen zu fördern, Sicherheit als nationalen Standort und Wirtschaftsfaktor zu etablieren und Gestaltungsspielräume auf europäischer Ebene zu eröffnen" (Bundesministerium für Bildung und Forschung, 2007: 7).

7 Boundary-work can mean both drawing and blurring boundaries. In Gieryn's (1999) study, scientists draw boundaries to protect their autonomy against what they view as 'outside threats'; and they blur boundaries in order to claim authority over new subjects of research ('expansion'). This does not quite apply to my case: Although the researchers in my study rhetorically blurred the boundary when communicating with the funders, they did so to protect their autonomy, not to expand their authority into surveillance technology markets – after all, they never pursued technology transfer.

The Universities' Transformation Thesis Revisited: A Case Study of the Relationship Between Nursing Science and Society

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Abstract

The universities' transformation thesis is often used to characterise the relationship between universities and society. It posits that, over the past few decades, universities have shifted from ivory tower research and disciplinary-based knowledge production to more and more active interaction with the surrounding society and towards transdisciplinary and problem-based knowledge production that targets solving the big problems of our time. The article revisits the transformation thesis in the context of Finnish nursing science at a time when this discipline was emerging at universities and the central arguments of the transformation thesis were formed. Using the social worlds framework, the article analyses the relationship between nursing science and society from the point of view of different social worlds and argues that the transformation thesis only partially captures these perceptions of the relationship between nursing science and society. Finally, the article proposes some other literature to be used in analysing universities' interaction with society and particularly with the profession-oriented disciplines.

Keywords: science-society relationship, nursing science, universities' transformation

Introduction

The universities' transformation thesis is often used to characterise the relationship between universities and society. It posits that, over the past few decades, universities have shifted from ivory tower research and disciplinary-based knowledge production to more and more active interaction with the surrounding society and towards transdisciplinary and problem-based knowledge production that targets at solving the big problems of our time (Slaughter and Leslie, 1997; Gibbons et al, 1994; Etzkowitz and Leydesdorff, 2000; for an overview, see Hessels and van Lente, 2008). The thesis has been criticised for emphasising science, technology and medicine and not covering the various disciplines of the humanities and social sciences (Albert, 2003; Godin, 1998). It has also been claimed that the thesis is poorly grounded in empirical research in individual disciplines and concrete university contexts (Ylijoki, 2003; Tuunainen, 2005a, 2005b; Ylijoki et al., 2011; Albert and McGuire, 2014). Inspired by these critiques, this article sets out to investigate the relation-

ship between science and society in the discipline nursing science, which represents applied social sciences, and has been discussed very little in the context of the transformation thesis.

The central idea of the transformation thesis is that the relationship between universities and society is changing. This change is illustrated by a contrast between two modes of knowledge production. Mode 1 knowledge production is characterised as autonomous science that is free from outside interests and which concentrates on research questions that arise from science. Mode 2 knowledge production is, by contrast, characterised as research activity that happens in an interaction with society and tries to answer questions that stem from societal needs and ideas. An essential distinction between these two modes is that knowledge produced in the first mode is found to be applicable within the context of a single discipline, but the latter is useful to broader society, including industry and government. Also, in the former, knowledge is produced within the hierarchical university system, whereas in the latter knowledge is produced in multiple arenas and the university is only one site of activity (Gibbons et al., 1994: 3-4). Transdisciplinarity is characteristic of Mode 2 knowledge production, meaning that many disciplines participate in the production of knowledge, but Mode 1 knowledge production happens within an individual discipline[1]. In fact, the claim includes that many actors participate in Mode 2, and consequently, this type of knowledge production has broad effects on the economic development of nation states and regions (Etzkowitz and Leydesdorff, 1998: 204, 2000: 117). The change in knowledge production, it is argued, results in more and more expectations from various directions, pointing to a more reflexive science that would respond to societal needs (Gibbons et al., 1994; Etzkowitz and Leydesdorff, 1998, 2000; Slaughter and Leslie, 1997; Hessels and van Lente, 2008).

This article brings the transformation thesis into the domain of the newly academic Finnish nursing science, which was established as an academic discipline in Finland in 1979. It began as the final stage of the nursing education path, intended to educate nurse administrators, nurse educators and nurses for various positions of expertise opened up by the growth of the Finnish welfare state (Laiho, 2005, 2012). It was also created to enable nurses to acquire doctoral degrees and to gather the already emergent research in the field of nursing – as, for example, conducted in a research group established by the Finnish professional association of nurses – and move it to universities, where research could be performed alongside other disciplines. The article focuses on this newly academic discipline in the 1990s. The relevance of this historical timing is in that the empirical investigation from the time when the transformation thesis was formulated evidences that the transformation thesis cannot be taken for granted. The article instead suggests that the specificities of the different fields are to be taken into account when designing theories about the evolution of science.

If the transformation thesis is simply applied in this context, Mode 1 knowledge production would mean that knowledge would be produced within the confines of nursing science and for other nursing scholars in order to advance the discipline and accumulate knowledge. There would be limited interaction between nursing science and society. Nursing science would be autonomous, and it would control knowledge production in this area. The Mode 2 knowledge production model, by contrast, would mean that the problems of the nursing sphere would be solved in interaction with multiple actors, such as nurse educators, nurse administrators, nurse practitioners, students of nursing practice and advanced students alike, nursing services in both the public and private sectors, and various care industries, including both official and unofficial therapists that provide care. All these actors would take part in the production of knowledge. The arena of this academic endeavour would be broad and open, so that knowledge would be expected to be applicable in many quarters, not only inside the limited nursing science community, but the various actors involved in the nursing sphere could use it and benefit from it. Knowledge would be produced as an answer to a specific societal problem, and many different disciplines would take part in the process because many different perspectives would be needed to achieve a new solution to the problem. All of society would

benefit from knowledge production in the form of increased welfare and better health. The transformation from Mode 1 to Mode 2 would have happened at a rapid pace, since nursing science was only just established. The article brings new insights into how the knowledge production of nursing science is understood to interact with society and which mode of knowledge production, if it is one or the other, is understood to dominate in its domain.

Finnish nursing science in the mid-1990s

In this article, I focus on Finnish nursing science in the mid-1990s, when the central texts of the transformation thesis were also written. Teaching curricula of nursing science had then been implemented at seven universities (Kuopio, Helsinki, Turku, Tampere, Oulu, Jyväskylä and Åbo Academi University[1]). These curricula were based mostly in the departments of medicine, with the exception of Kuopio and Åbo Akademi, whose nursing science programmes were organised under the department of social sciences. Many professorships were established either immediately before or during the mid-1990s. By 2000, there were a total of 16 professorships (including associate and assistant ones), 12 other researcher positions and 16 lectureships in the country (Laiho, 2005). These curricula developed rapidly, as there were many students applying to the universities to obtain a degree in the new discipline.

Nursing research was, back then, still at an emergent stage for two main reasons: 1) the need to develop teaching and nursing curricula at universities became the first challenge to be achieved, because there was a flow of students to these programmes that delayed the development of research to some extent; and 2) the pioneers of nursing science themselves were not nursing science graduates but graduates of other disciplines, mostly education, which meant that the research programmes were starting from scratch (AF, 2003; Laiho, 2005; Laiho, 2012). This stage provides an excellent context for this article because it offers a possibility to observe whether the emergent discipline would develop into the kind of transdisciplinary actor suggested by the thesis, focused on problem-solving and major societal issues through an open collaboration between industry, university and government. Or whether it would be sheltered under the wing of university, where autonomous research could safely be conducted free from outside influences.

In the mid-1990s, Finnish nursing science had established its educational programmes, and it was time to discuss its relationship with society, together with such themes as the basis for and practices of its research, the boundaries and identity of the discipline, terminology, its relationship with other disciplines, and its theoretical and methodological basis (Tuomi, 1997; AF, 2003; Eriksson-Piela, 2003; Laiho, 2005; Laiho, 2012; Vuolanto, 2013; for other countries, see Findlow, 2012; Meerabeau, 2005; Spitzer and Perrenoud, 2006; McNamara and Fealy, 2010). In Finland, there were many lively debates at the time and, in these discussions, various actors presented multiple views of the relationship between nursing science and society (Vuolanto, 2013).

Previous research has discussed the vulnerable position of nursing in academia and its struggles to find its voice there (Meerabeau, 2005; McNamara and Fealy, 2010; Findlow, 2012). Nursing science was founded as part of the nursing profession's efforts to secure and enhance its status in the context of so-called 'academic drift' (e.g. Neave, 1979), which has resulted in identity work that balances between a broad variety of approaches, including professional, ethical, spiritual and biomedical angles (Paley, 2008: 181; Paley, 2011). In terms of the relationship of nursing science with society this means, as McNamara and Fealy (2014: 158) state, that nurse academics are "constantly challenged to secure their discipline's legitimacy in academic and clinical settings as well as the wider public sphere". These characteristics of nursing science provide a particularly interesting setting for understanding the relationship between science and society in the framework of the transformation thesis.

Research material and method

Research material

The present article analyses the relationship of nursing science with society as articulated by

Table 1. The forum, timespan analysed and number of analysed items in the research material.

Forum, timespan analysed and number of analysed articles or news items (in parentheses)	**Description of the forum**
Documents	
Archive of the department of nursing science at University of Tampere 1993-1997 (8)	Official documents related to doctoral thesis, master's thesis and meeting minutes of nursing science departmental committee
Newspapers	
National newspaper, 11.12.-19.12.1996 and archival search[1] (7)	*Helsingin Sanomat,* Finland's biggest newspaper covering national affairs
Regional newspapers, 16.3.-30.3.1996, 16.12.1996-15.2.1997 and archival search (6)	*Aamulehti* and *Hämeen Sanomat,* newspapers in the Tampere region within the sphere of influence of the University of Tampere
Professional and scientific journals	
Scientific journal of nursing science, 1994–1997 (14)	*Hoitotiede* [*Nursing Science*], the main peer-review journal of nursing science in Finland published by the Finnish Association of Nursing Research
Professional journal of medical doctors, 1996–1997 (1)	*Suomen lääkärilehti* [*Finnish Medical Journal*], the main professional journal for medical doctors in Finland published by the Finnish Medical Association
Professional journal of nurses, 1995–1997 (2)	*Sairaanhoitaja* [*Nurse*], the main journal for nurses in Finland published by the Finnish Nurses Association
Other journals and magazines	
Popular health magazine, 1996-1997 (2)	*Hyvä Terveys*, a popular health magazine for the general public
Journal of the scepticism movement, 1995–1997 (10)	*Skeptikko*, the journal of the Finnish Association of Sceptics
Student bulletin, 1996-1998 (1)	*Epione*, journal for students of nursing science at the University of Tampere
University student magazine, 1/1996-5/1997 (1)	*Aviisi*, a student magazine from the University of Tampere students' union
University bulletin, 20/96-2/97 (2)	*Yliopisto,* a bulletin of the University of Helsinki, the largest university in Finland
Newscast	
Evening news, 23.3.1996 (1)	*YLE News*, Finland's main television news broadcaster

Finnish actors in the mid-1990s. The main actors in forming understandings about the relationship between nursing science and society were nursing scholars, medical specialists, sociologists, philosophers, sceptics, nursing science students, nurse practitioners, therapists and patients. There were several fora in which the relationship between nursing science and society was discussed: scientific and professional journals, magazines, newspapers and media broadcasts (see Table 1). The research material was drawn from these fora because they represent most of the main publicly available and relevant fora for discussing the issue.

I attempted to collect research material from all possible discussion fora of nursing science in the mid-1990s. Also, I tried to include views from as many social worlds as possible. The discussion back then was very lively, and individuals from many different social worlds took part. Most of the discussion was in *Nursing Science*, the main peer-reviewed journal of nursing science in Finland

at the time. Since the launch of this journal in 1989, the majority of the articles in its discussion column were published in 1994-1997 (52%), and after 2002 this column was mostly silent. In 1994-1997, the discussion was particularly rich in nursing science and society issues. These issues were discussed also before and after this time, but only in scattered sets of individual remarks.

What is more, in 1996, due to the Finnish scepticism movement becoming cautious about two dissertations in nursing science at the University of Tampere, one of the main sites for nursing science in Finland, the discussion spread to fora where there would normally be no discussion about nursing science at all, such as *Skeptikko*, the journal of the Finnish Association of Sceptics, and *Hyvä terveys*, a popular health magazine. It was also incredible that, at that time, the topic of nursing science was raised in the nation's main television news and national and regional newspapers. All these fora resulted in a broad variety of individuals reacting to the issue of the relationship between nursing science and society and articulating their views in public. It may be that, later on, the discussion moved to different and less public fora that were less accessible. This makes the mid-1990s an especially fruitful timing for this study.

Analysis

The analysis concentrated on the views on the relationship between nursing science and society expressed in the material. In this analysis, I applied the social worlds framework (Clarke and Star, 2008) to focus on the different views that the various actors had on this relationship. I adopted the idea that the relationship between nursing science and society meant different things to different actors coming from different social worlds in the arena (Clarke and Star, 2008: 123) of nursing science in the mid-1990s and asked how the participants from different social worlds understood the relationship between nursing science and society (Star and Griesemer, 1989) and what their 'multiplicities of perspective' were on the relationship between nursing science and society (Clarke and Montini, 1993: 45). In particular, I focused on the "legitimation processes by which different social worlds establish and enforce the standards and boundaries" (Gerson, 1983: 358) of an issue. In addition, patients – as silent implicated actors (Clarke and Montini, 1993: 45) whose perceptions about the relationship between nursing science and society were not explicated by themselves but assumed and implicated by other actors – represented the world of the clients of healthcare providers.

Within this framework, I analyse the main actors and their perceptions about the relationship between nursing science and society in the arena of nursing science in the mid-1990s. This analysis is based on a close textual analysis (see e.g. Fahnestock, 2009; Segal, 2009; Ceccarelli, 2001) of the research material where the various actors articulate the relationship between nursing science and society in this arena. I read the perceptions as representing a certain social world, not as an individual's opinion on the issue. The main question is, how was the relationship between nursing science and society perceived by different actors in the mid-1990s? After presenting these perceptions, I discuss, in the light of Modes 1 and 2 of knowledge production and their critique, how these perceptions parallel with or differ from the central ideas of the universities' transformation thesis, which was formulated during the same period of time.

Limitations

I recognize that there are limitations in the research material. The analysis does not cover nursing science textbooks, which could have provided a different angle on the pioneer scholars' understanding of the field, as Tuomi (1997) argues, nor does it look at the editorials in the main Finnish journal of nursing science, a thus-far unexplored corpus. However, both of these sources may have overplayed the views of nursing scholars, as they were written by these scholars only, and it would have been against the idea of the present article, which is to find the variety of social worlds that could have something to say on the issue. Another limitation was that the views of the social world of the patients are only stated in the texts written by the other actors, not the patients themselves. To overcome this limitation would have required a questionnaire or an interview study, but both of these sources would have

provided a picture of patient views today, whereas the focus of the study is on the discussion occurring in the mid-1990s, when the transformation thesis was written.

A third limitation, also related to the time frame, is that the discussion of the relationship between nursing science and society today is not part of this study. My research is focused on the discussion of nursing science in one country within a limited time frame. I realise that there are many factors that influence nursing science and have to do with its relationship with society. The transformation thesis is only one lens that could be used to analyse this phenomenon. In my research, I have used boundary work (Vuolanto 2015) and interdisciplinarity (Vuolanto and Laiho 2017). As nursing science is definitely an understudied topic in science and technology studies, the present analysis and these other works aim at providing a starting point for further research on contemporary nursing science in society through science and technology studies. As a historical case study, this analysis may have relevance to contemporary nursing science, as it enables self-reflection on its historical understandings of the issue.

Multiple perceptions of the relationship between nursing science and society

Nursing scholars

There were many nursing scholars involved in the discussions about the relationship between nursing science and society in the mid-1990s. They were represented by two incumbent professors, a lecturer and an emerita professor from the University of Tampere, one incumbent professor from the University of Turku, and one incumbent professor from the University of Helsinki. They wrote about the issue in many fora: a scientific journal of nursing science (eight articles in a discussion column), newspapers (one article), a student bulletin (one article) and in the professional journal for nurses (two articles). Moreover, they gave interviews in the journal of the scepticism movement (one long interview with a nursing science professor at the University of Tampere), in the university bulletin of the University of Helsinki (two brief interviews with nursing scholars: one a professor at the University of Helsinki and the other a former lecturer at the University of Tampere) and in a regional newspaper (a former lecturer at the University of Tampere).

Nursing scholars' perceptions about the relationship between nursing science and society could be understood as divided into two sets. Most of the nursing scholars understood the relationship according to Mode 1, but there were also some whose perceptions were closer to Mode 2 knowledge production.

Especially in situations in which nursing scholars were forced to take a defensive position, their perceptions of the relationship tended to be closer to the mode 1 knowledge production. They provided answers at a time when actors from outside, especially from other disciplines and the scepticism movement, had been active in observing nursing science and had pointed out deviances from the generally acknowledged scientific principles. Reactions to these deviances were strong, e.g. a Humbug Award given to a former master's thesis in nursing science or a text grading nursing science poorly in the authoritative journal of Finnish medical specialists. Thus it was natural that nursing scholars, when given an opportunity such as in an interview of the incumbent nursing science professor in the journal of the scepticism movement, defended their position and their discipline. The defence was verbalised in that the nursing scholars presented nursing science as purely academic ivory tower research, which has a right to exist and administer its subject area without outside intervention. They also emphasised nursing science's societal impacts as great: it produced health and welfare to the society and helped to reform practical nursing, in which large amounts of public and private money were invested. This societal impact legitimated the position of nursing science as an autonomous area at the university. Thus, the nursing scholars emphasised nursing science's right to work in the first mode of knowledge production, as an independent discipline free from outside interests.

The nursing scholars in defense positions also referred to the direct link – and thus the fluent knowledge transfer – of nursing science to nursing practice as a strength. This direct link came partly from the requirement of university education that

all the students accepted to the university must first have obtained nurse qualifications at a lower-level institution. This is specific to the Finnish dual education model, in which nurses are educated at lower-level professional institutions and nursing scholars at universities (for nurse education in other countries, see Spitzer and Perrenoud, 2006). Entrance to universities is not possible without a professional degree. In any case, the dual education model has ensured that nursing science students are qualified and registered nurses who often have long-term practical experience as nurses. They have also research interests directly driven from the practical domain, and their research is expected to be immediately used in practical settings. However, in the mid-1990s, it became necessary for nursing scholars to distinguish academic nursing from the other actors in the field. An excerpt from the interview of a nursing science professor at the University of Tampere helps to understand these views:

> It is also a fact that – in general, in getting any message across – that good marketing ensures that the message gets across. Summer universities, centres for supplementary education and adult education centres in healthcare education institutions receive requests from the field at every turn, especially requests to organise education based on Parse's thinking. Thus this education is easy to sell. Hospitals and healthcare centres also organise placement training according to the philosophy in question to some extent. (Ollikainen, 1996: 13)

Here the professor lists other educational institutions which confuse the transfer process of nursing science with their commercial aims. This must be understood in the context of the Finnish higher education system, which is free of charge for the students and publically funded. The problem here is that these other actors in the nursing sphere do not see the value of academic research in the same way as nursing scholars at the university level generally did. The purpose of the text was to separate sincere academic research from commercial goals and to build up the authority of nursing science against various other education that was offered. To the other education institutions, it seemed that anything goes for knowledge in nursing and that their criteria for knowledge production were dubious. For example, the professor implies that these other actors could consider nurses' theses at lower-level institutions equal to academic doctoral theses or a doubtful theory brought from abroad parallel to university research. These kind of remarks generated an impression of nursing science as the strongest authority within the field of nursing when it comes to knowledge production and its transfer to other settings. Thus the knowledge transfer was seen as an important feature but to be strictly in the hands of nursing scholars. Thus other actors – as expected in Mode 2 knowledge production – were not allowed to dilute the knowledge transfer process of nursing science, and disciplinary context was seen as the controller of knowledge production in which the practices of scientific control by peer review were exercised.

It appears that the nursing scholars in defence positions did value other educational institutions than university, nurses and also the students as important transmitters of nursing science. However, these others appeared not to have similar abilities to evaluate research quality, so they should have only a limited access to the quality control process of nursing science. Thus it seems that the nursing scholars held with the traditional discipline-based peer review system, and they did not see that the new mode (in Mode 2) of wider quality criteria would have become or should become predominant in nursing science. Overall, the writings of the nursing scholars give an impression that nursing science, an emergent discipline at a time when the universities' transformation thesis was written, was in fact somehow affected by new modes of knowledge production such as were suggested by Mode 2 knowledge production. However, even at that time, nursing scholars emphasised the importance of the Mode 1 type of knowledge production and were rather suspicious of the collaborative production of knowledge with non-university actors. This means that they worked towards Mode 1 knowledge production rather than emphasising the new mode. Thus in the social world of nursing scholars, the dominance of Mode 2 knowledge production model would be undesirable and not a good direction for this young academic discipline to take.

There were also some former nursing scholars who did not hold formal positions in nursing science and were thus not in a position to defend the discipline as such. They appeared to have a different view of the relationship between nursing science and society: one that was closer to Mode 2 knowledge production. They were represented by a professor emerita and a former lecturer of nursing science at the University of Tampere. They presented perceptions that nursing science has many starting points and research approaches, often imported from outside the discipline in its rich interaction with society and societal actors such as patient groups and various healthcare occupational groups. In their view, these many actors were seen to participate in knowledge production in nursing. According to the former nursing scholars, nursing scholars were not the main actors in the knowledge production process, but multiple actors produce knowledge together. The former nursing scholars understood that the feelings and perceptions of patients were to be seen more important in this process than the advancement of science and scientific interests. Their perception was that knowledge (that was to be used towards the goal of the good of patients) was to be gathered from different disciplines in a transdisciplinary collaboration. Thus, nursing scholars were not a unified group: there were multiple understandings of the relationship between nursing science and society, even within the group.

Representatives of medicine

Four representatives from the field of medicine took part in the discussions about the relationship between nursing science and society. Two medical specialists wrote to the professional journal for medical doctors, the popular health magazine and the journal of the scepticism movement: one article for each of these fora. One of the medical specialists was active in the scepticism movement and could be said to have played a dual role, as both a representative of medicine and an activist. In addition, two professors of medicine were involved in the discussions as examiners of a doctoral thesis. The medical specialists and the professors had different views of the relationship between nursing science and society.

The first type of understanding of the relationship between nursing science and society by two medical specialists was similar to that of the nursing scholars in the defensive position. This view could be summarised as a belief that the relationship between nursing science and society should be similar to the relationship between medicine and society. Thus nursing science ought to be a strong and autonomous discipline which maintains control of its knowledge production. This type of understanding emphasised empirical and objective knowledge production and the 'gold standard' of randomised controlled trial and evidence-based medicine (Derkatch, 2008), giving the academic community a great deal of control and power over research topics, and nursing science should act similarly. This view stressed that the disciplinary and academic context is the most central one in the relationship between nursing science and society.

By contrast, the other understanding of the relationship between nursing science and society by the professors of medicine considered the academic community not necessarily less powerful, but a less central actor among the many heterogeneous actors that pose research objectives and control over the quality of knowledge. The case was that a PhD thesis on fasting was under examination at the nursing science department. The thesis clearly belonged to complementary and alternative medicine (CAM), which is not usually taken under scrutiny in Finnish official medicine. The first preliminary examiner, a nursing scholar, had issued a negative statement about the thesis and recommended that it not be accepted as a thesis for a doctoral degree in nursing science. In this situation, two professors of biomedicine, selected as preliminary examiners after the first had been unfavourable towards the thesis, strongly favoured acceptance of the thesis, and in the end, the thesis was indeed accepted as a thesis for the doctoral degree in nursing science. These professors were known as supporters of research on CAM or could be termed as 'CAM-friendly' (Derkatch, 2008) actors.

Apart from the issue of whether CAM was categorised as scientific or unscientific (compare with Gibbons et al., 1994: 3), the argumentation of these two professors reveals that their percep-

tion of the relationship between nursing science and society was closer to Mode 2 knowledge production than that of the first type of understanding of the relationship between nursing science and society. By that I mean that their positive attitude towards CAM also meant that these actors viewed the knowledge production in nursing science to be heterogeneous and the diversity of potential sites where research could take place was emphasised. The network of CAM-friendly actors became an important social and informal community in which knowledge could be formed. In this network, multiple methods and methodologies, multi- and trans-disciplinarity, new societal contexts for knowledge production – such as fasting courses – and multiple interests – for example, popularity among people – were emphasised rather than the scientific community as the primary actor in the knowledge production process. In other words, the CAM-friendly representatives of medicine viewed the relationship between nursing science and society much in the same way as explicated in Mode 2 knowledge production.

Sociologists and philosophers

Two philosophers (one professor and one researcher) and one sociologist (a researcher in a university sociology department) were involved in the discussions about the relationship between nursing science and society. Each of them wrote one article published in the Finnish journal of nursing science in the mid-1990s. Their articles analysed the discipline from a broad theoretical viewpoint drawn from the philosophy of science. In their articles, it appeared that they saw the commitment of nursing science to societal objectives the most central feature of the discipline. They understood the knowledge base of nursing science to emerge from many different disciplines and societal actors, also from the non-professionals and from traditional knowledge about care. They held that knowledge production in nursing science took place in a dialogue between researchers and research participants, e.g. patients. Their views on the relationship between science and society can be interpreted as leaning more towards Mode 2 knowledge production than Mode 1.

Sceptics

The sceptics are a community of scholars and laymen who feel it is their mission to defend science from unscientific knowledge (*Skepsis,* 2015). The sceptics were not necessarily themselves academics (e.g. the secretary of the movement) in a sense that they conducted research or held academic positions, but they could be amateur scientists who strongly felt it was their mission to guard science from unscientific attacks. The Finnish Association of Sceptics participated in the discussions about the relationship between nursing science and society as an association by giving their 1996 Humbug Award to a former master's thesis in nursing science. Additionally, the secretary of the movement wrote very actively about the issue in the Association's journal (three articles) and in the bulletin of the University of Helsinki (one article). She also interviewed the professor of nursing science at the University of Tampere after the Humbug Award was conferred.

The sceptics characterised nursing science as a young discipline and provided understandings of the relationship between nursing science and society from the point of view of a mature bearer – or 'watchdog' – of the scientific world view (Forstorp, 2005). They argued that the relationship between nursing science and society was vulnerable because the unscientific societal currents may make an incursion to the scientific arena through nursing science. They also formulated the view that, as a young discipline, nursing science was on a societal and cultural ground where there were a great many other actors as well, particularly marketers of non-science and CAM actors. As a consequence, the relationship between nursing science and society required a well balanced and well controlled transfer of knowledge to society according to the rules and norms of the purest of academic communities and adhering to the Mode 1 type of knowledge production, otherwise the rampant and unorganised movements would infringe on what was the property of pure academic science.

Thus, in the view of the sceptics, the relationship between nursing science and society should be closer to Mode 1 knowledge production than Mode 2. In their opinion, knowledge production was not to be interfered with by an unscientific

network of actors both inside and outside science and, as such, they acted as a quality control for Mode 1 knowledge production on behalf of science. They were willing to protect autonomous ivory tower research.

Nursing science students

Two nursing science students were involved in the discussions of the relationship between nursing science and society. One of them wrote a newspaper article in a regional newspaper, and another was interviewed by the University of Tampere student magazine. A specific feature of Finnish nursing science is that the students have to finish their professional qualifications for becoming a registered nurse and complete the practical training periods for their nursing degree (at separate educational institutions: the polytechnics) before entering university-level educational programmes. Thus nursing science students often have extensive experience from nursing practice, and they may have worked as nurses for several years before their university studies. It is also typical that they continue to work during their university studies. This situation means that their role is to act as intermediaries between science and nursing practice, to transfer knowledge from universities to the various settings of practical work.

In fact, these students are expected to transfer knowledge both ways: the nursing scholars need the connection to nursing practice and the students bring insights and reports about the current status of and reforms in nursing practice to the scholars while also taking theories and viewpoints from university research with them to the settings of practical knowledge. Students are expected to transfer knowledge in ways that help improve and reform practical work and help nursing scholars stay in touch with the actual work that their research aims to reform. For nursing scholars, this intermediate role means that nursing science at university level does not need to train nurses in practical issues, but instead to train them in the theoretical and methodological skills needed for research and to enhance their writing and reporting abilities for their work as administrators, teachers of polytechnics or researchers. Thus, the relationship between nursing science and society becomes relevant in the intermediary role of nursing science students: they are expected to be Mode 2 actors, mediating and conveying knowledge to the various settings of their heterogeneous work, research, and educational contexts related to nursing science.

However, in the discussions of mid-1990s, this intermediary role was severely disturbed. A group of nursing science students were enthusiastic about conveying knowledge of certain theoretical viewpoints and used certain books in their intermediary role. They felt happy about finally finding a theory that was fairly easy to mediate and were also sufficiently fluent in translating it to practitioners. Then, out of the blue, immediately after the Humbug award was given by the sceptics, this theory and the related books were banned by decision of the nursing science departmental committee. A theory that had been accepted and was easily transferable to practice was in one night turned into dubious knowledge not to be used at all. The students were dumbfounded: what were they to do with their own theoretical works using the banned theories and books? And, even more acutely, what would happen to their credibility as intermediaries if they had to tell the practitioners that this theoretical viewpoint, which they had the day before happily promoted, was no longer accepted and was now to be forgotten altogether?

From the point of view of the nursing science students, the banning of the books and theories meant a severe fracture in their role as intermediaries. The first mode of knowledge production applied by the nursing scholars in the decision to ban hampered the students' orientation towards the Mode 2 type of knowledge production, which was closer to the understanding of the free flow of knowledge between various organisational settings and different actors. The peer review type of quality control and shifting the power balance towards the autonomic power of academic and disciplinary actors in line with Mode 1 knowledge production was greatly different from the type of action that the transfer role had previously given to students. Now the transfer of knowledge was much more difficult due to the students being forced to always ask permission and the restriction in what knowledge could be transferred to practice; they had to be cautious about their actions and not transfer knowledge that was not

approved by the scholars. No wonder the students became uneasy: they had to change position in their social world and start acting differently.

Nurse practitioners

There was only one nurse practitioner involved in the discussions of the relationship between nursing science and society in the written material used for this study. This is surprising, because nurse practitioners are fundamental actors in nursing science: the discipline concerns their work, practices and anything that goes on in the world of practice, be it patient care in the settings spanning from acute care to elderly care and from psychiatry to operating rooms, administration and multiprofessional collaboration, or teaching the next generation of nurses. However, in this context, it must be borne in mind that all nursing scholars are themselves always and necessarily also nurse practitioners, as follows from the requirements for university admission in Finland, and, consequently, they have the education and formal qualifications to work in practice.

One explicit expression by a nurse was published in the form of a question in a Q&A column in the journal for professional nurses. This nurse was astonished at the decision to ban books and theories. In her question, she tried to pose 'understanding the patient' as a major societal problem that needed to be solved through research and developmental work in collaboration with various actors rather than by research ghettoed to the university alone, restricted by prohibitions, bans and the strict interests of academic professors. This nurse could not understand how curiosity and openness could be encouraged and the key question of the patient's welfare could be solved in this kind of restricted knowledge production model. So again, we see the juxtaposition between two modes of knowledge production, this time from the point of view of the practitioners' social world. The relationship between nursing science and society formulated from the nurse practitioners' social world would then be closer to Mode 2 knowledge production than that of Mode 1.

There is one additional dimension to this relationship between nursing science and society view if we consider the implications that nursing scholars give about the practitioners' view in their writings. The nursing scholars expressed the opinion that the practitioners – because they did not have the qualifications to do research work – did not have the same autonomous copyright on and ownership of knowledge on nursing as the nursing scholars did. The nursing scholars must be the guards who ensure that no unscientific knowledge enters the nursing sphere, and the nursing scholars had that ownership. This view restricted the world of nurse practitioners, as there was then no dialogue, and the reflexivity required by Mode 2 knowledge production did not appear possible. This indicated that, for example, cultural knowledge drawn from professional tradition ought to be forbidden and hidden, even though it, in the view of nurse practitioners, could belong to the Mode 2 type of knowledge production.

CAM therapists

Two CAM therapists participated in discussing the relationship between nursing science and society. One of them was a therapist who had held fasting courses for obese patients for weight reduction purposes. She wrote two statements related to her PhD thesis, one article for the professional journal of medical doctors, and an article for the popular health magazine. She was also interviewed in the evening news by Finland's main television newscaster. The other therapist actively promoted therapeutic touch to be used in patient care. She wrote for publication in the regional newspaper and the bulletin of the University of Helsinki: one article for each.

The CAM therapists' perceptions about the relationship between nursing science and society implied that nursing science could benefit from and utilise traditional Eastern medicine and indigenous peoples' traditions of care. Knowledge would be produced in a broad open model in line with Mode 2 knowledge production, with different actors participating in nursing science knowledge production, including open communication between nurses, medical doctors and therapists of various kinds. Knowledge production would broaden the social composition of the review system, namely therapists and patients would become important actors in the review of knowledge. The boundaries between

professionals, therapists and patients would be loosened, and open communication between them would be valued. An important notion in this connection was that patients were considered to know what was best for them and thus they were considered the best ones to determine the criteria for applicable and best knowledge. The principle "anything goes that works for the patient" would be applied rather than strictly scientific quality control and thus knowledge quality control would become based on patients' judgment and not just the judgment drawn from academic work.

Multidisciplinarity and transdisciplinarity were central in the therapists' view of the relationship between nursing science and society in solving the problems of patient care. Multidisciplinarity, for them, meant the inclusion of CAM actors and perspectives in research and developmental work for the good of patients. Thus, in the therapists' views of the relationship between nursing science and society, CAM was one perspective to be taken seriously among the other perspectives, nursing science and medicine. From the point of view of therapists, nursing scholars would be the ones to promote the openness of the scientific system, to be reflexive and include the voices of individuals and groups that have traditionally been seen to be outside the scientific system. The most important aspect to consider would be the good of the patients, and the implications of knowledge in this aspect would be highlighted rather than the academic aims of knowledge production, which emphasise the growth of knowledge in separate parts of the patient. The therapists also stressed that the Mode 1 type of knowledge production had led to the current system of dividing the patient into separate parts to be cured and forgetting the patient as a whole. Nursing science was seen as a potential counterforce to the 'sliced individual' model and specialised medicine, and thus it could reform this less patient-oriented view and replace it with the holistic patient care model.

In the discussions of mid-1990s, the therapies and viewpoints of the therapists were judged to be unscientific and outside the scientific domain. Thus, the academic knowledge production model was emphasised, the science-oriented worldview was raised above other worldviews, and scientific methodology was referred to as incomparable to the haphazard judgment of individual patients. This boundary work (Gieryn, 1983, 1995, 1999) to build up the credibility of nursing science and to separate the scientific from the unscientific appeared also to juxtapose the Mode 1 and Mode 2 types of knowledge production. Whereas the nursing scholars appealed to Mode 1 knowledge production, the therapists used Mode 2 knowledge production terminology as a rhetorical strategy in gaining acceptance of the standpoint of CAM and its culture in researching and developing healthcare.

Patients

The social world of patients cannot be understood from their own active contributions to the discussions of the mid-1990s. Thus, the patients could also be called silent implicated actors (Clarke and Montini, 1993: 45): their understandings of the relationship between nursing science and society were implicated by other actors. In a complete analysis of the different actors' views on the relationship between nursing science and society, it is important also to pause to think about how patients were understood to take part in the production of knowledge as this was indeed constructed by other actors. In general, Mode 1 knowledge production by the nursing scholars and sceptics perceived patients as the passive objects of scientific studies. They were not considered as participating in knowledge production at all, but were seen to form the object of academic work. Knowledge must be properly tested first and then academics would judge whether knowledge concerning the welfare of patients would be useful or not. Knowledge production was not necessarily understood to have a practical goal concerning an individual, but was an issue to be solved from the point of view of a group of patients, which made it knowledge that could be produced purely based on academic interests.

Therapists, in contrast, claimed that patients had an active role in nursing science knowledge production. Patients were the key knowledge producers of nursing science, and would provide new perspectives in their interaction with other actors, including nursing scholars, biomedicalists, and other knowledge producers such as therapists. The value of patients lay in judging

the relevance and usefulness of knowledge in the context where knowledge is directly applied to patient care, not necessarily before that was tested and scrutinised according to lines of research determined by academic actors only. Thus, depending on the actor, the relationship between nursing science and society of patients was flexibly either Mode 1 or Mode 2 knowledge production.

Discussion

Analysis shows that there were multiple perceptions of the relationship between nursing science and society in the mid-1990s. This could be analysed well by using Modes 1 and 2 to distinguish between the different dimensions of the relationship between nursing science and society and the many types of understandings about the relationship between nursing science and society. However, when it comes to actual change in nursing science, the mid-1990s provides evidence that the academic forces around the discipline at its early stages of development tended, rather, to emphasise the autonomy and power of the academic community in knowledge production. In addition, these actors under the purview of a university were more likely to stress the homogenous academic lines and principles whereby knowledge is produced within nursing science instead of a heterogeneous composition of actors taking part in knowledge production. It follows from these views that quality control of nursing science was understood to be traditional, based on academic peer review systems rather than the loosening of the quality control of academics and "loosing the control" of scholars to a broad range of other actors (Albert and McGuire, 2014: 41).

Yet simultaneously, there are voices that seem to live in the understanding that the Mode 2 type of knowledge production may be preferable in nursing science rather than that of Mode 1. They promote this view in their writing and thus attempt to make room for multiple actors who could take part in the quality control of nursing science. They act and speak for transdisciplinarity and heterogeneity of actors as beneficial to the knowledge production process. However, these actors come from outside the academic community and represent a different kind of world view from that of the academics and, as a consequence, their power to shift the knowledge production mode of nursing science towards Mode 2 is limited.

Hence, my analysis of the relationship between nursing science and society provides very little evidence that the knowledge production mode of nursing science would have been transformed or, at the early phase of the discipline, immediately changed from Mode 1 to Mode 2. It is also unlikely, based on the views of nursing scholars, that the Mode 2 would have become the dominant understanding of knowledge production at this early stage. Rather, the study draws a picture of knowledge production as a complex, discipline-specific and negotiated process in which many actors with divergent interests and power from different social worlds function simultaneously, as other studies have also shown (Ylijoki, 2003; Tuunainen, 2005c; Albert and McGuire, 2014). In the discussions of nursing science at the time when the transformation thesis was written, many understandings of Mode 1 and Mode 2 co-existed. Therefore, the case supports views that more sociological empirical research is needed to understand the power struggles over knowledge production between different social groups and to add more sociological approaches, for example, Bourdieu's concept 'field' in these studies to understand the divergent interests that social interaction contains (Albert et al., 2007; Albert and Kleinman, 2011).

The connection of knowledge production to the economic development of nation states is central to the transformation thesis, but based on my analysis of the relationship between nursing science and society, the consequences of nursing science knowledge production did not include straightforwardly economic issues, and rhetoric about nation states was non-existent. However, nursing scholars raise themes regarding commercialisation in their view of the relationship between nursing science and society. These themes were referred to in negative terms when the scholars were concerned about commercial activities in connection with nursing, e.g. in educational institutions that charge tuition. In the views of these scholars, commercial activities

were seen to threaten the purity of the academic endeavour, and commercial education ran counter to academic education and the aims of academic knowledge production. In their views of the relationship between nursing science and society, the nursing scholars cleansed nursing science of economic terms and saw commercial activities as being fully separate from academic ones, which speaks more of Mode 1 type of knowledge production than the possible coming of an entrepreneurial university model (Slaughter and Leslie, 1997).

The transformation thesis claim that knowledge is increasingly produced in transdisciplinary contexts receives no support in the early developmental stage of nursing science and the discussions of its relationship between nursing science and society. On the contrary, nursing scholars emphasise nursing science identity work (Henkel, 2000; Amsterdamska, 2005) particularly strongly, which tells us more about contributing to a single discipline than being open to trans-disciplinary collaboration. The lone actors that highlighted the importance of multidisciplinarity were weak actors, such as therapists: those who did not have positions in nursing science. The significance of the disciplinary identity work lies in the fact that it was necessary in the early stage of nursing science development to situate this young discipline on the disciplinary map and to develop its disciplinary culture and practices, even though it worked against transdisciplinarity. Thus, transdisciplinarity did not seem relevant or tempting from the point of view of the strongest actors, but rather a threat that the other actors would gain the power to define their discipline. In this sense, the transformation thesis fails to capture the early stage of development of nursing science.

The idea of the transformation thesis, that a change has taken place in the public awareness and expectations, should also be discussed here. As stated at the beginning of this article, nursing science grew out of a public interest in providing nurses and their professional associations with a discipline that would enable and encourage research in the field of nursing (Laiho, 2012). It becomes evident in the views of the relationship between nursing science and society that there is nothing new in this public call for accountability and reflexivity. On the contrary, nursing scholars considered it self-evident that there was a direct link from practice to research and vice versa. In addition, many parties – including therapists, nurse professionals, and CAM-friendly representatives of medicine – presented their views in the debate about the greater reflexivity and the demand that their own actions be justified by nursing science. The change toward which nursing scholars were trying to turn the discussion was that nursing science could be reflexive and consider these different interests only to a limited extent. Nursing scholars worked in a direction that would limit the other parties' demand for reflexive nursing science and multiple views, and they tried to protect the autonomy of the discipline. Thus, again, the ideas of the transformation thesis do not seem relevant in the context of a profession-oriented discipline strongly both backed up and demanded by professionals and other parties even as early as its inception.

Critics of the transformation thesis have argued that the thesis was written from the point of view of science and technology rather than the humanities and social sciences (Albert, 2003; Godin, 1998). The perceptions about the relationship between nursing science and society show how the newly academic discipline of nursing science, belonging to the applied social sciences, does not seem to fit into the transformation thesis in a straightforward manner. Modes 1 and 2 can be used as an analytical toolkit, but the thesis in general fails to capture the multiplicity of views concerning the relationship between nursing science and society. The multiplicity has here been categorised according to the main poles of knowledge production presented by the thesis, but it becomes evident that there were actors – particularly the incumbent nursing scholars, enforced by the sceptics and biomedicalists to some extent – that drove nursing science towards Mode 1 knowledge production at the early stage of this field's development. Also there were forces and actors – especially the former nursing scholars, practitioners, therapists, CAM-friendly representatives of medicine and students – who pulled it in the opposite direction, towards the Mode 2 type of knowledge production. As a result, there is no evidence of a shift from one mode

to the other. The criticism of the heterogeneity of academic disciplines and the science-society relationship is thus strengthened by an analysis of nursing science. In this sense, nursing science offers a further reason to criticise the transformation thesis for its generality and lack of empirical validity (Hessels and van Lente, 2008: 13-14).

In addition to the discussion about Modes 1 and 2, there are other possible ways of elaborating on the relationship between nursing science and society. One of them is provided by Albert et al. (2007; Albert and McGuire, 2014). They propose that instead of the two camps or Modes 1 and 2 of knowledge production, scholars balance between two poles, production for producers and production for users. In their view, researchers are involved in both production destined for producers and others destined for users. The case of Finnish nursing science supports this view: the scholars and other actors were balancing service and science much in the same way as Albert and McGuire (2014) found in the case of medical education.

Another way of elaborating the relationship between science and society is provided by Ylijoki et al. (2011), who are critical of the transformation thesis and focus on finding a way to analyse the university-society relationship that would be sensitive to disciplinary differences in knowledge production. They distinguish between the different markets of university research to illustrate the diversity of forms that the university-society relationship takes: academic market, corporate market, policy market, professional market and public market. The academic market means that the main reference group of knowledge production is the scientific community and the main aim is to contribute to this field in top-ranked publications. The corporate market's reference group are companies and the target of knowledge production is commercial benefits. Policy market means that public administration bodies are the reference group for knowledge production and policy relevance of knowledge is highlighted. Professional market aims at professional development for the reference group of the profession. Public market, in turn, aims at production of knowledge for the general public and to empower ordinary people. To conclude my discussion, I use these markets to open up the relationship between nursing science and society in a way different from that of the Mode 1 and 2 discussion.

My analysis of the relationship between nursing science and society indicates that all the different markets existed in the views of the different actors in the mid-1990s. The academic market was quite strong, and nursing scholars particularly emphasised the ethos of this market when they put forward their understanding of the relationship between nursing science and society. The academic market was also debated and challenged as the only way of understanding the relationship between nursing science and society, as there were views that the corporate market had entered academia in the diverse forms of commercial education that took place in non-academic educational institutions. The policy market was especially activated by representatives of medicine, who reminded everyone of the policy guidelines – especially in the form of evidence-based practice – that were to be followed, also in nursing science. The professional market was part of this profession-oriented field, which was apparent from the needs and hopes of the nurse practitioners' views of the relationship between nursing science and society. The public market was revealed especially in the views of the therapists and nursing science students, who promoted the involvement of the general public, especially patients, in nursing science knowledge production processes. As my research was focused on the mid-1990s, studying these different markets in depth would be an important step further towards understanding the interaction of contemporary nursing science with society.

Conclusion

The analysis in this article concentrated on how the different actors from different social worlds understood the relationship between nursing science and society during the mid-1990s and how these different understandings fit with the central ideas of the universities' transformation thesis, which were brought forward at the same time. This analysis provided an opportunity in a new setting to add to the criticism of the transformation thesis failing to capture the whole of the

university and its various settings (Albert, 2003; Godin, 1998; Hessels and van Lente, 2008; Tuunainen 2005a, 2005b). Thus far, there has been a lack of knowledge about the ways the profession-oriented disciplines – among them nursing science, social work and education – reflected upon the concurrently claimed market-oriented trends and values. My analysis confirmed that there are different currents in knowledge production that belong to the central ideas of the transformation thesis, but that the claimed transformation is "no straightforward or unidimensional phenomenon but takes a variety of forms in different disciplines and organisational settings" (Ylijoki, 2003: 327).

To understand the relationship between universities and society even more fully, there is further work to be done. This article has shown the limits of the transformation thesis in capturing a profession-oriented discipline in the mid-1990s, but although it has been successful in this task, there is no doubt that it only provides a partial view of contemporary nursing science. Further studies are urgently needed to analyse the relationship between nursing science and society in the present day in order to understand nursing science in its more mature developmental stage and to find possible changes in the perceptions of different actors. In this task, it would be important to use a broader range of literature than the works arguing about universities' transformation in the mid-1990s. This article points to at least two sets of literature: the critique of the transformation thesis and the social worlds framework, both of which are shown to be beneficial in understanding the dynamics of science-society interaction. Also, this article has shown the limits of the transformation thesis in one profession-oriented discipline. There is a need to empirically investigate a broader set of disciplines of the same kind, and in this way include them as relevant entities in the current university world.

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Notes

1 More discussion on multi-, inter-, and trans-disciplinarity in Bruun et al., 2005 and Frodeman, 2010.
2 Since then, the curricula in Jyväskylä and Helsinki have been ended and at present five universities continue with nursing science programs (AF, 2003).
3 The archive search term was 'nursing science' in the electronic newspaper archives of the newspapers.

To Know or Better Not to:
Agnotology and the Social Construction of Ignorance in Commercially Driven Research

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Abstract

With an innovative perspective on the social character of ignorance production, *agnotology* has been a fruitful approach for understanding the social and epistemological consequences of the interaction between industry and scientific research. In this paper, I argue that agnotology, or the study of ignorance, contributes to a better understanding of commercially driven research and its societal impact, showing the ways in which industrial interests have reshaped the epistemic aims of traditional scientific practices, turning them into mechanisms of ignorance production. To do so, I examine some of the main contributions to agnotology and provide a taxonomy of practices of ignorance construction common in commercially driven research today. In particular, I present the tobacco industry's campaign against the health hazards of smoking as a paradigmatic case of ignorance production, identifying five central strategies. I then argue that the same strategies have been used in three other cases — global warming, pharmaceuticals, and the 2008 financial crisis.

Keywords: agnotology, commercially driven science, social construction of ignorance.

Introduction

Through the 20th century, the social organization of scientific research had radical transformations, from big in-house corporate labs fueled by major U.S. corporations, such as DuPont and General Electrics, to military funded projects for national defense during the Cold War, to new forms of private research in the global market, where outsourcing and off-shoring practices have prevailed (Mirowski and Sent, 2008). In particular, a general concern with a loss of U.S. competitiveness in the global market during the late 1970s and early 1980s, led to major changes in the organization of research and development (R&D) in the U.S. (Dertouzos, 1989: 306; Tyson, 1992: 291; Hunt, 1999: 19; Hart, 2001: 930; Coriat and Orsi, 2002: 1493; Mirowski, 2011: 115). Accordingly, with the end of the Cold War came the transition towards a new regime of science organization that has reshaped the role of science in liberal democracy (Slaughter and Rhoades, 2004; Davies et al., 2006; Fischer, 2009; Lave et al., 2010).

The weakened intellectual property legislation proper of the Cold War was strengthened to accommodate the new R&D commercial framework. A series of legislative Acts, transformed the connection between public and private research, allowing commercial profiting of publicly funded research (Barben, 2007: 62). The Bayh-Dole Act, for instance, famously granted property rights to universities over federally funded research, allowing them to profit from commercially driven research: "The act enabled universities to enter the marketplace and to profit directly when universities held equity positions in companies built around the intellectual property of their faculty as well as to profit indirectly when universities licensed intellectual property to private sector firms" (Slaughter and Rhoades, 1996: 318).

Changes in the corporate and university models meant changes in the organization of scientific research as well — most significantly, the in-house research lab was replaced through outsourcing R&D — leading to the consolidation of a new regime of science organization with a new liaison between the industrial and the academic sectors (Mirowski, 2011: 94). Hans Radder (2010: 4) characterizes this ongoing *commercialization* of academic research as "the pursuit of profit by academic institutions through selling the expertise of their researchers and the results of their inquiries." As one would expect, the industrial influence in academic research has become a growing concern among science scholars, who have warned us against some of the possible consequences of commercially driven scientific inquiry (Greenberg, 2001, 2007; Bok, 2003; Slaughter and Rhoades, 2004; Wise, 2006; Resnik, 2007; Radder, 2010).

With an innovative perspective on the social character of ignorance production, *agnotology*, or the study of ignorance, has been a fruitful approach for understanding the social and epistemological consequences of the interaction between industry and academic research.[1] Agnotology introduces a new perspective to the studies of science, one in which the social construction of ignorance becomes relevant for understanding scientific practice today. In particular, agnotological studies have uncovered the ways in which different mechanisms and practices, traditionally tied to knowledge production, have been reshaped and rechanneled to favor industry friendly outcomes, leading in many occasions to increasing ignorance among policy makers and the public at large. In this paper, I aim to show that the agnotological perspective contributes to a better understanding of commercially driven scientific research and its societal impact, showing the ways in which industrial interests have reshaped the epistemic aims of traditional scientific practices, turning them into mechanisms of ignorance production. To do so, I examine some of the main contributions to agnotology and provide a taxonomy of practices of ignorance construction common in commercially driven research today. In particular, I present the tobacco industry's campaign against the health hazards of smoking as a paradigmatic case of *agnogenesis*, i.e., of ignorance production, identifying five central strategies. I then argue that the same strategies have been used in three other cases — global warming, pharmaceuticals, and the 2008 financial crisis. To conclude, I summarize some of the advantages of using the agnotological perspective to understand commercially driven science as well as possible limitations of the approach.

The social construction of ignorance

Agnotology's main contribution to the social studies of science is its understanding of ignorance as a social construction. This differs from the traditional conception of ignorance as a natural vacuum:

> We need to think about the conscious, unconscious, and structural productions of ignorance, its diverse causes and conformations, whether brought about by neglect, forgetfulness, myopia, extinction, secrecy, or suppression. The point is to question the *naturalness* of ignorance, its causes and its distribution. (Proctor, 2008: 3)

Accordingly, Proctor (2008: 3) distinguishes three kinds of ignorance. In the traditional sense, ignorance is understood as *native state*: A vacuum or void that needs to be replaced with knowledge. In a second sense, ignorance can be a passive construct or *lost realm*. This is the type of ignorance

that grows from the social conditions in which science is made, including science's political geography (who is a scientist and why, and why science is done in some places rather than others) and the selection of certain lines of research over others. In this sense, ignorance emerges from the combination of decisions, circumstances and accidents that surround practices of knowledge production. Finally, ignorance can also be an active construct or *strategic ploy*: It can be deliberately made and maintained to fulfill the interests of certain people. In this sense, ignorance becomes a manufactured product, instead of being a natural or accidental result (Proctor, 2008: 6).

The study of the social construction of ignorance has two central characteristics (Fernández Pinto, 2015: 295). First, it is *constructivist* — it focuses on the manufactured character of ignorance as a phenomenon that needs to be studied, explained, and dealt with, especially, but not particularly, in scientific practice today. Second, agnotology in this sense is *social* — it is not concerned with the individual knower, but with the social circumstances that encourage the production of ignorance. The agnotological perspective emphasizes that a better understanding of the mechanisms through which ignorance is socially created and maintained would lead us to a more accurate understanding of the mechanisms of knowledge production.

Although the project of agnotology is not restricted to studies of commercially driven science — see, for example, the growing research on government secrecy (Galison, 2008; Balmer, 2012; Rappert, 2012; Kuchinskaya, 2014) — this has certainly been an important area for agnotology. The work of historian of science Robert Proctor (1988, 1995, 1999, 2012) on the tobacco industry's support of cancer research has been central to documenting the mechanisms of ignorance production in industry-funded science, as has been Oreskes and Conway's (2010) work on climate change and Michaels's (2008) account of the chemical industry. In addition, agnotological studies in the history of Big Pharma (Nik-Khah, 2014) and the history of the 2008 economic crisis (Mirowski, 2013; Mirowski and Nik-Khah, 2013) show further contributions in this respect. By synthesizing the main practices of ignorance construction and showing how their traditional epistemic purposes have been reshaped, this paper contributes to a better understanding of the import of commercial interests on scientific research today.

The tobacco strategy

The paradigmatic case of agnogenesis in the 20th century is the U.S. tobacco industry's denial of the health hazards of smoking.[2] The campaign began in 1953, when Ernest L. Wynder and his colleagues at the Sloan-Kettering Institute demonstrated that tobacco tars on the skin of mice caused fatal cancer (Wynder et al., 1953) and their findings were published in major journals and magazines in the U.S. On December 15th, the presidents of four major tobacco companies (American Tobacco, Benson and Hedges, Philip Morris and U.S. Tobacco) met with John Hill, founder and CEO of the famous public relations firm Hill & Knowlton (H&K). This was the launching point of the tobacco industry's denial campaign, in which the U.S. tobacco industry together with H&K would design and execute a strategy to counter scientific findings against tobacco smoking. Oreskes and Conway (2010: 6) call it the *tobacco strategy*: "Its target was science, and so it relied heavily on scientists — with guidance from industry lawyers and public relations experts — willing to hold the rifle and pull the trigger"; a strategy that has been widely acknowledged by the main scholars working on agnotology (Brandt, 2012; McGarity and Wagner, 2008; Michaels, 2008: 3-11; Mirowski and Nik-Khah, 2013: 282; Oreskes and Conway, 2010: 14-24; Proctor, 1995: 125-30; 2012: 22, 290-92).[3]

The mechanisms implemented by the tobacco industry to deceive the North American public and to perpetuate doubt about the health hazards of tobacco show that agnogenesis is a social and institutional phenomenon that has required the restructuring of many industry and academic settings. In particular, the tobacco strategy entailed the reshaping and rechanneling of different mechanisms and practices traditionally tied to knowledge production, with the aim of achieving new industrial goals.

I focus here on five core strategies or mechanisms fundamental to the tobacco strategy: (1)

The emphasis on scientific uncertainty, (2) the support of friendly research, (3) the recruitment of distinguished scientists, (4) the creation of an echo chamber effect, and (5) the attack to unfavorable scientific research. Notice that these are all practices traditionally tied to the process of knowledge production — scientists know that their results are uncertain, research centers support research that contributes to their goals, research teams aim at recruiting distinguished scientists, scientists want to disseminate their research results widely, and scientific research is hold to high standards of criticism — but in this case they have been reshaped or rechanneled to fulfill the industry's purposes. In what follows, I examine each of these mechanism and highlight how the tobacco industry transformed its epistemic purposes to achieve its commercial aims, turning these mechanisms into practices of ignorance production.

Emphasize the uncertainty

Every scientist is familiar with the uncertain character of scientific knowledge. As David Michaels (2008: 165) claims: "Absolute certainty in science is rarely an option; uncertainty is the norm, not the exception; and scientists base their judgments on the weight of the evidence because in many instances they have no other choice. Uncertainty does not mean the science is flawed". And while uncertainty does not mean that the science is flawed, it does not play well in the policy process, where the more conclusive a study or an expert opinion is, the more useful it is for regulatory advice. Similarly, uncertainty also contravenes the public understanding of science, according to which research provides conclusive results.

For that reason, Hill's first strategic move was not a direct attack against the scientific findings that were threatening the tobacco industry, but to exploit the inherent skepticism proper of scientific research (Brandt, 2012: 64): The tobacco industry would endorse the scientific ethos by claiming that more and better research into the causes of cancer was needed. Fostering the concept of scientific uncertainty and creating doubt about the reliability and accuracy of unfriendly scientific results became the industry's leading tactic to oppose the science connecting smoking to lung cancer (McGarity and Wagner, 2008; McGoey, 2009; Michaels, 2008; Oreskes and Conway, 2010; Proctor, 1995, 2012; Smithson, 1989). As the famous 1969 Brown & Williamson memo stated, "Doubt is our product, since it is the best means of competing with the 'body of fact' that exists in the mind of the general public" (quoted in Oreskes and Conway, 2010; Mirowski, 2012; Proctor, 2012).

As Oreskes and Conway (2010) explain, thanks to the popular idea that legitimate scientific claims are certain, uncertainty can be easily manipulated to create and sustain public policy debates. But, of course, scientists and science scholars know that certainty is an untenable ideal, and that science seeks high probabilities or best available knowledge: "History shows us clearly that science does not provide certainty. It does not provide proof. It only provides the consensus of experts, based on the organized accumulation and scrutiny of evidence" (Oreskes and Conway, 2010: 267-268). Taking advantage of this gap between the popular understanding of science and the actual status of scientific knowledge, the tobacco industry rechanneled the traditional notion of scientific uncertainty to foster ignorance instead of knowledge.

Support friendly research

In order to increase control on scientific research connected to tobacco smoking, U.S. tobacco companies united their efforts in the creation of a research council, initially named the Tobacco Industry Research Committee (TIRC) — later the Council for Tobacco Research (CTR). H&K made a big splash about the creation of the TIRC with the publication of an advertisement, later known as the "frank statement," in more than 400 newspapers across the country (Brandt, 2012: 66). In the advertisement the industry acknowledged the importance of the health hazard claims made against tobacco smoking and made a public promise to support further research on the issue, although emphasizing that cigarettes had not been proved to cause health problems.

The TIRC/CTR was funded by the tobacco companies to control their public image, through a close relation with H&K. In fact, more than half of the Council's first year budget went to H&K (Proctor, 2012: 267). The TIRC/CTR funded a

massive amount of scientific research through research grants, which were given to scientific experts in major universities and research institutions. In this sense, the TIRC/CTR was not conceived to support fraudulent research, but to use "good" research as a distraction. The agnotological move was a selection bias: "The bias stems from the fact that the CTR really wasn't designed to explore whether, how, or to what extent smoking causes illness… Grants were rarely given to anyone who knew much about tobacco and health" (Proctor, 2012: 269). When the first TIRC grants were announced, Alton Ochsner, a well-known thoracic surgeon, immediately noticed the problem:

> Of course, the critical areas of investigation, as every research scientist knows, have to do with the problem of how to make smoking a less lethal agent in lung cancer incidence and a less deadly killer in heart disease. Yet it is precisely these areas that apparently have been declared out of bounds for the industry's research committee. (Ochsner, 1954: 72)

TIRC grants funded research on heredity, infection, nutrition, hormones, nervous tension, and environmental factors, emphasizing that *all* of these fields of research were important for understanding the causal mechanisms of cancer and heart disease (Little, 1959: 2). Tobacco smoking was considered only as one of many environmental factors that could contribute to such health problems, downplaying its importance for future research. Even though the TIRC/CTR had a science advisory board, all grant applications were first filtered by CTR lawyers for litigation purposes (Barnes et al., 1995: 250). It was this bias at the macro-level that the TIRC/CTR together with H&K put in place to manipulate science and create ignorance.

The strategy was repeated by the tobacco industry later on during the controversy regarding environmental tobacco smoke (ETS), or second-hand smoke, and its connection to lung cancer. In 1987, the industry founded the Center for Indoor Air Research (CIAR) with the aim of producing friendly research (Drope and Chapman, 2001: 590). Like the CTR, the CIAR identified projects that seemed promising to the tobacco industry's interests and funded them through different grants. Research organization such as the CTR and the CIAR worked as intermediaries between the tobacco companies and "independent" scientists. In addition, the tobacco industry also provided funding for medical research projects in universities, created scholarships to finance medical students, and established relationships with members of some of the most important associations on health issues, such as the National Cancer Institute and the American Health Association (Parascandola, 2005; Oreskes and Conway, 2010; Proctor, 2012).

While the support of friendly research through financial and institutional arrangements is a common strategy to encourage scientific research in certain target areas — think for example about the directed efforts to find a cure for HIV, breast cancer, Alzheimer's disease, etc. — the tobacco industry reshaped this common strategy of knowledge production to appear interested in scientific progress, while obstructing the actual achievement of scientific knowledge, transforming in this way the support of friendly research into a mechanism of ignorance production.

Find scientists to support your cause

Along with the creation of research organizations came the recruitment of distinguished scientists, whose authority and academic connections would help the industry's goal of fostering uncertainty about scientific claims. Accordingly, the tobacco industry hired Dr. Clarence Cook Little as first scientific director of the TIRC. Little, a renowned biologist, geneticist, and eugenicist, was carefully chosen for his strong skepticism towards the epidemiological work connecting tobacco smoking with lung cancer. As a geneticist, he favored the idea that cancer had genetic origins and thus conducted and supported basic research regarding the mechanisms of cancer with animal testing, without ever making the connection to humans (Brandt, 2012: 66). Little became a key spokesperson for the tobacco industry's campaign against the health hazards of smoking, focusing on misleading lines of research, and increasing the confusion and misinformation by introducing scientific "noise" into the public discourse.

Another key example of the industry's recruitment of scientists as allies was the employment of Frederick Seitz as director of R. J. Reynolds' research program. Seitz was one of the most distinguished scientists in North America. A former student of Eugene Wigner, he became science advisor to NATO in 1959, then president of the National Academy of Scientists (1962 -1969), and in 1968 president of the Rockefeller University, one of the leading institutions in biomedical science in the country (Oreskes and Conway, 2010: 25-26). After retiring in 1979, he went to work for R. J. Reynolds, which gained the credentials of one of the most respected scientists in the country. Just as Little, Seitz favored the idea that cancer had genetic causes, and explicitly rejected the idea that emphysema could be caused by environmental factors such as smoking. His scientific credentials made Seitz a key figure for the tobacco strategy. He would publicly oppose scientific research on the environmental causes of cancer and directly attack scientists conducting this type of research.

The tobacco industry also looked for allies to counter the scientific findings that supported the hazards of ETS with the creation of an international program of scientific consultants. The ETS Consultants Program, as it was called, sought scientific experts worldwide to keep the "controversy" regarding ETS alive, and delay regulatory measures. In order to avoid the direct connection between the industry and the scientists, the industry hired a law firm, Covington & Burling, to conduct the screening, recruitment, and training of experts (Muggli et al., 2003: 306). Scientific consultants were asked to testify before Congress, to publish articles in scientific journals, to attend ETS conferences, and to submit letters to editors in response to adverse articles (Drope and Chapman, 2001: 590). Given the fact that the ETS Consultants Program was international, the program rendered "foreign" experts for the tobacco industry, as well as a web of regional contacts to campaign against regulatory efforts worldwide.

The tobacco industry took advantage of a strategy normally used to encourage the production of scientific knowledge, i.e., to recruit well-known experts on the field. The strategy, of course, is built on the presupposition that renowned scientists will contribute their particular expertise and experience in the field to buttress the process of knowledge acquisition. In this case, however, the tobacco industry recruited the scientists as a PR strategy to capitalize on their scientific credentials. That is, to support their own industrial interests with the credibility and recognition of these experts. In fact, the most renowned experts, such as Little or Seitz, occupied administrative positions and not research positions. Thus, the recruitment of renowned scientists was no longer guided by the traditional epistemic aims, but instead followed other commercial aims.

Spread the word (or the creation of an "echo chamber effect")

Proctor (1995) has emphasized the central role that PR firms play in the development of practices of ignorance construction. In particular, he calls attention to the fact that commercially driven science has become an instrument of PR, the tobacco industry's close relation with H&K being a clear example of this. The creation of research organizations and the support of scientific experts were crucial to Hill's strategy precisely because they sustained a full-blown advertisement campaign in the media to maintain cigarette consumption.

As one would expect, manipulating media coverage also became an important tool for the tobacco industry in the fight against regulation. Appealing to journalistic balance under the "fairness doctrine," which required broadcasters to include opposing views when treating controversial issues of public importance, industry lawyers demanded equal time for both sides of the debate. Every time scientific research was invoked to claim that smoking caused lung cancer, industry lawyers contacted the relevant venue and provided a list of "independent" scientific experts that would challenge the research. Such an appeal for "balance" and "objectivity" led the general public to believe that experts had not arrived yet at a scientific consensus regarding the link between smoking (and later second hand smoke) and lung cancer. In Oreskes and Conway's (2010: 19) terms: "Balance was interpreted, it seems, as giving equal weight to both sides, rather than giving *accurate* weight to both sides".

Much of the success that the tobacco industry had buying time and space in mass media was due to the journalistic commitment to telling both sides of the story. The TIRC/CTR exploited precisely this feature of the journalistic ethos to keep the public debate alive. Given that the strategy of appealing to balance was used, and continues to be used, in several other scientific controversies (see McGarity and Wagner, 2008: 224-26), it is now obvious that a fundamental conflict between the scientific and the journalistic ethos has been exploited as a strategy of ignorance construction (Antilla, 2005; Boykoff and Boykoff, 2004, 2007; Freudenburg and Musselli, 2010).

In addition to public media, the tobacco industry also made important efforts to support the dissemination of industry-friendly research within the scientific community. Part of this strategy was the creation and distribution of non-peer-reviewed journals and pamphlets, such as the *Tobacco and Health Report*, a monthly newsletter published by the TIRC (Proctor, 2008: 14-15). Another example was the funding and organization of symposiums, which allowed the industry not only to control the amount of industry friendly research presented, but also to increase industry-friendly publications through symposia proceedings. In this way, the tobacco industry circumvented peer review standards in publication, without compromising its façade of research supporter. Symposia proceedings are after all part of the scientific process through which a researcher communicates her work to her peers, even though the research is not really qualified as finished until it gets published in a peer-reviewed journal. The tobacco industry took advantage of this fact, and frequently used symposia proceedings as expert references in the "controversy" regarding ETS. Symposium articles, however, were more likely to agree with the tobacco industry's views on ETS (46% vs. 20%), less likely to endorse the risks of ETS (22% vs. 49%), and more likely to be written by scientists affiliated with the industry (35% vs. 6%), than peer-reviewed articles (Bero et al., 1994: 612).

Dissemination of results within the scientific community is a priority. In fact, scientists compete to publish results first, thus speeding up the process of knowledge production. The tobacco industry transformed this common scientific practice as well as its epistemic aims. Instead of emphasizing the prompt publication of reliable research results, the quality of which is assured by the peer review process, the industry reshaped publication mechanisms, emphasizing the breadth of the dissemination and undermining its epistemic quality.

Attack unfriendly research and researchers

Finally, the tobacco strategy also included the attack to scientific research and researchers, who were presenting unfriendly results for the industry. In some cases, the industry triggered personal attacks on scientists with good scientific credentials, accusing them of not following scientific standards or claiming that they were politicizing research. Salient here is the attack against Takeshi Hirayama and his research on the health hazards of second hand smoke (Hirayama, 1981). The industry launched a campaign to undermine Hiramaya's reputation, accusing him of committing crucial statistical errors (Oreskes and Conway, 2010: 137-143; also mentioned in Michaels, 2008: 86-7 and Proctor, 2012: 190), despite the fact that tobacco industry's researchers had found Hirayama's results to be correct (Ong and Glantz, 2000).

Part of the strategy was also to oppose government funded research. For instance, when the EPA released the report *Respiratory Health Effects of Passive Smoking* (1992), in which chronic disease and death was attributed to secondhand smoke, the industry (through Fred Singer) accused the EPA of doing "junk science," and holding positions that were not supported by scientific findings (Oreskes and Conway, 2010: 140-42). The "junk science" movement allowed the tobacco industry to accuse perfectly legitimate science of being "junk," while promoting the idea that their own research was "sound science," uncorrupted by particular political interests.

The main aim of the "junk science" rhetoric was to weaken academics and their research in the mass media by "promoting the idea that there is a surfeit of dodgy academic science corrupting the journals and airwaves, fostering the impression that every concerned citizen must gird themselves to be wary of corruption in the vast archive of scientific thought" (Mirowski, 2011: 297). Oreskes

and Conway (2010: 232) as well as Michaels (2008: xi) trace the "junk science" movement back to the tobacco industry's attempt to discredit scientific research that they didn't like. They portray it as an Orwellian crusade to undermine real science as "junk", and replace it with "sound" industry friendly research.

The attack to unfriendly research and researchers is probably the clearest case of the industry driven transformation of scientific research practices, undermining the production of knowledge. Of course scientific research results undergo a critical process in which other scientists are encouraged to object to the results, to find methodological flaws, to question the reliability of the data, etc. Within the scientific community it is also legitimate to attack other scientists for doing "bad" science, i.e., committing scientific fraud, not following the appropriate standards, being biased, etc. Both of these are mechanisms for keeping in check the epistemic quality of the research. The tobacco industry rechanneled them, however, to fulfill a very different aim, i.e., to undermine scientific research and researchers that were problematic for the industry's commercial interests, thus obstructing knowledge and creating ignorance.

The tobacco strategy was initially designed and developed by H&K as a PR campaign for U.S. tobacco companies, implementing at least five core practices: (1) The emphasis on scientific uncertainty, (2) the support of friendly research, (3) the recruitment of distinguished scientists, (4) the creation of an echo chamber effect, and (5) the attack to unfavorable scientific research. In each case, the tobacco industry transformed these traditional practices of knowledge production to fulfill its commercial interests. But in doing so, the industry also compromised the epistemic goals of these mechanisms, turning them into ignorance productive strategies.

The tobacco strategy meets climate science, pharmaceuticals, and economics

The tobacco strategy proved to be highly successful. The industry did not lose any court cases till the 1990s and the FDA was not able to regulate tobacco as an addictive drug until 2009 (Oreskes and Conway, 2010: 33). Moreover, the tobacco strategy has become a model of agnogenesis for countering scientific knowledge in other fields. In this section, I show that the same mechanisms have been similarly repurposed in three other cases: global warming, pharmaceuticals, and the 2008 financial crisis.

Global Warming

Global warming is one of the central case studies in Oreskes and Conway's *Merchants of Doubt* (2010). Despite the Intergovernmental Panel on Climate Change's (IPCC) endorsement of the anthropogenic climate change hypothesis by 1995 and its ratification by the scientific community, the American public remained skeptical about it (2010: 169). Noticing that this was at the peak of North American public awareness of climate change — the short period between 2006 and 2007 in which the Republican anti-environmental voting decreased, the Democrats were actively engaged in the fight against climate change, and Al Gore's documentary *An Inconvenient Truth* (2006) received two Oscars (Brulle et al., 2012) — Oreskes and Conway ask why the public did not endorse the scientific consensus on anthropogenic climate change.

To a great extent, the gap between the scientific community's and the public's stances regarding climate change responds to a denial campaign following the tobacco strategy. McCright and Dunlap (2000, 2003, 2010, and 2011) trace this campaign to a conservative movement led by major think tanks in the U.S., such as the American Enterprise Institute, the Cato Institute, the Heritage Foundation, and the Marshall Institute (2003: 355). Oreskes and Conway (2010), on the other hand, are more concerned with the scientists who put their scientific expertise and credentials to attack the scientific community and support private interests. But there is no doubt that the major financial support for this campaign has come from the U.S. fossil fuel industry, especially ExxonMobil (Gelbspan, 1997: 46; Mooney, 2005; Michaels, 2008: 56; Dunlap and McCright, 2010: 245; Oreskes and Conway, 2010: 246; Perrow, 2010: 62; Weber and Stern, 2011: 321). The Union of Concerned Scientists (2007) has documented the funding of several think tanks by ExxonMobil, and Greenpeace (2010) has done the same for the

case of Koch Industries. Moreover, Greenpeace has also developed an online tool to track Exxon-Mobil's funding to specific think tanks and then to specific scientists.[4]

The fossil fuel industry has followed the path of the tobacco industry in designing and developing a PR campaign to counter scientific consensus on global warming. Let me mention here some of the most salient similarities. First, they are both denial campaigns that foster scientific uncertainty and exploit scientific skepticism, undermining the epistemic value of scientific uncertainty. Just as the tobacco industry decided to attack science with more science, so have climate change skeptics. Their main strategy has been to create doubt and uncertainty regarding the science behind climate change, and they have succeeded by implementing some of the same strategies to confuse the North American public (Dunlap and McCright, 2010). For instance, take Frank Luntz's memo to the Republican Party: "Should the public come to believe that the scientific issues are settled, their views about global warming will change accordingly. Therefore, *you need to continue to make the lack of scientific certainty a primary issue in the debate*" (Luntz, 2003: 137; quoted in Michaels, 2008: xi).[5]

The fossil fuel industry, however, faced the challenge of global warming during a different time period than the tobacco industry. Research on the health hazards of smoking became public in the U.S. in 1953, while the first reports on scientific consensus on climate change came in the early 1980s (e.g., National Research Council, 1983). This might explain why the fossil fuel industry has not only used research organizations, such as the American Petroleum Institute, to channel its funding of scientific research, but has also supported several think tanks, which have become central to the anti-environmentalist counter movement of the American right, especially after the 1992 UN Earth Summit in Rio de Janeiro (Austin, 2002; Jacques et al., 2008).

Following the tobacco strategy, some think tanks have made a huge effort to recruit renowned scientists that are climate change skeptics. As Oreskes and Conway (2010) have documented, William Nierenberg has been crucial to the campaign against global warming. Former physicist of the Manhattan Project and former director of the Scripps Institution of Oceanography, Nierenberg was appointed chair of the Carbon Dioxide Assessment Committee to undertake a comprehensive study of CO_2 and climate, which later became the 1983 NAS report. A team of climate scientists and, surprisingly, two economists, were put together to evaluate the state of the Earth's climate.

In the end, instead of producing a joint report based on consensus, the 1983 NAS report, *Changing Climate: Report of the Carbon Dioxide Assessment Committee*, contained two contradictory views regarding climate change: "five chapters detailing the likelihood of anthropogenic climate change written by natural scientists, and two chapters on emissions and climate impacts by economists — which presented very different impressions of the problem" (Oreskes and Conway, 2010: 177). The natural scientists concluded that global warming was a real problem and that preventive measures were needed. The economists, on the other hand, argued that the evidence was not conclusive, and that the government ought to fund more research before acting. Despite the contradictory views that appeared in the report, the summary sided with the minority's views. Surprisingly, the report became a central tool in certain governmental sectors to counteract environmental policy: "the Nierenberg report didn't go out with the morning trash. It was used by the White House to counter scientific work being done by the EPA" (Oreskes and Conway, 2010: 182). In fact, the report was used against almost every initiative seeking to control the emission of greenhouse gases.

The NAS report is just one of the many examples that Oreskes and Conway (2010) examine regarding global warming, where information was manipulated — it was hidden, distorted, presented with a deceptive emphasis, etc.— to create the idea that scientists had not yet achieved consensus about global warming. Moreover, the controversy was advocated by a small group of very distinguished scientists, which gave the impression that this was in fact a scientific controversy, with prominent scientists in both camps. In addition to Nierenberg, Robert Jastrow (astrophysicist, head of the Goddard

Institute for Space Studies), Frederick Seitz (previously mentioned as a key player for the tobacco companies), and Fred Singer (rocket scientist, first director of the Nation Weather Satellite Service) also participated as science collaborators.

Just as the tobacco industry, climate change skeptics have also made several attacks to scientific research that goes against their interests. For instance, Oreskes and Conway (2010: 190-197) document the dubious attempt to coerce a convalescent Roger Revelle into collaborating with Fred Singer to publish a paper against his own scientific views.

In this way, industrial interests have reshaped and rechanneled mechanisms of knowledge production in the global warming case, such as the emphasis on scientific uncertainty, the recruitment of renowned scientists, the support of friendly research, and the attack to unfriendly research and researchers.

Big Pharma

The pharmaceutical industry learned quickly from the tobacco mongers. Edward Nik-Khah (2014) tracks the beginning of the pharmaceutical industry's application of the tobacco strategy to the 1971 *Drugs* conference at the University of Chicago. Financed by some of the major pharmaceutical corporations (such as Novartis, Merck, and Pfizer), and organized by faculty members of the Chicago School of Economics (Milton Friedman, Richard Posner, and George Stigler among them), the *Drugs* conference was a collective attempt of Big Pharma to oppose the 1962 Kefauver-Harris Amendments to the Federal Food, Drug, and Cosmetic Act, which allowed the FDA to exercise media control, establish standards for clinical trials, and demand appropriate efficacy proofs for new and marketed drugs. After the conference, the Center for Policy Studies published the volume *Regulating New Drugs* (Landau, 1973), a collection of the main contributing papers. The book's contributors opposed the 1962 Amendments and drug regulation more generally, showing a strong commitment to the neoliberal ideology of the Chicago School of Economics (Nik-Khah, 2014: 494). The resulting volume of the conference was well publicized, receiving the support of the well-known neoliberal economist Milton Friedman in his *Newsweek* column, and of Alan Greenspan in the *Objectivist Newsletter* (Nik-Khah, 2014: 495).

Not long after the conference, the American Enterprise Institute (a major think tank with Friedman among its members) created the Center for Health Policy Research (CHPR).[6] The CHPR brought together many of the participants of the *Drugs* conference, who united their efforts to organize further conferences, publish books and research studies, and distribute easily understandable pamphlets. In a similar vein and only two years later, Louis Lasagna, who also attended the 1971 conference, established the Center for the Study of Drug Development (CSDD). As Nik-Khah explains, the CSDD instituted a "policy of secrecy" to keep hidden its close collaboration with the University of Rochester, and later with Tufts. The CSDD collected data from pharmaceutical companies, put together studies on new drugs, and helped with publications, following articles through the peer review process, all while granting full confidentiality to pharmaceutical companies (Nik-Khah, 2014: 502).

The pharmaceutical industry not only exploited the resources of PR and academic ties in its favor, but also went further than the tobacco industry in controlling scientific research through the peer review system. While the tobacco industry emphasized the publication of pamphlets and symposiums proceedings, the pharmaceutical industry has created a way to spread friendly research through peer-reviewed journals. In "Ghosts in the Machine" (2009), Sergio Sismondo carefully describes the central aspects of "publication planning," which is nothing but the organized effort of pharmaceutical companies to obtain favorable scientific research through exercising a close control over every phase of the research process leading to publication — i.e., data collection from contract research organizations (CROs), ghostwriters, signatures from "independent" medical researchers, peer review process, and finally the creation of an echo chamber effect. Sismondo summarizes the process as follows:

> Most sponsored clinical trial research is handled by contract research organizations (CROs), the data they produce is typically analyzed by pharmaceutical company statisticians, papers are written by medical writers, and the whole process

> is guided and shepherded through to publication by planners and planning teams… To gain the most commercial value from research, the papers publicizing it are written under the names of independent medical researchers… (Sismondo, 2009: 172)

This "ghost management" of research and the publication process has become central for the pharmaceutical industry. According to Sismondo (2009: 172), up to 40% of reports on clinical trials of new drugs are the result of some publication planning firm, making this practice a well-structured and organized form of commercially driven medical research. In this case, the pharmaceutical industry controls not only the lines of research pursued, but it actually intervenes, first in the research process, using its own statisticians to find favorable ways to present research results, and later in the publication process, through ghost-writing practices. The pharmaceutical industry is changing traditional methodological standards in medical drug trials with respect to experiment design, data gathering and interpretation, and results publication, which leads to questioning the implications of these changes for the production of scientific knowledge.

Publication planning however did not appear overnight. Big Pharma learned the lesson from the tobacco industry on how to create an "echo chamber effect" so that a single finding favorable to the industry could resonate as much and as loud as possible. But it also learned from its own experience to strengthen its practices of ignorance construction. Take for example the Vioxx scandal. Initially targeted for the treatment of arthritis, Vioxx had the advantage over other painkillers of not causing gastrointestinal complications. In 1999, Vioxx manufacturer Merck decided to conduct a major clinical trial in an attempt to prove Vioxx's superiority to Aleve. The findings were rather unfortunate for Merck: People taking Vioxx had four times the risk of suffering a heart attack than those taking Aleve (McGoey, 2009: 156; Michaels, 2008: 146). Refusing to accept these results, Merck's scientists decided to go with a much more favorable interpretation of the data. Instead of claiming that Vioxx increased heart failure, they favored the idea that Aleve helped reduce such risk. A rather unjustified move, from a scientific point of view, given that Aleve was not known to reduce heart failure, and Merck's interpretation made it much more effective than aspirin in this respect. In *Doubt is their Product* (2008), David Michaels explores the Vioxx controversy, and concludes:

> I found it harder and harder to believe that this was merely a case of well-meaning scientists unintentionally misinterpreting the data... No drug has ever been shown to reduce heart attack risk by 80 percent. If the scientists honestly believed their claim, they should have lobbied the government to pour Aleve directly into the nation's water supply. (Michaels, 2008: 148)

Merck went on to defend this controversial interpretation of the data, displaying an array of ignorance constructive tactics such as threatening scientists who opposed it, and using constant PR to undermine the risks of heart failure from Vioxx in the media (Michaels, 2008: 147).

Linsey McGoey (2009) portrays the Vioxx scandal also as a case of unjustified fostering of uncertainty on Merck's part (another of the central components of the tobacco strategy). McGoey (2008: 158) highlights the manipulation of the concept of uncertainty in the industry's advantage: "By stressing the *uncertainty* of the facts surrounding the safety of drugs such as Vioxx, regulatory hesitations in removing the drug from the market seem prudent rather than negligent." And she concludes by stressing that this misuse of uncertainty further protects the concerned players, by making them many times immune to liability.

As Merck's Vioxx case illustrates, major pharmaceutical companies have followed closely the tobacco strategy. They have not only learned, but improved many of the tactics, pervading the peer review system in unprecedented ways (through 'ghost management' of publications), and creating new and better strategies for manipulating scientific data through a careful design of clinical trials to favor their drugs (Michaels, 2008: 149). But, in general, the pharmaceutical industry has followed the same strategies as the tobacco industry. It has fostered uncertainty regarding scientific research that is unfriendly to the industry (McGoey, 2009), it has created research organizations to oppose

scientific consensus (such as the CHPR and the CSDD), using major think tanks (such as the American Enterprise Institute) to channel their funds, it has recruited distinguished scientists to use their scientific credentials in their favor, and it has used the media, the funding of conferences, and the publication of favorable research as a PR strategy to promote their views with the public and within the scientific community.[7]

The 2008 Economic Crisis

In August 2012, the Justice Department closed the criminal investigation of Goldman Sachs regarding its role in the 2008 financial crisis. While the U.S. government failed to convict anyone for federal fraud, the public has remained deeply unsatisfied with the outcome. A 2012 editorial in the *New York Times* put it bluntly:

> The financial crisis, fomented over years by big banks and presided over by executives, involved reckless lending, heedless securitizations, exorbitant paydays and illusory profits, all of which led to government bailouts and economic calamity. Is it plausible that none of that broke the law and that none of the people in positions of power and authority knew what was going on? (*NYT*, 2012)

Even more striking has been the realization that economists were caught up by surprise, and seemed totally unprepared to explain what happened. As Paul Krugman (2009) explains, macroeconomists were divided "between those who insisted that free-market economies never go astray and those who believed that economies may stray now and then but that any major deviations from the path of prosperity could and would be corrected by the all-powerful Fed." And financial economists completely ruled out the possibility of a crisis by definition: they "came to believe that markets were inherently stable — indeed, that stocks and other assets were always priced just right" (Krugman, 2009).

In the aftermath of the crisis, part of the problem has been the lack of consensus within the economics profession to explain what really happened, and why it happened. Phil Mirowski (2013) presents an agnotological analysis of the 2008 financial crisis, uncovering some of the reasons there has not been a serious effort from the economics profession to explain the crisis in theoretical terms, which in turn has made any attempt at prosecution pointless. In particular, he argues that different strategies of ignorance production have been implemented in the aftermath of the crisis to confuse the American public and delay any serious interpretation of the event:

> The most important part of the history of the crisis that has been neglected … is that there have surfaced in the crisis some relatively systematic attempts to pump doubt and confusion into public discourse; in other words, some 'explanations' of manifestations of the crisis and its aftermath have been launched as trial balloons *not* expressly for purposes of further test and elaboration by sanctioned professional economists, but rather as calculated interventions in public discourse in order to buy time and frustrate any shared impressions of a few sharply delineated positions on a contentious issue. (Mirowski, 2013: 226)

In this sense, the financial collapse should not be interpreted as a mere lack of agreement among economists (i.e., as a legitimate scientific debate), but as a new case of agnogenesis, in which practices of ignorance construction have impeded the understanding of this economic phenomenon. I will focus on one of Mirowski's examples, i.e., the spreading of a mistaken explanation for the crisis, and the subsequent failure of the Federal Crisis Inquiry Commission.

The most popular right-wing account of the crisis reduces the financial collapse to a housing bubble, which grew uncontrolled thanks to an extension of loans to people who couldn't financially support them. This type of low-quality (subprime) loans became possible, so the story goes, after the Democrats passed the Community Reinvestment Act (CRA) in 1977. On the other end, the Government Sponsored Enterprises (GSEs) Fannie Mae (Federal National Mortgage Association) and Freddie Mac (Federal Home Loan Mortgage Corporation) facilitated the concession of mortgages to cover such loans. Thus, at the end, "the government had polluted the mortgage market, first causing the housing bubble, and then the subsequent collapse. It was all the fault of the government. Full stop" (Mirowski, 2013: 313).

However, as some economists and reporters have pointed out, blaming the CRA together with Fannie and Freddie for the collapse is mistaken (Krugman, 2008; Goldstein and Hall, 2010; Min, 2011; Nocera, 2011). First, the CRA did not regulate the private firms in the subprime market (where the crisis started), and even the number of loans that fell under the CRA was small among the firms it did regulate; not to mention that the timing is somewhat off, after all the CRA was passed in 1977 (Mirowski, 2013: 316). Second, the GSEs had been actually losing shares in the subprime market since 2002, given that this market was primarily a privately run machine, and their own government guidelines limited the GSEs mobility in it (Mirowski, 2013: 317). Thus, neither the CRA nor the GSEs provided enough control of the subprime market to account for market failure. This is consistent with the Federal Reserve Board data, according to which "more than 84 percent of the subprime mortgages in 2006 were issued by private lending institutions" (Goldstein and Hall, 2010).

Why then did Fannie and Freddie become common targets in the aftermath of the financial crisis? Mirowski traces the way in which this hypothesis became the central cover up story for the political right and in particular for what he calls the "neoliberal thought collective." Here again the repurposing of traditional practices of knowledge production became central: Recruitment of well-known economists, funding of research that supports the right-wing cause, the publication of a concrete story that favors private interests, and finally the creation of an echo chamber effect in popular media.

By creating an "echo chamber effect," the neoliberal thought collective was able to expand the Fannie and Freddie story from a few experts to the public at large. Mirowski documents how the hypothesis was first tried in August 2008 by Charles Calomiris (from the Cato Institute) and Peter Wallison (from the American Enterprise Institute) in the *Wall Street Journal* (Calomiris and Wallison, 2008). The two think tanks played a central role in spreading the hypothesis around, supporting it with numerical data, putting it in the blogosphere, and selling it through academic publications (e.g., in Raghuram Rajan's *Fault Lines* (2010)). In particular, the AEI financed Edward Pinto's research on the financial crisis (Pinto, 2010). Pinto's study provided the numerical data to back up the idea that the CRA together with the GSEs were to blame for the crisis. For instance, Pinto (2010: 29) claimed that in 2008 over 70% of weak or high risk loans were owned or supported by GSEs or banks under the CTA.

The real power of this strategy was not seen until the Financial Crisis Inquiry Commission (FCIC) was set in place, early 2010. Created initially with the aim to "provide trenchant research and a communal teaching experience concerning the causes of the crisis" (Mirowski, 2013: 319), the FCIC failed to deliver a joint report. The official report was endorsed by a majority of six out of ten bi-partisan members, while the four remaining conservative members decided to express their disagreement in two dissenting appendices added to the official report: "Dissenting views by Keith Hennessey, Douglas Holtz-Eakin, and Bill Thomas" and "Dissenting views by Peter J. Wallison." In the official conclusion of the report, the FCIC explicitly discarded that Fannie and Freddie as primary causes of the crisis (FCIC, 2011: xxvi). Meanwhile in his dissenting remarks, Peter Wallison (fellow of the AEI) endorsed the Fannie and Freddie story, blaming the U.S. government's housing policy for the crisis (FCIC, 2011: 444), and quoted Pinto's (2010) data as evidence for the lack of objectivity of the official report: "the Commission majority's report ignores hypotheses about the causes of the financial crisis that any objective investigation would have considered, while focusing solely on theories that have political currency but far less plausibility" (FCIC, 2011: 476). In the end the report was inconsistent. The majority's conclusions stated that GSEs were not to blame for the crisis, while the dissenters claimed the opposite.[8]

If Mirowski is correct, the neoliberal thought collective, using Wallison as key expert on the case, was able to twist a national effort (worth six million dollars) to investigate the origins of the financial collapse, spreading confusion and creating the idea that disagreement among experts in the commission probably entailed a true difficulty in determining the real causes of the crisis and who was responsible. Which takes us back to the beginning of this section on the

economic crisis. The government couldn't find out whom to blame, it is now very unlikely that anyone will be prosecuted for the collapse, U.S. taxpayers' money was just wasted, and we still lack reliable knowledge about what happened. And, finally, notice again the similarities with the tobacco industry's reshaping of distinct practices traditionally involved in the process of knowledge production; in this case, support from key "experts," downplaying real scientific analysis of the crisis, and creating an "echo chamber effect" through publications.

Conclusion

The aim of his paper was to show that agnotology is a useful tool for better understanding the impact of the industrial interests that pervade science today. In particular, agnotological studies have uncovered the ways in which different mechanisms and practices, traditionally tied to knowledge production, have been reshaped and rechanneled to favor industry-friendly outcomes, turning them into mechanisms of ignorance production. Through an examination of the tobacco case, one can identify at least five of these mechanisms, where the epistemic purposes have been compromised: (1) The emphasis on scientific uncertainty, (2) the support of friendly research, (3) the recruitment of distinguished scientists, (4) the creation of an echo chamber effect, and (5) the attack to unfavorable scientific research. Moreover, the same mechanisms have been used to favor industrial interests, undermining at the same time the process of knowledge production in a variety of cases, such as climate science, pharmaceutical research, and economics.

Endorsing the agnotological perspective, however, also entails further challenges. Some have objected to the project, claiming that agnotology's treatment of ignorance is too ambiguous or too vague to constitute a legitimate area of inquiry. Indeed, agnotology aims to be as broad as epistemology, i.e., to study ignorance with the breadth and depth with which epistemology studies knowledge (Proctor, 2008: 1). Thus, agnotology would have in principle as many branches as epistemology: Formal, Bayesian, local, evolutionary, moral, feminist, naturalized, social, and so on. But many of these different possible approaches to the study of ignorance have not been explored yet, and accordingly they have not gained disciplinary recognition. Until then, agnotology will probably remain a vague field of study.

If one looks closer at particular cases though, one narrows the breadth of the agnotological study and works with a narrower understanding of ignorance that is less ambiguous. In the paper, for instance, I examine cases of ignorance as strategic ploy or active construction (Proctor, 2008: 3), in the context of commercially driven scientific research today, where particular mechanisms of ignorance production can be identified. In this case, ignorance is the product of the reshaping and rechanneling of traditional practices of knowledge production, whose epistemic purposes are transformed in the process of fulfilling industrial interests. It is in this shift of purposes from epistemic to industrial that the product of the practice changes as well, in this case, from knowledge to ignorance.

I do not wish to claim that all commercially driven research uses practices of ignorance production in the way just described or that government funded research is free from incurring in the same actions. My claim is the smaller one that commercially driven research has encouraged these types of practices, as industrial interests start driving scientific research. Hence, the connection I want to highlight here is a contingent one between industrially driven research today and the reshaping of what have been practices of knowledge production.

Another important challenge to agnotology is related to the normative character of the concept of ignorance. Just like knowledge, ignorance is normally used in normative terms. While knowledge is traditionally understood as positive, as justified true belief, perhaps with a specific type of communal approval, ignorance is understood as negative, as a state of non-knowledge. But just like the normative character of knowledge has been contested (Barnes and Bloor, 1982), so can the normative character of ignorance. One could argue that what I have called in this paper practices of ignorance construction are nothing but historical rearrangements of practices of knowledge production, and that my exposing

of these practices as detrimental to science just follows a lack of historical awareness of the ways in which the human understanding of science and knowledge changes through time.

First, I grant that agnotology presupposes a fundamental distinction between knowledge and ignorance, a distinction that is normative in character, and that still needs important philosophical input (see Fernández Pinto 2015, for some advances in this respect). Second, and after acknowledging that agnotology has this limitation, let me explain the way in which I think the knowledge/ignorance distinction is playing a role in this paper. I have tried to show that the five practices identified here lose their epistemic purpose in favor of some other commercial or industrial purpose, and this is precisely why they stop contributing to the production of knowledge and instead start contributing to the production of ignorance. The search for knowledge is replaced for a search of non-knowledge in two ways: first, because the goal of the practice is no longer knowledge but some commercial interest, and second because the practice has been traditionally associated with the production of knowledge, so that it appears to have an epistemic goal when this is no longer the case, and in this deception it produces further ignorance.

Historians of science and other science scholars have uncovered cases of agnogenesis that seem detrimental to scientific knowledge, i.e., cases which not only impede the proper communication of scientific results, but also affect the way scientific research is done (adjusting the peer review system, shaping lines of research at universities, and fostering a research structure that is industry friendly). As the cases in this paper show, practices of ignorance construction have succeeded in challenging scientific consensus, obscuring scientific knowledge, and fostering confusion among policy makers and the general public. Understanding these practices and the epistemological and social consequences that they entail is certainly a pressing task for science and technology studies today, and agnotology, despite some of its initial limitations, has opened the door for STS scholars to make important contributions in this respect.

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Notes

1 The neologism comes from the Greek *agnosia*, meaning "a state of ignorance or not knowing," and it was originally coined by Proctor with the help of linguist Iain Boal in the spring of 1992 (Proctor, 2008: 27-28). Later on, Proctor and Schiebinger edited the programmatic volume *Agnotology: The Making and Unmaking of Ignorance* (2008), where they introduced the new terrain of agnotology to the academic audience. The volume emerged from two workshops, one held at Pennsylvania State University in April 2003 and another one held at Stanford University in October 2005. For a list of the participants to both workshops, see: http://www.bshs.org.uk/agnatology-the-cultural-production-of-ignorance (accessed 04/08/16) and http://www.stanford.edu/dept/HPS/AgnotologyConference.html (accessed 04/08/16).

2 The U.S. tobacco industry's campaign against scientific research linking tobacco smoking to lung cancer has been documented by a number of scholars and science journalists (See, for example, Glantz, 1996; Hilts, 1996; Kluger, 1996; Parascandola, 2005). For a comparison with the UK tobacco industry case, see Berridge (2006).

3 MacGarity and Wagner (2008) provide a much longer list of agnotological strategies, highlighting the need for a deep change in the legal system regarding science policy issues. Although their focus is not on tobacco, their taxonomy corresponds in many cases to the tobacco strategy. My aim here is to synthesize further the core mechanisms of the tobacco strategy.

4 You can find it at www.exxonsecrets.org (accessed 04/08/16).

5 Frank Luntz worked for the Republican Party during the Bush administration, and is famous for suggesting the use of "climate change" instead of "global warming" in public policy communications to downplay the severity of the issue.

6 Notice the similarity to the tobacco industry's creation of the TIRC.

7 Big Pharma has also innovated in its use of science as PR. Going beyond the tobacco strategy, and taking advantage of their success in the current regime of privatized science, pharmaceutical companies extend their control of the peer review system, not only through ghostwriting and publication-planning strategies (Sismondo, 2009), but also through massive funding of medical journals through advertisement, purchase of reprints, and publishing supplements. But it doesn't stop there. Big Pharma's use of PR has achieved new levels with their use of "seeding trials." These are scientifically meaningless trials conducted with the unique goal of increasing drug prescriptions (Smith, 2003). Doctors get paid important sums of money just to get patients into the "trial," without leading to any scientific advancement.

8 Notice the similarity with Nierenberg's effort to discredit the 1983 NAS report. The government's effort to investigate the underlying causes of the crisis was truncated by partisan interests that further contributed to the confusion regarding who was responsible for the collapse.

Donna Haraway (2016) Staying with the Trouble: Making Kin in the Chthulucene. Durham: Duke University Press. 312 pages. ISBN: 978-0822362241

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As a theorist, one of Donna Haraway's central concerns has been to break out from some of the most deadly habits of Euro-American thinking. Her famous "Cyborg Manifesto" was explicitly written as an "ironic political myth" (1991: 149) designed to help us think outside of "the maze of dualisms in which we have explained our bodies and our tools to ourselves" (1991: 181). When we make categorical distinctions between nature and culture, animal and human, organic and artificial, she argues, we fail to grasp how worlds hang together and proliferate specific forms of living and dying. In her new monograph, *Staying with the Trouble: Making Kin in the Chthulucene* Haraway stages another break-out. Here she takes on a particular kind of dualistic thinking specific to Western environmental thought in the 21st century. On the one hand, we're witnessing unprecedented environmental optimism born of a misplaced trust in technology, the naïve belief in efficacy of "spreading awareness" without really changing anything, and flat-out denial in the face of climate change, species loss, and planet-wide environmental violence experienced predominantly, but by no means only, by the world's poorest people (Nixon, 2011). On the flip side, there is a pervasive eco-apocalyptic cynicism, a feeling that "the game is over, it's too late, there's no sense in trying to make anything any better" (Haraway, 2016: 3). While these positions at first seem opposed, their effects are the same: inaction that does very little except protect the status quo of ongoing environmental violence in advanced capitalism.[1] Haraway argues that if we want anything to change, we can't afford to dither, throw our hands up, or opt out; we have to enter the fray, "stay with the trouble," and get our hands dirty (even learn to play in the mud!). As feminist science studies scholars like Alexis Shotwell (2016) and Max Liboiron (2016) point out, it's no longer possible to have "clean hands" in a "permanently polluted world" (Liboiron, 2016: 104); purity politics are a no-go. Instead, it is about collectively learning the arts of living on a damaged planet (Tsing, 2015: 292); pushing against complacency, without losing sight of complexities, contradictions, and complicities that come with living in a time characterized by the uneven distribution of wealth and environmental violence.

As in the "Cyborg Manifesto," Haraway's strategy for resisting dualistic environmental thinking is through feminist storytelling. In *Staying with the Trouble* Haraway spins stories that are mythic, recursive, and looping, stories that repeat central motifs and then spin off on different trajectories. These stories are told in productive tension with recent work gathered around the charismatic concept of 'the Anthropocene.' On the one hand Haraway is sympathetic to the term Anthropocene, recognizing the need for a word that captures the sheer scale of environmental change, such that "any geologist of the future will find the

synthetic chemistry of DuPont in the composition of the rocks" (Haraway 2015, 259). However, she argues that stories about the Anthropocene often flatten out the politics; when humans-in-general are seen as the agent of environmental change, we don't get at the crucial question of who benefits and who suffers from the environmental practices of transnational corporate capitalism (Haraway, 2016: 49). Haraway offers the 'Capitalocene' as a term that might better account for the uneven economic power and privilege at work in the time Kim Fortun (2012) calls 'late industrialism.' But although it offers a more precise diagnosis of the problem, neither the Capitalocene nor the Anthropocene offer a way forward. *Staying with the Trouble*, then, is the story of something else; it is the story of the Chthulucene. The Chthulucene is the name Haraway uses to gather stories, figures, and practices that might help us to collectively build more livable worlds: "Unlike either the Anthropocene or the Capitalocene, the Chthulucene is made up of ongoing multispecies stories and practices of becoming-with in times that remain at stake, in precarious times, in which the world is not finished and the sky has not fallen — yet" (Haraway, 2016: 55).

To conjure the myth of the Chthulucene, Haraway brings motley multispecies stories from different times and places into generative relation with one another as the chapters unfold. In this way, her storytelling practice performs her argument—namely that "a livable world must be composed [collectively] bit by bit, or not at all" (2016: 40). She collects thing up in her net-bag (2016: 118); she thinks sympoietically,[2] cites exuberantly. Her sources include not only theoretical interlocutors like Isabelle Stengers, Vinciane Despret, Marilyn Strathern, and Anna Tsing, but artists, scientists, her former students, videogames, children's books, web comics, and the work of dozens of projects all over the world that promote multi-species flourishing: The Crochet Coral Reef, Navajo Churro Sheep Project, numerous projects with pigeons including Pigeon Loft in Melbourne where pigeons receive food and shelter, but their eggs are replaced with artificial eggs to reduce their numbers. She writes science fiction about queer interspecies kinship as a response to environmental damage (Chapter 8), revels in scientific facts about symbiogenesis (2016: 59-68), and spectacularly fabulates about "the chthonic ones," creatures with "tentacles, feelers, digits, cords, whiptales, spider legs, and very unruly hair" that make up the figural substrate of her Chthulucene (2016: 2). "SF worlding" is the name of Haraway's game—telling speculative stories to transform our sense of what is possible. And, as I have been emphasizing, the organization of the book reflects her ontological and aesthetic sensibility—there are no foundations or bottom lines, "it's turtles all the way down" (Haraway, 2008: 32). This is a tentacular, relational kind of storytelling; a generous act of theorizing. Reading *Staying with the Trouble*, I was reminded of J.G. Ballard's advice to readers of his 1970 novel *The Atrocity Exhibition*: "Rather than start at the beginning of each chapter, as in a conventional novel, simply turn the pages until a paragraph catches your eye. If the ideas or images seem interesting, scan the nearby paragraphs for anything that resonates in an intriguing way" (Ballard, 2001: vi).

So it's a shaggy book, to be sure. But its shagginess is clearly by design. Haraway has chosen this style of storytelling to cultivate in her readers the capacity for response. At the end of her chapter about the complex histories of the estrogen medications DES and Premarin®, which begins with feeding her dog pills for urinary leakage and opens onto stories of DES daughters, German zebras in the 1930, and Pfizer contracted horse ranches in Canada, Haraway writes:

> Why tell stories like this, when there are only more and more openings and no bottom lines? Because there are quite definitive response-abilies that are strengthened in such stories. (Haraway, 2016: 115)

Haraway's concept of response-ability (see also Haraway, 2008) is not the liberal humanist obligation to be responsible for one's own choices; rather it is about learning to participate in a collective "praxis of care and response… in ongoing multispecies worlding on a wounded Terra" (2016: 105). The chapters in *Staying with the Trouble* might best be read as *fables of reponse-ability* (Kenney, 2013), stories that activate our capacities to attend to and therefore respond within the messy worlds we inhabit every day. They are fables because they

are didactic; they act on our sensoria; involve us in alternate "economies of attention" (Daston, 2004); initiate us into unfamiliar "arts of noticing" (Tsing, 2016); teach us how to respond and make openings for different types of response. By narrating surprising relations across disparate geographies, temporalities and materialities, Haraway sensitizes us to the ethical and political obligations that these relations demand. If our dogs take DES for urinary leakage, what should we know about the histories of agriculture and big pharma? Haraway argues that we all need to become more curious about the relations that constitute our ways of living, if we want to learn to make worlds that are less deadly for human and non-human others. In her estimation this is "probably still possible. Barely still possible. Still possible *if* we render each other capable of worlding and reworlding for flourishing" (2016: 96). Storytelling is one practice (among many others!) that can render us capable of responding better "within and as part of the world" (Barad, 2007: 37).

What can we take from such a tentacular, generous, and sympoietic book? Often we look to our best theorists to provide us explanations, terminology, or analytic frameworks that we can apply in our own Science & Technology Studies (STS) projects. I'd like to suggest that this isn't the only way to approach *Staying with the Trouble*. It's not that Haraway doesn't offer a compelling theoretical framework for understanding capitalism, environmental violence, and what it might take to enact better worlds; she does. However, it also offers something more important: With her exuberate and omnivorous approach to feminist storytelling, Haraway gives us permission to follow our own curiosities and experiment with our own fables of response-ability. If we believe storytelling is one of the practices that can render us capable of responding to the ravages of the Capitalocene, we will need more stories, not fewer. And not all of these stories will have the same aesthetic and political strategies as Haraway's. Some will be empirical, others imaginative; some haunting and poetic, others no-nonsense practical; some energetic, full of brilliant paradigm-shifting neologisms, others plain spoken and heartfelt. Different kinds of stories engender different ways of attending, responding, and relating. In *Staying with the Trouble*, Haraway offers important guidance to us, the storytellers of the Chthulucene (i.e. STS scholars); in our own situated projects, we need to ask what it would take to avoid thinking traps of environmental optimism/pessimism, stay with the trouble and imagine ourselves as participants in collective world-making. While the Anthropocene has been useful for gathering the arts, humanities, and social sciences around environmental questions, Donna Haraway's Chthulucene asks us not only diagnose problems but to embrace our roles as technoscientific fabulists and learn to tell stories that strengthen ecological response-ability in a world characterized by ongoing environmental irresponsibility that is both appallingly murderous and spectacularly profitable.

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Notes

1 After science fiction writer Kim Stanley Robinson, Haraway calls our current era of environmental inaction, "The Great Dithering" (144-145).

2 Sympoiesis is Greek for "making together." One of Haraway's central arguments is that everything, including life, is sympoietic. Her citation style reflects an ethical and theoretical commitment to scholarship as something that emerges from ongoing, collective exchange and not sui generis from the minds of great thinkers.

Jan-Peter Voß and Richard Freeman (eds.) (2016) Knowing Governance: The Epistemic Construction of Political Order. Basingstoke: Palgrave Macmillan. 300 pages. ISBN:978-1-137-51449-3

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Knowledge matters to governance. What counts as authoritative, legitimate and truthful knowledge matters to how political orders and collectivities are oriented and assembled. While this has been acknowledged for quite some time, the actual practices, instruments and actors involved in the production and stabilization of certain ways of knowing governance has received relatively less attention. *Knowing Governance: The Epistemic Construction of Political Order* provides a forceful argument for why this current neglect should be rectified. Split into five parts and twelve individual chapters, the anthology demonstrates why and how STS scholars can engage with the production of political knowledges. It shows why *knowing governance* might be central to understanding contemporary forms of governance as such.

"[W]e want to know governance through the ways it is made known to those who govern" (p. 2) Voß and Freeman, the editors of the volume, state in their introductory chapter. According to them, this requires a sensitivity towards the ways in which knowledge of governance is produced and constructed in the making. "Knowing governance", the productive conceptual device offered by the editors to cover such processes, means looking at the "formalizations and developments of ways of knowing how to do politics" (p. 2), and "the production and mobilization of ways of knowing about governance" (p. 3). Based firmly in the field of STS, this implies studying how particular collectivities, actors, models, metrics, standards, technologies, and instruments become enrolled in the production of knowledge about governance. It means paying attention to how certain representations of reality are made authoritative and legitimate through distributed decision-making processes. In this way, *knowing governance* serves as a methodological corrective to otherwise reified, instrumental, and neutralized accounts of political knowledge and knowledge of governance. These conceptual coordinates are developed in subsequent chapters through a number of empirical studies. Covering a wealth of different settings, from the European Union and global anti-piracy groups to the OECD and 'citizen panels', each chapter carefully unpacks the practices involved in knowing governance. Although the contributions are generally of a high quality, three chapters should be highlighted as particularly productive.

Chapter 4 by Christian Bueger draws on detailed fieldwork conducted as a part of a *Lessons Learned Project* intended to document knowledge on governance in the case of *the Contact Group on Piracy off the Coast of Somalia*; an international forum created to counter-act piracy off the coast of Somalia. Describing the Group as a 'laboratory' conducting 'experiments' in order to "compile facts and information" (p. 95), Bueger shows how it connects otherwise disparate sites into a relatively stable collective and is able to circulate its

selective representations of piracy through the use of official strategic documents, the *communiqué*. Turning to the Project that Bueger played a pivotal role in himself, he recounts the immensely difficult, and oftentimes overtly 'failed' attempts to produce knowledge *of* and *with* global governance. In bringing the porous boundaries between academic and political laboratories to the front, the chapter provides a rich narrative of the oftentimes messy realities of doing and knowing governance.

Chapter 5 by Holger Strassheim and Rebecca-Lea Korinek turns to a study of behavioral science within the UK. These authors set out to investigate an empirical puzzle: why and how behavioral science, epitomized by the Behavioral Insights Team (BIT) in the UK, "has gained such a remarkable reputation" (p. 110)? Focusing on the practices through which "behavioral approaches became authorized and legitimized" (p. 110), they provide a compelling account of how these methods have managed to shape contemporary ways of knowing governance. They showcase how BIT (and similar organizations) have "cultivated politico-epistemic authority by claiming the role of 'choice architects', mobilizing easily demonstrable forms of evidence and modelling the policy process after experimental designs" (p. 121). These ideas have simultaneously been imported within discourses about the 'Big Society' promoted by political parties in the UK. In this way, behavioral science has become a core component within "sociotechnical imaginaries about future state-citizen relationships" (p. 121). Due to this 'double reading', focusing both on the establishment of political authority *and* the translation of ideas across institutional boundaries, the chapter produces exciting insights into how knowledge is legitimized, circulated, and translated across different sites.

Finally, chapter 10 by Brice Laurent looks at 'boundary-making' through a study of the OECD and the report on 'Public Engagement in Nanotechnology' it produced in 2012. Tracing the complex practices involved in producing the report, Laurent shows how demarcations "between 'technical' and 'policy' expertise, between 'expertise' and 'normative judgement'" (p. 231) are central to its making. The group in charge of the report continuously had to distinguish, delineate and demarcate its area of concern in order to balance different national agendas and organizational divisions of labor. In this sense, boundaries result from continuous processes of 'purification', which "ensure that reports are written as they are supposed to, that questions are answered the right way, and that projects are presented appropriately during plenary meetings" (p. 231). The chapter demonstrates how a specific case may provide "empirical lenses into processes of international ordering" (p. 232). In this way, the study showcases practices of international governance often left in the dark.

Laurent's chapter is also productive because it hints at a topic that remains unaddressed throughout the book, namely how the production of certain knowledges also implies invisibilities, exclusions, deviances, and 'monsters'. He thus argues that "purification allows the OECD to render invisible both the politics of technical expertise and the potential redefinitions of governance practices with emerging technologies" (p. 232). Yet, the role of such invisibilities is all too often rendered *invisible* in the book. But how *does* the making of certain knowledges about governance render other knowledges invisible, silenced and illegitimate? How might certain ways of knowing governance bring peripheralizations and exclusions into being (and vice versa)? And, we could ask in a more political register, how are contemporary forms of ghettoization, incarceration, and ethno-racial stigmatization linked to certain ways of knowing governance? Can we simply see these as unforeseen 'side-effects' of governmental practices or are they at the core of current ways of knowing how to govern by those who govern? Addressing such questions would have allowed the link between *knowing* and *ordering* to emerge more clearly, highlighting the consequences and impact of certain ways of knowing governance. While the book does shed light on "governance through the ways it is made known to those who govern", it has comparatively less to say on what such knowledges *do*. What happens to the pirates off the coast of Somalia? How is behavioral science used to govern, discipline and recast citizens? Put more simply: why is *knowing governance* different from knowing other settings and practices?

Attending to such questions could have provided a bridge to the overlapping concerns of scholars within e.g. Foucauldian governmentality studies. While the editors do use a considerable amount of space in the introduction on connecting the present work with governmentality and interpretive policy studies, this cross-disciplinary dialogue fades out within large parts of the volume. However, a discussion of the book's theoretical, methodological and empirical implications for the wider study of governance could have amplified its impact significantly.

These caveats aside, *Knowing Governance* provides an in many ways impressive collection of work. It manages to intervene in current discussions in thoughtful ways, and offers useful conceptual devices for understanding the epistemic construction of knowledge. Despite a diversity of empirical sites, the book is held together by a firm thematic focus. This is a major strength of this work. While the book leaves certain questions open, it manages to provide a cogent argument for its overarching goal, namely to establish *knowing governance* as an exciting research agenda going forward.

Christopher G. Brinton and Mung Chiang (2017) The Power of Networks: Six principles that connect our lives. Princeton and Oxford: Princeton University Press. 328 pages. ISBN: 9780691170718

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What are the crucial engineering principles of networks? How is data traffic routed, how are search results ranked and how do video clips become viral? These are some of the fundamental questions Christopher G. Brinton and Mung Chiang address in their book "The Power of Networks". In particular, they explore the following six principles behind networking: (1) "sharing is hard", (2) "ranking is hard", (3) "crowds are wise", (4) "crowds are not so wise", (5) "divide and conquer" and (6) "end to end". Four of these principles are supplemented with compelling interview material from Internet pioneers such as Eric Schmidt, former CEO of Google, and Vinton Cerf, who co-invented the TCP-IP protocol. With this book the authors have set themselves the task to break down complex network knowledge into easy-to-understand principles, which are in the understanding of the authors "simple phrases that summarize a whole lot about the way networks are designed, built, and managed" (p. x). Throughout the whole book the authors use illustrative analogies and historical anecdotes such as the postal system, traffic jams or cocktail parties and therewith succeed to make the inner workings of networks available for a wide readership. In order to give a greater insight into the book, I will summarize and elaborate on two of the principles: (1) "ranking is hard" and (2) "crowds are not so wise".

The first principle I have chosen to describe further – "ranking is hard" – engages with the fundamental question of how to rank and categorize large amounts of information effectively. From an engineering perspective this is a challenging task, especially because the meaning of information is often ambiguous and today the number of available webpages has risen enormously; according to current statistics the number of unique website are around 60 trillion (p. 86). To illustrate the engineering task associated with how to categorize and rank information effectively, the authors start by outlining the original ideas of the two founders of Google, Sergey Brin and Larry Page. Unique to their approach of data management was the combination of the two factors "relevance score" and "importance score". While the "relevance score" is related to the content itself, the "importance score" determines the popularity of a website. According to the authors, it is especially the "importance score" that made Google so incredibly successful and hence, they explain how the "importance score" draws on graph theory in more detail. In addition to this explanation of the so-called graph theory, a short description of the random surfer concept is offered to illustrate how importance gets quantified. An interesting side note in this regard is, that ranking is not only employed for search queries, but build the fundament of Google Ads and other Internet auctions as well. Thus, for those readers interested in the economics behind such spaces, Brinton and Chiang explain methods such as "pay-

per-click" and "click-through rate" in very comprehensible mathematical terms.

The second principle I have chosen to treat in more detail is "crowds are not so wise". This principle opposes another principle "crowds are wise" also described in the book. Both principles engage the analysis of group behavior, shedding light on why crowds sometimes may take better decision and other times not. The principle "crowds are not so wise" examines group behavior associated with the phenomenon of videos going viral. Drawing on the example of "Gangnam style", the most watched video on YouTube at the time of the book publication, the authors analyze what factors made especially this video so incredibly famous. They start by defining "viral" as "1. a high total view count, 2. a rapid increase of sufficient duration, and 3. (sometimes) a short time before the rapid increase begins" (p. 173) and end by stating that there may be no "golden formula (…) to guarantee your video will become viral" (p. 173). In the meantime, the authors walk their reader through models and concepts that aim at predicting how information spreads online. It is a strength of this chapter, that the authors recognize, that the models they describe, are highly idealized. Hence, readers come to understand that while the models introduced in the book aim to predict, they cannot identify when a particular effect might occur. The chapter's intelligible introduction to sequential decision making and the mathematics behind information cascades, is thus very helpful as it makes no false promises. At the end of the chapter the authors draw a bridge to Hans Christian Andersen's tale of *The Emperor's New Clothes* (p. 185) in order to show how fragile information cascades are. Basically, it only needs very few individuals in a crowd to disturb positive network effects.

In sum, the book provides an in-depth overview of engineering models and principles employed in the digital realm. All explanations are easy-to-follow and make those principles accessible to a wide audience. What remains unclear is why exactly the mentioned six principles became ennobled as network maxims. This can give the impression that such principles are neutral without broader social implications. Hence, this book is especially recommendable for those interested in explanations of digital networks from an engineering perspective. It is not recommendable for those interested a critical analysis of networks. In this regard, however, the authors contributes with their easy-to-understand concepts towards a demystification of some underlying engineering principles of digital networks.

In this regard, a user study by Eslami et al. (2016) has shown that some users have "far-out" explanations of digital mechanisms such as algorithmic ranking. In their study, Eslami et al. examine Facebook's Newsfeed via the notion of "folk theories", which are "non-authoritative conceptions of the world that develop among non-professionals and circulate informally" (Eslami et al., 2016: 2). While some of such folk theories are unsurprising and expected, such as the "global popularity theory", which relates to the principle of "popularity ranking", other are rather surprising. One folk theory is for example that the personal similarities between friends "would affect the number of stories [a user] would see from those friends" (Eslami et al., 2016: 6). According to the explained principles in "The Power of Networks" this "magical" folk theory can be undermined from an engineering perspective. Even though this very folk theory might continue to guide everyday user behavior, it is not factual.

That user's folk theories are not always for the best shows another study by Eslami et al. (2015). Examining user's ideas of Facebook principles the authors conclude: "In the extreme case, it may be that whenever a software developer in Menlo Park adjusts a parameter, someone somewhere wrongly starts to believe themselves to be unloved" (Eslami et al., 2015: 8). To reiterate in this context Brinton and Chiang's work sheds light on the actual engineering mechanisms and so can help users to avoid such beliefs. It is clearly a good idea for undergraduates and interested citizens, especially those with a non-technical background, to engage in networks from an engineering perspective, so they can distinguish between their own folk theories and actual engineering principles.

References

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Sonic Acts Festival 'The Noise of Being', 23-26 February, Amsterdam, The Netherlands.

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Sonic Acts is a festival with a theme[1]. Since 1994 it has explored themes at the intersections of art, technology, music and science by means of an international conference, concerts, performances, exhibitions and screenings. This year's theme was The Noise of Being. The curators introduced the theme in turbulent terms: "Every day, artists, hackers, diseases, natural disasters, stock market crashes, media, commodities and fascist oligarchs administer us with a brutal portion of dissonance. Even our geological time is subject to dispute: the catastrophic Anthropocene epoch seems to have arrived, while the Crapularity, an era in which 90% of everything that surrounds us is deemed garbage, is just around the corner. But while confusion is paramount and insecurity rules, no one would dare to refer to this time as the heyday of noise. There is more at stake than noise: we know too much to conflate these tragedies to a buzz."

Over the course of four days the festival explored the stakes of our time between buzz and noise. While none of the speakers and artists directly spoke to the idea of noise, I will take the liberty to locate it, both in (only a few) topics of conversation and in the format of the festival. So, where was noise?

Noise was there as an effect of governmentality. Eyal Weizman discussed the complexities of the threshold of the Naqab desert in southern Israel. It was along this threshold of the desert/non-desert that Israeli state campaigns to uproot Palestinian Bedouins were conducted. Using aerial photographs, remote sensing data, state plans, court testimonies and nineteenth century travellers' accounts, Weizman showed the different temporalities of the threshold. It incorporated the temporality of the longue durée, with climate change altering the course of the threshold thereby prompting the Bedouins to move. Yet, the threshold also invited immediate response in border incidents, signalling in a split second who was in and who was out. Applying a mode of counter-forensics Weizman made us aware of the multiple dissonances that make up the signal.

Can we build shelters of signal and keep noise out? Artist Kate Cooper experimented with this in 'We Need Sanctuary', a video-installation that was part of the 'Noise of Being Art Exhibition'. In the work we see a computer-generated body that gracefully interacts with other coded objects. The body bleeds, however, more and more. As the body starts failing, so does its representation. The images do not look real – they are not real. The craft of the computer-generated image where reality is the representational objective, consists of the interaction between noise and signal. It is in this interaction that computer generated images are entities with power and intent. Their infrastructures are not freely available for us to use however we might wish, but instead entities that resist and demand fleshy engagement in order for noise to turn into signal.

Can we even distinguish between noise and signal in this time of fake news and the self-refer-

ential cocoons that algorithms build us? Designer and artist duo Metahaven showed 'The Sprawl', a film that deconstructs propaganda while at the same time throwing into confusion what is real, what is original, and what is intended. While the topic of the film is propaganda, the form of the film *does* the tension between information and disinformation as well. The film discusses *Russia Today*'s news strategies while it shows footage of president Ronald Reagan whom it describes as an actor in the caption. Peter Pomerantsev — author of the book *Nothing is Real and Everything is Possible: The Surreal Heart of the New Russia* — says in the film: "I used to think propaganda was about persuading people. Now it doesn't seem to be about that. It's just about disrupting the other side." 'The Sprawl' forcefully confronts the viewer with the difficult distinction between persuasion and disruption, and who decides about this difference.

Noortje Marres' talk raised the question of attuning to noise and signal. In the context of driverless cars, she discussed the participation of users in the testing of these cars without formal official approval for the testing regime. This situation undermines protocols of accountability in testing, and this indifference to the formal inclusion of the public puts democracy in question. Marres argues that democracy makes certain kinds of accountability and experimentation visible while remaining blind to others. It requires imagination, or experiments in translation, to explicate the complexities involved in driverless car testing and their consequences for relations of accountability.

Noise was not just what Sonic Acts was about. Noise was also how the festival was done. A mix of styles, jargons, and disciplines, it wasn't always easy to know of which "post-human predicaments" we were speaking, or what kind of "ontological catastrophes" we were invited to imagine. But while we must acknowledge that people did not speak the same language — in the many senses of language — it is equally important to recognize there was not much attempt at a conversation either. While the festival's curators introduced each panel carefully, they left little to no time for questions from the audience and collective exchange.

Revealing my commitment to the codes of academic language use, even if these are often not lived up to in academic settings either, I realized I was annoyed by the masses of people walking in and out of talks and screenings, just sampling and moving on. My hope was for some conversation after the talks to guide me through some of the more obscure talks and to make sense of jarring juxtapositions. I felt myself to be in a minority as people floated in an out of sessions, seeing a bit of film or art here, hearing part of a talk there, and most probably dancing off any dissonance during the club program, while I was home with my two-year-old.

It was after all a festival and one to celebrate noise perhaps in the way that Michel Serres speaks of it in *The Parasite* (1982). Without noise, Serres claims, there is no communication. Without noise, there would hardly be a relation, as sender and receiver would be fully similar. Noise then is what makes translations necessary, and transformation possible. To understand how these mediations happen requires being there where the noise is, risking oneself between one and the other - *being* the relation. This reminds me of Helen Verran's presentation at the festival. She juxtaposed two Australian stories. One was an advertisement video made by Meat and Livestock Australia. The setting is a barbecue at an Australian beach hosted by Indigenous people. One by one the European and Asia-Pacific settlers arrive in their boats. The ad was released on Australia day and celebrates diversity by asking, aren't we all boat people? And later on, without mentioning Australia day which marks the arrival of the first British ships, the answer to the question "What is the occasion?" is "Do we need one?" The elimination of noise makes for a sameness that allows for simple capitalization and the selling of lamb chops. The ad was contrasted with a story that was told to an anthropologist in Australia's Arnhem Land in the 1950s. The story teller was Marwalan, a Yolngu Aboriginal Australian leader, who used the words and the logic of his Yolngu language. This language does not designate spatio-temporal entities, but rather designates relations. Being the relation, speaking the relation, is like being the noise. It requires assiduous engagement and investment to make a signal.

Noise troubles the idea of sameness and order that often animate our expectations of and aspirations for the social. Noise brings to mind our partially connected engagements, projects, and hopes. While noise produces trouble and disconnection, this is not all bad. As noise always demands engagement to produce signal, these may enable new communicative practices and relations.

References

Serres M (1982) *The Parasite.* Baltimore: Johns Hopkins University Press.

Notes

1 http://www.sonicacts.com/2017/festival

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